THE AMERICAN ECONOMIC REVIEW

VOLUME LVI | March 1966 | NUMBER 1

The Journal of
THE AMERICAN ECONOMIC ASSOCIATION

THE AMERICAN ECONOMIC ASSOCIATION

[illegible]

THE AMERICAN ECONOMIC REVIEW

VOLUME LVI



THE AMERICAN ECONOMIC ASSOCIATION

CONTENTS

RTICLES:

MMUNICATIONS:

EVIEWS OF BOOKS

PAPERS AND PROCEEDINGS

(In May 1966 Number)

PUBLIC REGULATION: THE IMPACT OF CHANGING TECHNOLOGY

THE SOVIET ECONOMY, KNOWLEDGE, INFORMATION AND INNOVATION IN

CONTRIBUTORS:

Leading articles are marked (a); communications (c); papers read at the annual meeting published in the Papers and Proceedings, separately paged, are marked (p); and all others are book reviews.

THE AMERICAN ECONOMIC REVIEW

JOHN G. GURLEY, Managing Editor
JANE LADD, Assistant

Volume LVI | MARCH 1966 | Number 1

ARTICLES

COMMUNICATIONS

Manuscripts and editorial correspondence relating to the regular quarterly issue of this REVIEW should be addressed to John G. Gurley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California 94305. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.
No responsibility for the views expressed by authors in this REVIEW is assumed by the editors or the publishers, The American Economic Association.

BOOK REVIEWS

OTHER DEPARTMENTS

THE AMERICAN ECONOMIC REVIEW

JOHN G. GURLEY, Managing Editor
DORIS MERRIAM, Assistant

Volume LVI | DECEMBER 1966 | Number 5

ARTICLES

COMMUNICATIONS

Manuscripts and editorial correspondence relating to the regular quarterly issue of this REVIEW should be addressed to John G. Gurley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California 94305. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.

No responsibility for the views expressed by authors in this REVIEW is assumed by the editors or the publishers, The American Economic Association.

OOK REVIEWS

OTHER DEPARTMENTS

Number 67 of a series of photographs of past presidents of the Association.



Joseph J. Spengler

The American Economic Review

Volume LVI MARCH 1966 Number 1

THE ECONOMIST AND THE POPULATION QUESTION*

By JOSEPH J. SPENGLER

No quantity can increase by compound interest and remain within finite limits.

—LAWRENCE DENNIS

Man's industrial activities are merely a highly specialized and greatly developed form of the general biological struggle for existence.

A. J. LOTKA

In Megalopolis the sentiment of friendship wastes away.

—R. M. WEAVER

My concern is to put the population question in temporal and statistical perspective. I pass over classical concern lest numbers become excessive and Asian and Roman concern lest they not become extensive enough. I pass over the upward shift of the military and nonmilitary demand for manpower in the Age of Mercantilism and the emergence of a populationist philosophy. I pass over those who after 1750 rejected populationism. I begin with Malthus, at the time of whose birth the world's population numbered just over 750 million, perhaps treble what it was at the start of the Christian era, though growing faster than ever before, now that pestilence had been partially tamed and the supply of the means of existence had become more elastic and more stable.

I. *The Malthusian Age: 1800-1930*

Malthus may well serve as eponym to the period 1800-1930, since it was he who conceptualized and put into *paradigm* the demographic issues which dominated it. At its start Malthus was warning that an unfettered stork could visit poverty upon most and prevent realization of the aspirations of the Age of Enlightment. At the period's close the world's rate of population growth was double what it had been in 1800,

* Presidential address delivered at the Seventy-eighth Annual Meeting of the American Economic Association, New York, December 29, 1965. The author is indebted for suggestions to his colleagues, Ralph Braibanti, F. T. DeVyver, C. B. Hoover, and William Yohe.

and its annual increment roughly 22 instead of 4 millions. Meanwhile the world's population had risen from less than a billion to just over 2 billion; that of Europe and the Soviet Union to over a half billion, nearly triple the 1800 figure. Of Europe's population nearly three persons in ten lived in 248 cities of 100,000 and over, compared with only about one in thirty in 18 such cities in 1800. Meanwhile the world's cities of 100,000 and over had risen in number eighteenfold to nearly 700 and their relative population eightfold to 11 per cent of the world total. It is not entirely surprising, therefore, that Alfred Marshall could write in 1920 (*Principles,* preface):

> In the present age, the opening out of new countries aided by low transport charges on land and sea, has almost suspended the tendency to Diminishing Return . . . and yet, if the growth of population should continue for very long even at a quarter of its present rate, the aggregate rental value of land for all uses . . . may again exceed the aggregate of incomes derived from all other forms of material property.

Nor is it surprising that Malthus's Geometrical Ratio should be rediscovered in the 1920's [39], and that J. M. Keynes could warn that the Malthusian Devil had broken his chains in a world ridden by diminishing returns. Overpopulated Europe in particular was threatened, he believed. The inequality essential to adequate capital formation was unlikely to be tolerated, and Europe's terms of trade were destined to turn against her, dependent as she was upon produce imported from a New World now become subject to diminishing returns. It was questionable, therefore, if her precarious economy any longer enjoyed a margin of safety against dislocation, a probable sequel to the treaty of Versailles [23]. At least four aspects of economists' response to Malthus's *Essay* in this period merit attention.

1. Malthus's *Essay* directed attention to the critical role of limitational factors and to the impact of adverse externalities associated with population growth. His ultimate limitational factor was land or "room." He implied, however, that it could be some other source of man's living budget, should man practice moral restraint. While England's marginal per acre wheat yield was only 8 bushels in 1812, the average having fallen from 24 in 1771 to 22 in 1812 [8, 2nd. ed., pp. 225-26]. Malthus did not draw upon this sort of evidence. Instead he seems to have reasoned from the limitedness of arable land and its yields to his expositive theorem that the food supply could grow only 1 $/n$ per quarter-century where n denotes the existing food supply. This model gives a falling average but not finally a falling incremental rate of food and population growth as did P. F. Verhulst's logistic curve, designed in 1838 to reflect the increasing resistance population growth encountered from increasing density and its concomitants.

Malthus's model stressed the services of land. This interpretation is in keeping with K. F. Mather's account of the changes both in man's demands upon his environment and in the nature of his resource limitations. "A hundred years ago, nearly 80 per cent of all the things men used were derived from the plant and animal kingdoms, with only about 20 per cent from the mineral kingdom. Today only about 30 per cent of the things used in industrialized countries come from things that grow; about 70 per cent have their sources in mines and quarries" [25, pp. 55-56]. Some present-day economists, of course, take into account all or nearly all services, the demand for which derives significantly from demand for population. They view economic growth as a process of accumulating capital in all its manifold forms [20, Ch. 14], and population growth as a process of transforming population, physical environment, and capital of all sorts, as of given time periods, into population of subsequent time periods. "Life," as A. N. Whitehead [38, p. 160] observed, thus becomes "robbery," with human societies living on other societies and (as A. J. Lotka noted), having triumphed over their subhuman foes, finding themselves pressed by increasing numbers to struggle with one another. Malthus may thus be looked upon as an inspirer of post-Darwinian as well as of Darwinian theories of struggle. Moreover, Malthus, in his rationalization of essential agricultural self-sufficiency, a doctrine that later helped inspire German agricultural protectionism, reveals himself a defender of bastions of strength against both leveling forces and hordes. "When degree is shak'd," he might have said with Shakespeare, "then enterprise is sick."

Malthus's emphasis upon limitations to the augmentability of the food supply was not reasserted again until the close of the century. For, although natality did not begin generally to decline in Europe until in or after the 1870's, and then not enough to affect the rate of population growth significantly, per capita food consumption improved, both because yield per acre had gradually increased and because food, mainly cereal, was imported in large quantity from land-rich Eastern Europe and Europe Overseas as well as from some heavily peopled parts of Asia [14] [8, 3rd ed., pp. 296-301]. Toward the end of the nineteenth century, one and one-third centuries after England began to import cereals on balance, Sir William Crookes (1832-1919) warned that the question of the food supply "is of urgent importance today, and it is a life and death question for generations to come. . . . England and all civilized nations stand in deadly peril of not having enough to eat." He pointed to the importance of wheat, the lowness of yields, and the limited additional acreage suited to sowing —conditions that would permit world population to overtake world wheat supplies by 1931. Augmentation of yields and acreage prevented

the materialization of Sir William's forecast. J. S. Davis, having reviewed its basis and nonmaterialization, declared in 1932 that "the specter of coming dearth of food is a ghost that deserves to be laid" [13].

The shift in the bias of the resource base from organic to inorganic facilitated a higher rate of development and some economy in transport already near the beginning of the nineteenth century, if not earlier. It did not become pronounced until later in the century, however. Thus, between 1880 and 1937, while world population was growing about two-thirds and its industrial production was almost sextupling, world metal and fuel production increased 606 and 652 per cent, respectively [14] [31, pp. 248-50]. The shift, however, added to the fear of classical diminishing returns, the additional fear of mineral depletion, analogue of soil-fertility depletion. Already in 1865 W. S. Jevons forecasted that rising costs of coal attendant upon the working out of the superior seams would depress British industry and trade [19]. A quarter-century later, there arose even in the United States concern respecting both soil depletion and unduly rapid mineral exhaustion. This concern contributed to the development of the conservation movement later popularized by anti-Malthusian Theodore Roosevelt.

2. Malthus as well as subsequent writers drew attention to the externalities associated with population growth, many of them adverse. A clear-cut distinction was not initially made, however, between externalities produced directly and externalities produced indirectly through urban concentration. Malthus and his successors hoped to control aggregate growth and thus minimize the impact of adverse externalities—through institutional and related controls of private origin, that channeled costs and benefits to responsible and discretionary individuals and households. He had little confidence in Leviathan. In fact J. S. Mill was almost alone in believing that under communism—albeit small-scale communism rather than present-day bureaucratic socialistic or mixed-economy collectivisms—the feedback system generative of population-control would be much more effective than it was under noncommunism (*Principles*, II, i, 3) Concern regarding externalities associated with urban growth became both more intense and more research-oriented toward the close of the century. Even Marshall manifested such concern [24].

3. In the wake of this growing awareness of externalities there developed interest in improvement of the quality of populations. While few economists had the faith of a Helvetius or a Bentham in the capacity of education to eliminate inequality, not many attached to reproductive selection the importance assigned it by F. Galton's follow-

ers or by some exponents of Social Darwinism. After all, it was economic environment with which economists worked, and it was their usual view as well as Marshall's that A. C. Pigou [29, pp. 113-14, 121] expressed when, despite his awareness of the importance of genetic selection, he said, "The environment of one generation *can* produce a lasting result, because it can affect the environment of future generations. Environments, in short, as well as people, have children." He inferred, furthermore, that "improvement in the distribution of the dividend may be expected actually to diminish the proportion of the children born from inferior stocks" whose untrammeled propagation remained "notorious." Pigou seems to have been less alert than Ricardo (*Principles,* Ch. 31), however, to the possible depression of the demand for labor, especially among the inferior members of society, by accumulating capital. Otherwise he might have put more stress upon eugenics as well as upon adverse effects of life in large, input-short families.

4. Corollary to Malthus's thesis is the notion that there exists, under given conditions in a given state, an optimum population size and an optimum population growth rate. J. S. Mill was the first to give some precision to the former notion. "After a degree of density has been attained, sufficient to allow the principal benefits of combination of labour," wrote Mill in 1848, "all further increase tends in itself to mischief, as regards the average condition of the people." Such density had been attained in England. Mechanical inventions could, of course, increase average income, but they would increase it even more were numbers not to grow (*Principles,* I, xiii, 2-3; IV, vi, 2). Unlike present-day technicians with their dehumanizingly exclusive emphasis upon *know-how,* Mill was concerned also with *know-why*—he was not prepared to trade natural grandeur, solitude, "the spontaneous activity of nature," and other free goods for the junkyards and carscapes and deteriorating landscapes so characteristic of unaesthetic twentieth-century societies seemingly bent upon frustrating Henry Ford's desire that his car give even the man of little means easy access to "God's great open spaces." Mill would probably have endorsed B. A. Weisbrod's suggestion that account be taken of the willingness of nonusers of facilities (e.g., a park) to pay for the option of using them in the future, given that total revenue from users falls short of costs [37].

Mill's view was endorsed 78 years later by A. B. Wolfe, foremost economic demographer, who declared that "economically, the desideratum is . . . to obtain, in return for whatever expenditure of effort the people may regard as normal and proper, the largest permanently practicable, *per capita* product" [15, p. 68]. The population permitting this would be of optimum size. Wolfe did not accept, any more

than had J. S. Mill, the inference of A. M. Carr-Saunders [6] (and earlier Henry George) that a population normally assumed optimum size. Mill emphasized, as had Jeremy Bentham and others, the need for contraception, a need increasingly stressed during the Malthusian period [18]. Only a tremendous stimulus resulting in an improvement of "signal character," such as had been produced in France by the Revolution, could change men's habits, elevate their standards, and reduce their natality, Mill observed (*Principles,* II, xi, 2); and J. B. Clark (*Essentials,* Ch. 19) 59 years later expressed a somewhat modified version of this view. Not until the 1950's, however, did H. Leibenstein and R. Nelson incorporate a Mill-like view into growth models entailing the displacement of populations from the Malthusian equilibria in which they had been trapped.

The Malthusian period witnessed many improvements in both the art of population projection and the quantity and quality of statistical data, together with the instruments employed to analyze them. A major objective was to eliminate distortions occasioned in crude measures of fertility or mortality by differences or abnormalities in age, sex, and marital composition. Gross and net reproduction rates came into use toward the close of the period, some years after they had been anticipated; they made it possible to determine the rate of increase per generation in a population of stable age distribution, a model developed by A. J. Lotka as early as 1911. The demographer could then cope with changes in age composition associated with adjustment to constant age-specific fertility and mortality rates, though not with unpredictable variations in mortality, fertility, or sex composition [16, Ch. 12]. Population forecasters therefore resorted to projections of present populations based upon various sets of assumed age-specific fertility and mortality rates. Recourse was had also to logistic curves fitted to past populations, a method originated by Verhulst and rediscovered by Raymond Pearl and L. J. Reed, who forecast a maximum world population of about 2,646 million and a maximum American population of about 197 million as of around A.D. 2100 [10].

II. *A Time of Reconsideration: 1930-65*

Opinion on population issues was homogeneous during the period 1800-1930. All but Marxian opinion, whether critical or affirmative of Malthus's views, took his model as a point of departure. Not so in the period 1930-65, or perhaps better, the period 1925-65. Voices were many and discordant, and divers earlier views were rejected. Honored in the breach was Jacob Viner's advice to this body (in 1938), that the economic theorist "is the special custodian for society of the long view in economic matters" (*Am. Econ. Rev.,* March 1939, p. 9) as well as

the discoverer of hidden costs; or Winston Churchill's observation that "a hopeful disposition is not the sole qualification to be a prophet"; or Walter Bagehot's reflection that "it is a question whether the benevolence of mankind does most good or harm" (*Physics and Politics*, Ch. 5).

First of the earlier views to be reversed was that imbedded in the population forecasts (or projections as they now are more cautiously labeled) of the 1930's. Already in 1895 Edwin Cannan forecast a virtual cessation of population growth in England and Wales by 1950. In the late 1920's and 1930's it was inferred that the populations of most Western countries would soon become stationary, the American at a level below 200 million. Even eventual decline was anticipated. Net reproduction rates were near unity, and sometimes still falling. Economists began exploring economic life in hypothetically stationary or declining populations. Publicists and reformers, some of whom may have recognized an opportunity to clothe welfare-state philosophy in demographic terms, refurbished old arguments for a family wage as well as diverse subsidies to parents, with the result that family allowance systems won widespread approval. Inadequately explored in a still-Kiplingesque West, however, were implications of the fact that while the rate of population growth in a politically fissured Western world was falling, that of underdeveloped lands, containing about two-thirds of the world's people, was incipiently high and potentially rising.

By the late 1940's, however, changes in both fertility and population prospects were evident. Not only was death control augmenting natural increase in the underdeveloped world, natality and natural increase had risen in the developed world, in part as a response to rising employment and the emergence of the welfare state. Although fertility later moved somewhat below its postwar peak, population projections were revised upward, sometimes by a factor of 2 or more. Thus the population anticipated for the United States in 2000 was set above 300 million instead of below 200 million [10]. Still, should age-specific fertility move downward, as it now seems to be doing, postulated growth rates will be reduced. Real cohort analysis, designed in the 1940's to replace use of synthetic cross-sectional cohorts, should presently confirm whether current fertility declines reflect a decline in desired family size and signal the advent of a growth rate of only about 1 per cent. In the 1930-70 interval, of course, population will have increased more than anticipated in the 1930's, about 283 million, or 42 per cent, in the developed world, and 1,271 million, or 91 per cent, in the underdeveloped world.

A second reversal of interpretation relates to population growth and capital formation. G. Cassel [7, pp. 134-35, 149] had implied that a 1

per cent rate of population growth absorbs annual savings approximating 5 per cent of national income, but he did not anticipate excessive saving. Such confidence gave way, however, to fear of the advent of a stationary population, despite Mill's earlier description of its advantages. "Investment" (or savings-offsetting expenditure) had been freed meanwhile of its moralistic trappings, and made *a*, and after the appearance of Keynes's *General Theory* in 1936, *the* strategic variable in employment theory and policy. Given little population growth, investment would be inadequate because provision would no longer have to be made for large population increments, and at the same time there would be too little inclination to invest in improving the environment, quality, and productivity of those already here or replacing those here. This concern reflected an excessive estimate of the difficulties supposedly attendant upon adjustment from a higher to a lower rate of population growth, in an economy made sensitive to the contractile impact of the acceleration principle by a high ratio of reproductive wealth to population. In fact, J. R. Hicks, in his review of the *General Theory* (*Econ. Jour.*, June 1936, p. 252), declared population to be Keynes's "strongest card," and three years later added (*Value and Capital*, Ch. 24) that the "whole Industrial Revolution of the last two hundred years" had been "such a disappointing episode in human history" because (perhaps) it had been "nothing else but a vast secular boom, largely induced by the unparalleled rise in population." Alvin Hansen clearly posed the problems in his address to this body in 1939 (*Am. Econ. Rev.*, March 1940, pp. 1-15), at a time when economists were not yet writing about compensatory public investment in an affluent society. There were critical responses to exponents of stagnation theory, of course, especially G. W. Terborgh's respecting the favorable behavior of investment in a past marked by falling population growth [34], and those of economists who believed that small adjustments in a flexible economy could easily bring annual savings and offsets thereto into balance and assure "full" employment in an economy whether population was growing appreciably or not. Man need not be slave to the stork in the twentieth century any more than he was in the late nineteenth. Ansley Coale [12, p. 371] aptly capped this theme in 1960 when he pointed to implications of current American fertility levels:

> Thus a continued secular economic boom could gain partial support from a continued baby boom. But after a century this trend would produce about a billion Americans, and after two centuries some six billion. There must be a better way to stimulate employment.

After all, it should be quite easy to adjust saving and/or offsets thereto,

along with other significant variables, to changes in population size and age composition, both of which are quite predictable in the short run. Until recently, the inverse of the stagnation argument, the thesis that excessive population growth generates inflation, especially in underdeveloped countries, received little attention.

Perhaps the greatest reversal of opinion in the period 1930-65 is that relating to the role played by land and other natural resources in economic development and the disenthralling of populations from Malthusian traps. The importance of this role has been played down for a variety of reasons. First, investment in scientific discovery, applied technology, and education has been found to account for a major fraction of the increase in output per head in advanced countries, although recently the need to complement this type of investment with physical capital has again begun to be emphasized. Second, input of the services of land and natural resources per unit of GNP has greatly decreased in advanced countries. The composition of an individual's consumption changes as his income rises and becomes more biased toward products with relatively small resource content. This is reflected in a below-unity income elasticity of demand for most organic and inorganic raw materials. Furthermore, considerable economy has been achieved in the use of raw materials, with the result that resource-service input per unit of output of many specific types of products has fallen. For example, the input of cropland service per unit of output of agricultural products declined by nearly half in the United States between 1930-32 and 1960-62. In short, changes in the character of both demand and technology have slowed down the growth of various material requirements, sometimes to the pace of population growth and sometimes even below it. Third, discovery and technological change, together with substitution at producer and consumer levels, have greatly augmented both the visible and the immediately potential stock of fuel, mineral, and related sources of natural-resource services. Man, it is supposed, is confronted by chains of natural-resource substitutes which modern molecular engineering and alchemy can subvert to his purposes, replacing links that weaken and elevating inferior sources (e.g., taconite rock) as well as substituting less expensive for more expensive sources of particular natural-resource service needs [1] [33]. For example, energy should prove producible in large amounts through fission assisted by breeder reactors, and in almost unlimited amounts should fusion prove technologically and economically feasible.

A measure of parochialism permeates this optimistic account. It neglects the Apostle Matthew's dictum that "for whosoever hath to him shall be given, and he shall have abundance," and the observation in Deuteronomy "that man doth not live by bread only." Skill and capi-

tal, so essential to overcoming mineral and agricultural shortages, are generally least abundant where needs are greatest. Should income grow as expected in the underdeveloped world, natural-resource shortages will be intensified, probably in greater measure than technical capacity to countervail them. Similarly, diets reportedly are quantitatively and/or qualitatively most deficient in Asia, Africa, and parts of Latin America—in areas commonly short of skill and capital, even when not also of land. There one finds not even a basis for the inverted Malthusianism of Western optimists which equates solution of the population problem with a full stomach. One also finds neglected two income-depressing effects of population growth: (a) absorption of nonagricultural land which might better have supplied man with amenities; (b) steady diminution of the amount available per capita of suitably situated space and goods and services formerly *free*.

Neglected above all is man's conversion of such "parameters of state" as atmosphere, topography, and climate into variables with the result that conditions of life and interspecies equilibrium—never very stable—are changing. Lotka anticipated this possibility in his remarkable *Elements of Physical Biology* (1925). Malthus had treated physical environment as a constant even though man had for many centuries been modifying this environment [35]. But then Malthus lived before the goat had been replaced by bulldozer, dragline, and nuclear explosives, and man had become the most destructive of biotypes. After all, even the physicist, R. A. Millikan, could write as late as 1928: "The energy available . . . through the *disintegration* of radioactive, or any other, atoms may perhaps be sufficient to keep the corner peanut and popcorn man going, on a few street corners in our larger towns," but no more (*Science*, Sept. 28, 1928, p. 284). Today, man is confronted not only by denudation and erosion of soil as well as possible mineral shortages but also by the threat that accumulating filth will convert his promised great society into a merely gray society. Witness here in America endless dumping of trash (four pounds per person per day), lead contamination in excess of that experienced in ancient Rome, and water and air pollution soon destined to cost above $7 billion yearly. Merely cleaning up the nation's polluted waters could cost over $50 billion. Indeed, some hold, J. K. Galbraith had better labeled ours an effluent society than an affluent one, perhaps so effluent that many of its members have lost their sensitivity to effluvia. Possibly more threatening still are the changes man is producing "in the geochemical cycles of the elements," thereby "disturbing their natural balance in the uppermost geospheres." Thus since 1900 the carbon dioxide content of the air has risen about 10 per cent and is still rising [30, pp. 365-66, 546-47]. In short, multiplying man, forced by popula-

tion pressure to cope with emerging shortages in his micro-environment, may be worsening his macro-environment and constraining the adaptive potentialities which have gotten him where he is.

Optimum theory has been elaborated and clarified in recent decades. While account has been taken of the content of the welfare index being maximized, it has been noted that output per man-hour may constitute a quite satisfactory index, insofar as deviations from any other index can be resolved through exchange supplemented by taxes. It has been observed also that, while population size may condition a society's capacity for reducing income inequality, it is size of economy much more than size of population which facilitates economies of scale, competitiveness, flexibility, etc. The circumstances governing the response of welfare indicators to variation in population size have been found to include extent of economies of scale, interindustry fit, external economic relations, tastes, technology, length of time horizon, burden of defense or tribute and foreign aid, and so on. Deviation of actual population size from optimum size does not tend to induce a decline in fertility unless this deviation reduces the rate of growth of income below the expected level; this rarely happens in the neighborhood of the optimum, and is not very likely when other income-increasing forces are ascendant. It is possible, of course, that as man's time horizon is extended through prolongation of life and institutionalization of expectations, sensitivity to possible deceleration of income growth increases. Little attention has been given to the stability conditions surrounding an optimum; yet failure to act in conformity with these undermines some of the conditions defining this optimum.

Today emphasis is placed less on the optimum as such than on the broader question: Is additional population growth advantageous? Will it elevate or reduce the rate of growth of per capita income with which other indicators of welfare are highly correlated? Will it contract or extend the range of choice? After all, in much of the world average population density exceeds the optimum level; moreover, many states are too small to accommodate a modern economy, even given a great deal of external exchange. In those countries, moreover, where a larger population would be advantageous, it is highly desirable that increments be added slowly, since a high growth rate sacrifices population quality and equipment to sheer quantity (e.g., in Latin America and Africa). Major issues tend to be examined when inquiry is directed toward assessment of the effects of further growth. Such inquiry led Richard Stone to ascertain how population growth retarded a society's approach to F. P. Ramsey's state of Bliss. It directs attention to the adverse input-absorbing externalities generated by population growth in general and through increasing megalopolitan concentration—usually

in greater measure than associated external economies (if any). It prompts examination not merely of effects associated with short-run allocation and partially offsetting responses to disadvantages of population growth, but also of the longer-run consequences of alternative uses of inputs transformable into population. It reveals how population growth and megalopolitan concentration tend to bureaucratize society and dissipate individual autonomy [32].

The optimum concept has been applied to cities as well as to national populations, though with even less success, because of the heterogeneity of cities. Some attention is also being directed to the advantages and disadvantages of city growth. Much of the work currently done on cities amounts to little more than empiricist inquiry, insensitive to the manner in which distortion of exchange in the urban realm, especially distortion of key exchanges, may distort the urban structure and impose suboptimal conditions of existence upon the passive majority.

Turning from optima to change in population composition, we find two dimensions of increasing concern. The first is age structure. France's population was the first to shed the child-ridden Oriental age structure common to populations in Malthus's day. Modern age structures, together with increases in the proportion of the population over 60-65 years, are concomitants of relatively low fertility; prolongation of life, unless beyond 70, affects this proportion very little. Of course, should it be forgotten that death is essential to progress and should man become immortal, a really Struldbruggian society could develop. For then, given a Gross Reproduction Rate of 1.5, the proportion over 60 would approach 41.4 per cent [11, p. 40]. There would still be a way of escape, however; for, by raising the GRR to 3.0, the proportion aged 60 and over could be held to 9.1 per cent. Man would thus be faced with a choice—between life in a nursing home and life in a sardine can. Fortunately, the bad as well as the good still die, though perhaps in suboptimal proportion. There is need for hardheaded socioeconomic gerontological inquiry into the allocation, employment, income, and related implications of a doubling or more of the fraction of the population over 60 or 65 years since (say) 1870 when only about 5 per cent of the males were 65 or older and many of these lived in nonurban areas.

Perhaps the central economic task flowing from this aging is that of optimizing, over the life span of the representative individual, of his work-time and his discretionary time. Presumably, this arrangement would result in greater vacation time than now—say six weeks per year—and deferment of retirement until age 70. Such arrangement is currently threatened by corporate, trade union, and governmental bureaucrats who, abhorring discriminating decision as the devil abhors

holy water, would fix the age of retirement at or below 60 when one still looks forward to 17-21 years of life. Such a policy would produce an army of endowed, if not particularly happy, loafers. Even in the absence of continuing inflation, decrease in the ratio of work life to retired life by four-sevenths since 1900 entails an offsetting increase in saving for retirement which has not usually been achieved. And even then, as a rule, the retiree does not share appreciably in the fruits of technical progress. Solution consists in part in the re-establishment of flexible, discriminatory wages, periodic worker retraining, possible job reassignment, portable retirement provisions, price stability, and an evolution in work scheduling. The alternative could be a variant of Townsendism.

The second problem-ridden structural change accompanying population growth is not mere urbanization (which within 30 years, according to K. Davis, will gather over half the world's population into cities of above 100,000), but the concentration of so large a proportion of the population in megalopolitan centers which appear to be uneconomic devourers of time and capital as in the past they were devourers of population. This concentration appears to be the product of an indeterminate mechanism, made up in the part of random variables and fostered by disparities between social costs and social benefits as well as by neglect of adverse externalities associated with concentration. In short, the urban growth pattern may be stochastically determined, with chance working on the side of the largest centers and in keeping with Zipf's generalized Paretian, or rank-size, "the-higher-the-fewer rule." Here we may not have a causal mechanism operative, since, as M. G. Kendall [21] notes, "chance can mimic choice," nor do we have simple, unlagged cause-effect relationships [22]. There could, of course, be something like G. Myrdal's cumulative causation at work, or perhaps a cumulative process somewhat analogous to that described by W. J. Baumol in his "theory of cumulative deterioration" [2].

What is of immediate concern, however, is whether the mechanism at work produces an optimum distribution of activities in space and of population among cities. The outcome, whether good or bad, is almost irreversible, underpinned as it is by fixed capital, special interests, inertia, subsidies, and so on. That the outcome is optimal is to be doubted despite the clamor of hyperurbanists. They forget that vulnerability to attack by missile and from the air is positively associated with population concentration. It is essential that one take into account *all* costs as well as *all* benefits associated with large-city expansion, so remindful of the growth of polyp colonies; only then is the marginal net effect ascertainable. Excessive concentration of population in large cities not only tends to dissipate *gemeinschaftliche* ties, and generate

alienation; it is also inimical to individual-political and consumer sovereignty. Mathematically, the larger the city, the smaller is the relative size of the resolute minority that could rule it [28]. Politically and psychologically, the larger the city, the weaker tends to be the belief of the voter that he can do *anything* to influence the ruling camarilla and improve his community. Finally, as political power is channeled into the hands of city-ruling minorities, the taxing power of the central government tends to be placed increasingly at the disposal of these minorities and perhaps in keeping with what one may call the Augustus Caesar principle of public finance. (You will recall Seutonius's account. Caesar once considered discontinuing the practice of distributing free grain, but he decided against it even though it was injuring Italian agriculture. "I did not carry out my purpose," he said, because I felt "sure that the practice would one day be renewed by someone ambitious of popular favor.")

While notable contributions have been made to the economic analysis of fertility [3, 12], the explanation of its short-run behavior has not yet been made as economic as possible, nor have the actual and the potential feedback connections of household with economy been adequately identified and explored. If one allows for random variables and treats certain fertility-affecting elements (religious and other values, rural-urban and educational differences) as short-run constants, one should be able to explain changes in gross reproduction in terms of changes in price structure and opportunity costs and of movements along curves of demand for children and of shifts of both these curves and curves representing costs of children. For a change in fertility reflects the response, usually lagged, of actual and potential households to changes in (a) the prices and character of alternatives to children, and (b) the benefits anticipated to be derivable from children and/or changes in the input costs of children, together with additions (subtractions) in costs associated with changes in family economic status. It must be assumed, of course, that fertility is easily controlled. This assumption is quite tenable in advanced countries, now that earlier contraceptive methods, including the unreliable "safe-period"—in Spain children who owe their existence to failure of this method are eponymously described as *Oginitos* after the method's discoverer (*New Leader*, Sept. 13, 1965, p. 7)—have been supplemented by contraceptive pills and cheap, effective intrauterine devices. It is essential also that the analyst view the child as the partial analogue of both a consumer's and a producer's durable good. Then the analytical as well as the empirical problem becomes one of isolating the determinants that affect the demand for, and the supply of, children at household levels, and thus give rise, along with household interdependence, to aggregate demand and supply functions.

The demand for children is not quite parallel, however, to that for ordinary durable goods. The relevant time interval is too long to permit a household to test *ex ante* expectations respecting children by *ex post* experience. No one can foresee what the genetic grab bag will bring forth, probably a useful citizen, possibly a genius, perhaps a hooligan or a cretin, but by then corrective response may no longer be possible. There is less uncertainty, of course, on the supply side. Most direct costs are fairly predictable, and optional additional costs need not be incurred unless they fall short of the prospective benefits. Less predictable is the impact of trade and longer cycles, or of increases in unemployment consequent upon upswings in the relative number of young job-seekers, cobweb-like sequels to earlier upsurges of births [12]. Less predictable also, especially in a modern demand-oriented economy, is the response over time of a household to its increasing access to durable goods and imperfectly foreseen complements thereto. For in this sector of consumption the income elasticity of demand can easily rise above unity and, if this rise is widespread enough, divert income from support of procreation to purchase of durable products and complements. The full significance of the emergence of durable goods for reproductive and other activities has yet to be analyzed adequately. Indeed, the Ford Foundation might fund a project permitting a corporate body of social scientists to follow through life a representative cohort, or better still a sequence of cohorts chosen at (say) 10-year intervals, and ascertain how and why patterns of expenditure change and with it fertility and related behavior.

Underdeveloped countries, United Nations demographers find, differ from developed in respect of both fertility and indices of modernization. Gross reproduction is below 2 in all developed countries, and above 2 in the underdeveloped world which includes nearly all African, Asian, and Latin American countries. Within the modern world there is no correlation between variation in indicators of modernization and variation in fertility; and within the underdeveloped world there is only slight correlation. The data do suggest, however, that there exists a threshold, or a set of thresholds, movement beyond which by indicators of modernization is quite likely to be accompanied by a perceptable downward movement of fertility, though not by one immediately adequate to offset falling mortality.

Convincing evidence of a widespread, imminent, and pronounced decline of fertility and natural increase remains to be uncovered. The United Nations reports: "the launching of new countries upon the transition from high to low fertility seems to have been temporarily halted." The evidence reveals no continuing inverse relation between population density and fertility in keeping with the logistic theory; only a mildly inverse relation is found in both the developed and the

underdeveloped world. The pressure of numbers upon family resources generated by the decline of infant and child mortality has not yet widely reduced births per mother. Governments not only show little awareness of the urgency of the population problem; they even cushion the incidence of child-rearing costs on responsible parents. The Economic and Social Council of the United Nations, together with individual countries, has sanctioned making contraceptive assistance internationally available, but governments have provided little incentive thereto [36].

The economic implications of United Nations studies of fertility and population growth are at least fourfold. First, for a number of decades population growth will absorb large amounts of physical and personal capital that might otherwise have been used to elevate per capita income. For example, *ceteris paribus,* per capita income can grow at least one percentage point more per year, given a zero instead of a two-per-cent-per-year rate of population growth. Second, little explanatory insight is provided by economic models which merely suggest that income will permanently outstrip numbers, but which are unequipped with feedback mechanisms that produce at least limited downward movements of fertility. It is necessary to show that movements of income generate even greater relative demands for present and future goods, services, and leisure; that elasticity of consumption expectations exceeds elasticity of income expectations. Then, if the standard of life rose ratchet-like, but always guarded against decline by pawl-like acquired tastes, one might count upon the retention of internalized standards. Third, the apparent existence of a set of interrelated thresholds suggests that international differences in shorter-run fertility-affecting constants are dissipatable through subjection to socioeconomic solvents. Fourth, even though these parametrical differences exist, it is certain that fertility can be greatly reduced, given availability of contraceptives, if having more than a stipulated number of children, say three, is economically penalized instead of subsidized as at present. That it would pay the governments of underdeveloped countries to reward those who curb their fertility has been demonstrated by Stephen Enke who estimates one dollar thus spent to be worth about $100 of other forms of aid [17].

III. *The Future*

Technology, prudence, and reason could generate a bright future. That it will be bright, however, is questionable, given an unfettered stork, together with continuing escalation of submarginal man in a world of Freudian irresponsibility and distribution according to al-

leged need rather than demonstrated performance. Just as relative immunity of an organ's cells to division and mutation causes that organ to age, so does the relative immunity of a society to rigorous selection tend to bring about its deterioration. It may be well, however, for one interpreting these portentous trends to recall what Samuel Johnson wrote in the preface to his dictionary about the prophecies he had made earlier in his launching prospectus: "I . . . now begin to fear that I have indulged expectation which neither reason nor experience justify." Yet, mankind does appear to be at a demographic crossroads, along which weak homeostatic response under current conditions may not properly direct man. Population growth still resembles too closely Newton's "uniform motion in a straight line," unrelieved by the prospective emergence of fertility-controlling "impressed forces."

Consider India. Down one of the crossroads could lie a sanguine future. For, as S. J. Patel suggests, the economic distance between India and present-day America might be bridged, though hardly in a half-century; the difference between Indian and American real agricultural output per capita could be removed in some 50 years; that in other sectors, in a longer period. Yet, as Coale and Hoover have shown, there can be little improvement in the average Indian's lot if India persists down the road of population growth [26].

More generally, to paraphrase President Kennedy's warning against war: Unless man halts population growth, population growth will halt man. As matters stand, two demographic processes are bound to bedevil man increasingly and, if they are not soon resolved, will greatly diminish whatever prospect he has of establishing a society both peaceful and great, both moral and aesthetic. These two processes are continuing population growth in a finite world and increasing concentration of people in progressively larger megalopolitan centers. Progeny and space will have to be rationed much more effectively in the future than in the past.

The more remote prospect is bleak while the immediate prospect, blurred by current income growth, is misleadingly hopeful. Population growth—demographic entropy—is dissipating man's capacity for maneuver much as economic entropy is dissipating the utility locked up in depletable natural resources. After all, increase in number is an essentially irreversible process; men usually multiply, but they seldom dwindle as did avian, mammalian, and other species now extinct. Even in the United States should natural increase plus a prospective annual immigration of 350,000 long continue to increase population 1.0-1.5 per cent per year, our population, 325-350 million in 2010, would number 0.8-1.33 billion by 2100, and 1.5-2.8 billion (with perhaps one-fourth nonwhite) two centuries hence. Acres of land of all sorts per

capita, probably still close to 5.5 in 2010, would continue to decline until there would hardly be space even for our national flower, the concrete cloverleaf. Aesthetic considerations of all sorts would be swamped by masses instead of by costs masquerading as pseudo-utility as at present. Even in the absence of strepitous poverty-mongers many would be conscious of being poor.

An even worse prospect obtains in the world at large. Between 1960 and 2000, according to United Nations estimates, world population will grow nearly twice as fast as in 1920-60—around 113 per cent to between 6 and 7 billion. The proportion in the developed world will fall to around 22 per cent, since its numbers will grow only 61 per cent, or but two-fifths as fast as the population of the underdeveloped world which is expected to increase about 151 per cent. Should world population continue to grow about 2 per cent per year it would number 16-18 billion by 2050 and 43-50 billion by 2100. Even if it should proceed only 1.5 per cent per year it would number 13-15 billion by 2050 and 26-31 billion by 2100. Acres of all sorts per capita, still about 4.7 to 5.5 in 2000, will then have fallen to 0.6-1.25 by 2100, and close to one-half of this amount will be unfit for habitation except by Lower Slobbovians. Man will have become essentially spaceless in a space age, unless competition for living space should accidentally transform the "ultimate deterrent" into the "ultimate detergent" and, as D. M. Heer estimates (in his *After Nuclear Attack*), destroy perhaps 30 per cent of the population, mostly in heavily urbanized areas.

Given growth of the order suggested as likely in the absence of concerted efforts to limit numbers, food shortages would eventually develop despite schemes for deriving protein from coal and petroleum as well as subsistence from algae and other crude organic matter, or for supporting a man on 20 well-cultivated square meters of soil. First, cultivable land is limited, though only about 7 per cent is used for nonfood crops, mainly fibers. In 1964 arable and permanent-crop land totaled about 3.6 billion acres; land in permanent meadows and pasture, about 6.2 billion acres; and land in forest, about 10 billion acres. Just how much land in the two latter categories is convertible into crop land is indeterminate, though subject to exaggeration. It depends upon technology, upon mastery of cultivation in the tropics and in droughty and cold regions, upon land clearing and development, often a costly process, and upon adequacy of the water supply. From the gross amount of land so converted must be subtracted land blotted out of agriculture by creeping concrete, construction, city growth, etc., which in the United States has amounted to one-half acre or more per inhabitant added to the urban population.

Second, though average yield per acre could prove capable of 300 or

more per cent increase, crop yield per acre is limited. Yield increases are promising, of course. After all, "over a large part of the world today agricultural productivity is now the same as, or even inferior to, what it was in the leading civilized communities 2,000 years ago" [9, p. 78]. Yields per acre vary widely; thus Japan produces roughly 3-4 times as much rice per acre as do South and Southeast Asian lands. This variation is associated mainly with variation in input of fertilizer, addition of a kilogram of which may increase grain output about 10 kilograms, given suitable methods of cultivation. Developed countries use 5 times as much fertilizer per acre as do the underdeveloped; Europe uses about 20 times as much as do Africa and Asia and 9 times as much as does Latin America; Japan uses over 100 times as much as do India and Pakistan.

In many if not most underdeveloped countries pre-takeoff conditions essential to the great upsurges in yields experienced in the United States, Britain, and Japan have not yet been established [5]. Nor are they soon likely to be established, given excessive population growth and heightened barriers to the detraditionalizing of agriculture. Yet, as James Bonner shows, even if a takeoff is experienced, it cannot carry average production per acre above the limits "determined by the factors that regulate photosynthetic efficiency" and "being approached today . . . in parts of Japan, of Western Europe, and of the United States" [4, p. 14] [5]. Within limits, of course, the amount of nutrient supplied per acre can be augmented through recourse to higher-yield crops (e.g., soya beans).

The future of the food problem may be stated simply. Suppose we increase world output 700 per cent by doubling crop acreage and quadrupling yields. World population would catch up with this increase in about 107 years, given an annual growth rate of 2 per cent, and in about 140 years, given a 1.5 per cent rate. Trebling crop acreage would add perhaps a quarter to the above output, but only about one-tenth to the years of grace. Rationalization of fisheries would not change the data significantly. Augmentation of the food supply in the measure indicated is unlikely. This is not *one* world; requisite knowledge and its necessary complements are not likely to be imported and assimilated rapidly in backward lands; nor will progressive lands have incentive to export food, mainly cereal (of which about 5 per cent net is redistributed by international commerce) to exchange-short countries.

Turning from land, the principal source of man's food and the sole source of his living space, to water and minerals, the other primary ingredients of man's material well-being, one encounters limitations parallel to those of land. A precise measure of these limitations is not available. It depends on rates of consumption which are rising, upon actual

and exploitable reserves which remain underinventoried, and upon man's ability to develop substitutes for current sources. Even so, the outlook is not as promising as our Indian Summer natural-resource experts would have it. Consider water, long the economist's standard exemplar of a free good. We Americans use in all forms over 370 billion gallons of water per day and by 2000 we shall require 900 billion gallons, which probably can be provided out of the 700 billion available of the 1,200 billion that daily flow to the sea [40]. This amount, together with what is otherwise required and used of an average daily rainfall of about 5 trillion gallons, will not suffice beyond the present century. Then we shall have to reduce our average consumption, so imbedded in our mode of living, since it is improbable that desalination of sea water can add greatly to our supply of appropriately situated water.

The long-run availability of large-volume materials (e.g., steel, aluminum, concrete, ceramics, glass), together with our ability to cope with shortages of special small-volume materials, depends, as F. G. Tryon [15, pp. 136-38] suggested 40 years ago, upon man's ability to produce and appropriately distribute cheap energy. Under present conditions and at prospective use rates, the world's exploitable reserves of a number of minerals will last less than a century. Aluminum and iron would last 4 centuries at recent use rates, and the key fertilizers, potassium and phosphorus, much longer. The World Power Conference of 1962 put economically exploitable coal reserves at 3.5 trillion tons, enough for 800 years at current annual world consumption rates (about 4.4 billion tons in 1960) and 100 years at American rates. Other fossil fuels will last only into the next century. Water power could supply the equivalent of 2.5 billion tons of coal per year, or less than half current world energy consumption. Given breeder reactors, usable uranium might come close to supplying as much energy as current mineral or fossil fuel reserves; and low-grade thorium could supply 10-20 times as much. Fusion, when it becomes manageable, could make the supply of energy virtually inexhaustible, even in the absence of tidal, solar, geothermic, and related sources [27] [33, pp. 74-97]. In short, given the cheap energy likely to be available, shortage of space, land, and water rather than of minerals would check the growth of population and living standards. Limits will be imposed also, of course, by unavoidable shortages of skilled personnel. Even given requisite natural talents, we cannot long increase skilled manpower faster than population; otherwise the whole of the labor force will be transformed into economists, engineers, physicians, and other categories of skilled manpower, and none will be left to minister to other wants.

I have just been concentrating upon what exponential growth can do in a finite environment, upon a growth process which, as Coale reports [11, p. 36], within 65 centuries and in the absence of environmental limits, could generate "a solid sphere of live bodies expanding with a radial velocity that, neglecting relativity, would equal the velocity of light." Let me now recall that second process operative in our world of shrinking space, a process to which man must respond even as the ratio of suitably situated space to population declines. I refer to that seemingly stochastic process which is concentrating an ever larger fraction of the world's population within the confines of a small number of megalopolises or human rabbit warrens. This process accentuates the adverse externalities associated with population growth and, as has been suggested, may produce unfavorable political effects as well as frustrate the demand for living space which tends to rise with income. But let me turn to solutions.

Curbing population growth could prove easy in modern nations, given current contraceptive methods and knowledge thereof. It is necessary only to alter the terms of trade between gross increments to the population and alternative uses of the inputs involved. In essence, this might entail holding each couple responsible for all or most costs, visible and invisible, direct and indirect, of reproducing and rearing children, in keeping with at least minimal standards prescribed by the state. Should the number of births still be too large, excise taxes could be imposed upon higher-order children and upon child-oriented products and services, while if births were too few, these products and services could be cheapened through use of subsidies.

Control of megalopolitanization and the redirection of urban growth are achievable through use of taxation, penalties, and constraints. Initially, however, much more study needs to be made of longitudinal and cross-sectional relations between variation in city size and marginal cost-benefit ratios under essentially comparable conditions.

As matters stand, the longer-run prospect is definitely Malthusian, with man sitting on a demographic time bomb into which legislators, and others, here and abroad, continue to shovel combustibles. Yet there may be grounds for conditional optimism. Not only can economists devote to the population question more attention than they gave it in the past 80 years when they devoted only about 1-1.5 per cent of their articles to population,[1] but there are many more economists to do the job. Today more economists are practicing than lived and died in the past four thousand years, and their number is growing even faster than the world's population.

[1] This estimate is based upon entries in the *Index of Economic Journals*, sponsored by the American Economic Association.

REFERENCES[2]

1. H. J. BARNETT AND C. MORSE, *Scarcity and Growth*, Baltimore 1963; T. W. SCHULTZ, *Economic Organization of Agriculture*, New York 1953.
2. W. J. BAUMOL, in H. G. Schaller, ed., *Public Expenditure Decisions in the Urban Community*, Resources for the Future, Washington, D.C. 1963.
3. G. S. BECKER, "A Theory of the Allocation of Time," *Econ. Jour.*, Sept. 1965, *75*, 493–517; B. OKUN, *Trends in Birth Rates in the United States since 1870*, Baltimore 1958.
4. JAMES BONNER, "The Upper Limit of Crop Yield," *Science*, July 6, 1962, *137*, 11–15.
5. L. R. BROWN (U. S. Department of Agriculture), *Increasing World Food Output*, Washington, D.C., and *Man, Land & Food*, Washington, D.C. 1963; T. W. SCHULTZ, *Transforming Traditional Agriculture*, New Haven 1964.
6. *The Population Problem*. London 1922.
7. *On Quantative Thinking in Economics*. Oxford 1935.
8. COLIN CLARK, *Conditions of Economic Progress*, 2nd ed., London 1951, 3rd ed., 1957.
9. COLIN CLARK AND M. R. HASWELL, *The Economics of Subsistence Agriculture*. London 1964.
10. M. CLAWSON *et al.*, *Land for the Future*, Baltimore 1960; IRENE B. TAEUBER, "The Development of Population Predictions in Europe and the Americas," *Estadi'stica*, Sept. 1944, No. 7, 323–46; J. J. SPENGLER AND O. D. DUNCAN, eds., *Demographic Analysis*, Glencoe 1956; J. S. DAVIS, *The Population Upsurge in the United States* (Food Research Institute War-Peace Pamphlet 12), Stanford 1949.
11. A. J. COALE, "Increases in Expectation of Life and Population Growth," in Louis Henry and Wilhelm Winkler, eds., *International Population Conference*, Vienna 1959.
12. ———, ed., *Demographic and Economic Change in Developed Countries*. Princeton 1960.
13. J. S. DAVIS, "The Specter of Dearth of Food; History's Answer to Sir William Crookes," in *Facts and Factors in Economic History*, Cambridge 1932, pp. 732–54.
14. J. F. DEWHURST *et al.*, *Europe's Needs and Resources*, New York 1961; S. N. PROKOPOVICZ, *l'industrialisation des pays agricoles et la structure de l'économie mondiale après la guerre*, Neuchatel 1946.
15. L. I. DUBLIN, ed., *Population Problems in the United States and Canada*. New York 1926.
16. ——— *et al.*, *Length of Life*, rev. ed., New York 1949; R. R. KUCZYN-

[2] Bibliographical note: For statistical and background information I have drawn on W. S. and E. S. Woytinsky, *World Population and Production*, New York 1953; United Nations, *Population Bulletin*, Nos. 6–7. and *Provisional Report on World Population Prospects . . . in 1963*, New York 1964; D. V. Glass and D. E. C. Eversley, eds., *Population in History*, Chicago 1965.

SKI, *The Balance of Births and Deaths*, New York 1928, and *The Measurement of Population Growth*, New York 1936.

17. S. ENKE, *Economics for Development*, New York 1963; "Economic Programs to Prevent Births," presented at World Population Conference, Belgrade, Sept. 1965; and "The Economic Aspects of Slowing Population Growth," to appear in *Econ. Jour.* March, 1966, *76*.
18. N. E. HIMES, *Medical History of Contraception*, Baltimore 1936; R. O. GREEP, *Human Fertility and Population Problems*, Cambridge 1963.
19. *The Coal Question*, London 1865; H. W. SINGER, "The Coal Question Reconsidered: Effects of Economy and Substitution," *Rev. Econ. Stud.*, June 1941, *8*, 166–77.
20. H. G. JOHNSON, *The Canadian Quandary*. Toronto 1963.
21. M. G. KENDALL, "Natural Law in the Social Sciences," *Jour. Royal Stat. Society*, Ser. A, Part 1, 1961, *124*, 1–18; H. A. SIMON, *Models of Man*, New York 1957, Ch. 9.
22. ———, *New Prospects in Economic Analyses* (Stamp Memorial Lecture 1960). London 1960.
23. *The Economic Consequences of the Peace*. New York 1920.
24. *Memorials of Alfred Marshall*, edited by A. C. PIGOU, London 1925; A. F. WEBER, *The Growth of Cities*, New York 1899; Don Martindale's introduction to Max Weber, *The City*, (1921), Glencoe 1958; P. A. SOROKIN AND C. C. ZIMMERMAN, *Principles of Rural-Urban Sociology*, New York 1929.
25. *Enough and to Spare*. New York 1944.
26. S. J. PATEL, "The Economic Distance Between Nations: Its Origin, Measurement and Outlook," *Econ. Jour.*, March 1964, *74*, 126–30 and Sept. 1965, *75*, 632–36; A. J. COALE AND E. M. HOOVER, *Population Growth and Economic Development in Low-Income Countries*, Princeton 1958.
27. E. W. PEHRSON, "Mineral Resources-Hydrosphere vs. Lithosphere," unpublished; D. F. FRASCHÉ, *Mineral Resources*, Washington, D.C. 1962; K. HUBBERT, *Energy Resources*, Washington, D.C. 1962; *Index*, no. 6, 1965, 1–2.
28. L. S. PENROSE, "Elementary Statistics of Majority Voting," *Jour. Royal Stat. Society*, 1946, *109*, 53–57.
29. *Economics of Welfare*. London 1932.
30. K. RANKAMA AND T. G. SAHAMA, *Geochemistry*. Chicago 1950.
31. J. J. SPENGLER, "Aspects of the Economics of Population Growth," *So. Econ. Jour.*, Jan. 1948, *14*, 233–65; E. A. WRIGLEY, "The Supply of Raw Materials in the Industrial Revolution," *Econ. History Rev.*, Aug. 1962, *15*, 1–16.
32. ———, "Population and Freedom," *Population Review*, June 1962, *6*, 74–82; also Colin Clark's counterargument, "Do Population and Freedom Grow Together?" *Fortune*, Dec. 1960, 136 ff.
33. ———, ed., *Natural Resources and Economic Growth*, Resources for the Future, Washington, D.C. 1961.
34. *The Bogey of Economic Maturity*. Chicago 1945.

35. W. I. THOMAS, *Man's Role in Changing the Face of the Earth.* Chicago 1962.
36. UNITED NATIONS, *Report of the Secretary General* (E/3895, May 18, 1964) to the Economic and Social Council, 37th Session, Agenda item 21.
37. "Collective-Consumption Services of Individual Consumption Goods," *Quart. Jour. Econ.*, Aug. 1964, *78*, 471–77.
38. A. N. WHITEHEAD, *Process and Reality*, New York 1929; A. J. LOTKA, *Elements of Physical Biology*, Baltimore 1925.
39. A. B. WOLFE, "The Population Problem since the World War," *Jour. Pol. Econ.*, in 3 parts, 1928–29, *36–37*.
40. ABEL WOLMAN, "The Metabolism of Cities," *Scientific Amer.*, Sept. 1965, *213*, 178–93; ROGER REVELLE, "Water," *ibid.*, Sept., 1963, *209*, 92–109; C. C. BRADLEY, "Human Water Needs and Water Use in America, *Science*, Oct. 26, 1962, *138*, 489–91.

AN INTERNATIONAL COMPARISON OF THE TREND OF PROFESSIONAL EARNINGS

By TIBOR SCITOVSKY*

I

The purpose of this paper is to assemble and present in comparable form some of the statistics available on professional incomes in a number of Western countries. The professions selected are the law, medicine, dentistry, the higher civil service, and higher learning; the countries are Canada, Denmark, France, Germany, Norway, Sweden, the United Kingdom, and the United States. An attempt has been made to carry the data back to before World War I whenever possible.

My interest in professional incomes has been aroused by the alleged and oft-cited connection between economic growth and the advance of democracy on the one hand and the worsening economic status of the intellectual and professional classes on the other. This trend has been noted not only in the United States but in most Western countries; it explains the springing up, all over Western Europe, of organizations of intellectuals and university graduates, aimed at defending their interests and drawing the public's attention to their plight. In Canada, in the 40 years between 1911 and 1951, the income of professional people is estimated to have risen only one-quarter as fast as the income of industrial workers; in Germany, over a period half as long (1936-55), the average income of the free professions is said to have risen by half as much as the income of the rest of the economy [4] [5]. The same trend, though less pronounced, is present also in Great Britain, where, between 1911 and 1956, the average income of the liberal professions has risen by two-thirds of the rise in the national income per head of occupied population. (Cf. Col. 16 of unpublished appendix table on U.K.)

Perhaps the first person to notice and analyze this phenomenon was Alexis de Tocqueville. In his *Democracy in America* he remarked and

* The author is professor of economics at the University of California, Berkeley. The data presented in this paper were collected during his tenure of a Ford Faculty Fellowship in 1957; and he is also indebted to many people in Denmark, England, France, Germany, Norway, Sweden, and the United States for helping with the search. The Institute of Business and Economic Research at the University of California, Berkeley, generously provided research assistants for this and other projects; and the author owes a special debt to Mary H. Hager, his research assistant who did the most work on this paper.

speculated upon the lesser disparity in the United States than in France, and in the France of his day than in that of Napoleon, between the salaries of high civil servants and that of office messengers.[1] He explained the difference in these disparities by differences in the degree of democracy—more democracy in America than in France, and more democracy in the France of 1833 than in that of 1810. According to Tocqueville, an aristocracy tends barely to allow a subsistence minimum to office messengers but to vote high salaries for the high offices of State, positions which they or their children might occupy and benefit from; whereas the people, when they govern, are generous toward the humbler public officers but envious of and niggardly toward the higher ranks. He might have found his explanation even better confirmed had he known that by the mid-twentieth century these income disparities were to decline very much further in France but change little in America; and had he also known that, of the Western countries, the British pay the highest and the Swiss the lowest salaries to their high civil servants (Table 1).

Tocqueville's political argument might be carried a little further. In aristocratic and feudal societies, great disparities between the salaries of high and not-so-high civil servants have also served to restrict entry to the profession and make it a preserve of the well-to-do. For the way to the high offices of State leads through the middle ranks; and if the pay in these is insufficient to enable officials to maintain the standard of living expected in their station, then the way to the higher ranks is blocked to all but those who at this early stage can supplement their pay out of private income. An example of this has been until not so long ago a branch of the civil service in Germany and other Central European countries—the academic profession. A university professor earned as high an income as the highest ranking civil servant; but he attained this income and the accompanying high prestige late in life, and until then held the position of *Privatdozent*, a title with no, or virtually no, income. Hence the need in those days and in those countries for a private income as a prerequisite for an academic career.

The political factors, however, tell only part of the story of civil service pay and are no help whatever in analyzing the earnings of the free professions. For a full explanation of the level and trend of professional incomes we need economic explanations as well. The most important of these, probably, is the increasing availability of higher education and its diminishing cost to the indivdual. This, too, has to do with democracy, of which it is one of the main aims and achievements; but it is democracy exerting its influence through economic channels. The

[1] Cf. Vol. II, Ch. 5, pp. 77-81 in the 2nd English edition (London 1836); p. 221 of the new French edition.

TABLE 1—CIVIL SERVICE SALARIES (IN NATIONAL CURRENCY AND AS MULTIPLES OF THE LOWEST SALARY)

Country	Office	1834 National Currency	Mult.	1875 National Currency	Mult.	1915 National Currency	Mult.	1955 National Currency	Mult.
United States	Under Secretary of the Treasury	—		6,000	7.1	—		17,500	5.4
	Chief Clerk	2,000	3.3	4,000	4.7	4,000	3.0	14,800	3.6
	Messenger	600	1	840	1	1,320	1	3,200	1
France	Directeur Général des Finances	20,000	13.3	25,000	14.6	25,000	12.8	2,490,000	5.1
	Huissier	1,500	1	1,700	1	1,950	1	488,000	1
United Kingdom	Permanent Head	—		1,920	32.0	1,950	23.5	4,623	15.9
	Sorter	—		60	1	83	1	291	1

Sources:

United States, 1834, A. de Tocqueville, *Democracy in America*, London, 1836; 1875 and 1915, U.S. Bureau of the Census, *Official Register of the United States*, 1955, U.S. Civil Service Commission, Form 490, August 1956, "The Pay Scales of the Classification Act of 1949, as amended, and Prior Pay Scales of the Classification Act of 1932, as amended." For 1955 the GS-18 classification was taken as equivalent to the chief clerk whose post had been abolished.

France: 1834, A. de Tocqueville, *op. cit.* (at this time the Directeur Général des Finances had the title of Secrétaire Général des Finances); 1871 and 1914, Statistique Générale de la France, Rapport . . . relatif aux échelles des traitements, remises et indemnités fixes des fonctionnaires, agents, sous-agents et ouvriers de l'Etat remunérés au mois. *Journal Officiel*, Annexes 1911, p. 1495; 1955, André Tiano, *Le Traitement des Fonctionnaires*, Paris 1957.

United Kingdom: 1876–1950, G. Routh, "Civil Service Pay, 1875–1950," *Economica*, August 1954.

increasing availability of higher education has increased the supply of people eligible for the professions and has thus tended to lower professional incomes (Table 2).[2]

TABLE 2—STUDENTS ENROLLED IN UNIVERSITIES PER 10,000 INHABITANTS

	United States (1)	Canada (2)	Great Britain (3)	France (4)	Germany (5)	Sweden (6)	Norway (7)	Denmark (8)
1900	31.3							
1909					10.1			
1910	38.6						9.0	
1911								
1913					12.9			
1914				10.5		11		17.4
1915								
1920	56.6					13	9.1	
1929		30			20.0			22.5
1930	89.4		13.3	17.7		16	16.8	25.2
1935	94.4	32			11.8	19		
1936				17.6				
1939	113.4			19.1			17.7	27.9
1940						18		
1941		32						33.0
1945								
1947		64		31.7		20		
1948	179.0							
1950	176.4				22.9		22.3	31.1
1951				33.0		25		
1952		45						
1953			19.8					
1954						29		
1955			19.7					
1956	154.4	45		36.1	26.5			28.9
1957							16.3	

(1) *Sources:* F. Edding, *Die Ausgaben fuer Schulen und Hochschulen im Wachstum der Wirtschaft*, Pt. II, Kiel 1957, p. 94; *Biennial Survey of Education in the United States*, "Statistics of Higher Education," 1957–58, p. 7. Coverage: "Institutions with courses creditable toward bachelors' or higher degrees," including less than 3 per cent "Technical Institutions and semiprofessional schools" such as teachers' training colleges, etc. Figures are for full- and part-time undergraduate and graduate students.

(2) *Sources:* F. Edding, *op. cit.*, p. 43; For later years: Canada, Bureau fédéral de statistique, *Apercu biennial sur l'enseignement au Canada*, Pt. II, Relevé de l'enseignement supérieur, 1950/52, pp. 56–57; 1956, Canada, Bureau of Statistics, *University and College Enrollment*, Fall 1955, p. 5. Coverage: Universities and colleges other than teachers' training colleges. According to the English version of the Biennial Survey used by Edding, figures are for full-time students throughout while its French version describes them as including part-time

[2] The increasing supply of trained and educated people is also attributed to technical progress increasing the *demand* for training and education. Neutral and uniform technical progress raises all earnings in proportion and hence raises the return on education, expressed as the (absolute) difference in earnings between the skilled and the unskilled. If education is among the uniformly progressing industries, the gap between the cost and return on education is widened, thus raising the demand for it [1, pp. 53ff.].

It is true that, hand-in-hand with the increasing availability of education, educational requirements for eligibility to the professions have also been raised, tending to restrict supply. Where of old anyone could hang out a shield and practice his profession, now diplomas, licenses, and many years of training are required. Mostly this represents a welcome protection of the public; in some cases the desire to raise incomes by restricting numbers has also played a part. Both factors have probably been present in medicine in the United States, where alone among the countries considered, the limited number and capacity of the medical schools and their strict admission requirements have reduced the ratio of physicians to total population over the past 60 years. In all other countries and all other professions considered in this study, the relative number of professional people has increased over time.

The importance of the supply of professional incomes is also suggested by an international comparison of these two quantities, shown in the scatter diagram of Figure 1. Along the vertical axis is measured the salary of a highly paid natural scientist, the head of the national meteorological service, expressed as a multiple of an office-messenger's pay; along the horizontal axis is measured a rough index of the supply of professional training, the number of students enrolled in universities and institutes of technology per 100,000 of population. The correlation

enrollments for the postgraduates; however, the possible error is small, postgraduate enrollment being roughly 5 per cent of undergraduate enrollment before 1952 and 7 per cent in 1952. For the 1956 figure, there is no indication whether part-time enrollment is included or not.

(3) *Sources:* F. Edding, *op. cit.*, p. 81; United Kingdom, Central Statistical Office, *Annual Abstract of Statistics* 1960 (No. 97), p. 100. Coverage: Full- and part-time university students taking courses.

(4) *Sources:* F. Edding, *op. cit.*, p. 35; Annuaire Statistique de la France, Rétrospectif, 1961, p. 67. Coverage: "Enseignement supérieur public, effectif des étudiants français et étrangers."

(5) *Sources:* 1956, *Statistisches Jahrbuch fuer die Bundesrepublik Deutschland*, 1957, p. 94; 1950, *ibid.*, 1952, pp. 70–71, 1935, *Statistisches Jahrbuch fuer das Deutsche Reich*, 1936, pp. 544 and 549; 1929, *ibid.*, 1930, pp. 454–60; 1913, *ibid.*, 1914, pp 316–21; 1909, *ibid.*, 1910, pp. 276–78. Coverage: Universities, "Technische Hochschulen" and other types of "Hochschulen," excluding teachers' training institutions. The 1956 and 1950 figures are for "Immatrikulierte Studenten," and the earlier figures for "Studierende" excluding "Hoerer and Hospitanten" which gives a fairly consistent time series.

(6) *Sources:* F. Edding, *op. cit.*, p. 59; 1947, Sweden, Statistisk Årsbok, 1949, pp. 275–76. Coverage: "Institutions of Higher Education" which means Universities and various types of "Högskolorna."

(7) *Sources:* 1957, Norway, *Statistisk Årbok*, 1959, p. 251; 1950 and 1939, *ibid.*, 1953, p. 254; 1930, *ibid.*, 1931–32, p. 163; 1920, *ibid.*, 1920, pp. 183–84; 1910, *ibid.*, 1911, p. 175 and *ibid.*, 1910, p. 146. Coverage: "Universities and Equivalent Institutions."

(8) *Sources:* 1956, Denmark, *Statistisk Årbog*, 1957, p. 235; 1950, *ibid.*, 1951, p. 223; 1945, *ibid.*, 1947, p. 180; 1940, *ibid.*, 1941, p. 165; 1935, *ibid.*, 1937, pp. 153–54; 1930, *ibid.*, 1933, pp. 132–33; 1915, *ibid.*, 1920, p. 174. Coverage: The tables were for "Students in Institutions of Higher Education, Senior Vocational Schools, etc." and some of the enrollment figures had to be subtracted out to obtain a coverage that seemed reasonably comparable to that in the other European countries considered.

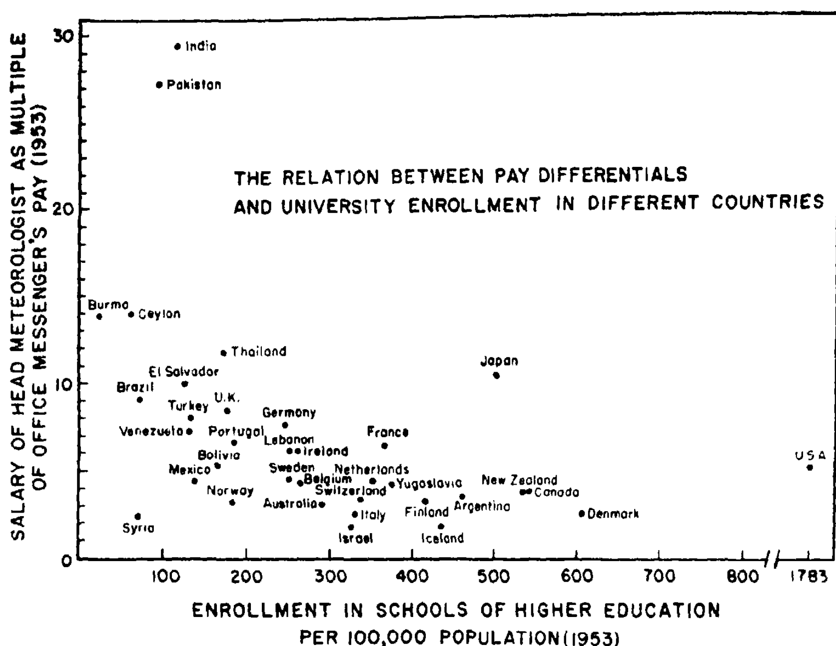


FIGURE 1

is striking and could go a long way toward explaining the very great national differences in salary differentials.[8]

Having discussed the influences on professional incomes from the supply side, we must now turn to those exerted on the side of demand. First, the rise in real incomes that results from economic growth tends to raise demand for most consumers' goods and services, including professional services. Second, the increasing complexity of our civilization has created a demand for a whole range of new professions and occupations that require extensive scientific and technical training and thus compete with the older professions for the available supply of highly educated and trained manpower. These two factors tend to boost professional incomes and thus to counteract the depressing effect of the increase in supply.

A third factor, but one likely to exert a downward pull on the demand for professional services, is the probable lag in the productivity of professional people. The rise in incomes tends to raise the demand

[8] On the other hand, the correlation could also be explained by the much greater international mobility of natural scientists than of office messengers, which would account for the much lesser absolute differences in the earnings of natural scientists. It seems likely that earnings in mobile occupations should be roughly the same in different countries, while in immobile occupations they should be quite different, higher in rich and lower in poor countries.

for goods and services because the prices of these rise either not at all or more slowly than incomes. The faster rise of earnings than of prices measures the rise in the productivity of labor; and unequal advances in the productivity of different industries and activities create changes in the structure of prices, in the pattern of demand, and also in the structure of incomes and earnings. In fields where the productivity of labor has made the greatest strides, product prices fall the most relatively to earnings and the prices of other products; in fields where labor productivity has not risen at all, product prices remain unchanged in relation to earnings in the same field but rise in relation to the prices of other products.

The productivity of professional people is notoriously very difficult to measure, because changes in the quality of professional services, in medicine for example, can be major and must be taken into account. In many professions, however, these changes cannot have been great, and productivity is likely to have advanced less than that of most other types of labor. If this is so, and if this trend has not been counterbalanced by increasing disparity in the length of the working week of professional people and others, then one would expect the prices of professional services to have risen relative to the prices of most other goods and services; and this relative rise to have discouraged demand for the services of professional people. This force, then, goes counter to the stimulating effect of rising real incomes on the demand for professional services.[4]

Yet another factor very different from those so far discussed but believed to have a great influence on professional incomes is inflation. The incomes of the professional groups here considered are determined to some extent by tradition. The members of these professions traditionally believe and like others to believe that they are rendering essential public service, the price of which is determined by factors other than their bargaining power in the market place. This tradition inhibits them and their professional organizations in seeking to maximize their incomes and explains why these respond sluggishly to market forces and may have a range of indeterminacy within which they do not re-

[4] One is unduly tempted in this connection to focus attention on the relation between the prices of professional services and the average level of money incomes, expecting demand for these services to rise if their prices fall and to diminish if their prices rise in relation to average incomes. The logic of this argument is best seen in the simple case of a parallel rise in money incomes and the prices of professional services. If the prices of other goods and services stand still or increase more slowly, *real* incomes will rise and stimulate the demand for professional services; on the other hand, the slower increase of other prices implies a rise also in the *relative* price of professional services, which discourages the demand for them. There is no reason to expect these contrary forces to offset each other exactly.

spond at all. Hence the belief that inflation lowers the real incomes of the professions.

This argument is probably valid at a time of small price increases or at the beginning of an inflationary period, and especially with respect to salaried professionals. The experience of more severe and more prolonged inflation, however, shows that market forces protect the interests of the so-called fixed-income earners as well as those of any other group. France's civil service (which includes teachers and professors) is protected no less effectively than organized labor by an escalator clause linked to the cost-of-living index; and in Chile, white-collar workers and the professions are supposed to be among the beneficiaries of inflation.

Prolonged inflation might nevertheless hurt the salaried professional groups whenever market forces are against them. For their salaries, once set, are not likely to be reduced in response to a fall in demand or rise in supply, and may remain at a higher level than the forces of supply and demand would justify. In such cases, an inflationary price rise may be necessary for market forces to exert their downward pressure on the real incomes of these groups. In other words, inflation can hurt fixed-income earners by *enabling* market forces to overcome the downward rigidity of salary scales.

While it is easy to list factors that influence the demand or supply of professional services, it is difficult to appraise their relative importance. All I could do and have done was to give data on the earnings and numbers of people in the free professions, in an attempt to furnish the coordinates of the point of intersection between supply and demand curves.[5] A third bit of information, the length of the working week, was not available; but a guess at least can be made. U. S. data for a part of the period suggest that the secular shortening of the working week was largely confined to workers and clerks. Businessmen, managers, officials, and professional people work a longer week and one that has not become appreciably shorter over the years. In relative terms, therefore, compared to the rest of the occupied population, the length of the professional person's working week has increased—and the trend was probably similar in the other countries too.

II

In an international comparison of this type, the main problem is to obtain comparable data. National differences in the availability, the

[5] No data are given on the number of people on university faculties and in the civil service, owing to the difficulty of obtaining comparable series on comparable definitions. The same difficulty explains why the income data are confined to only one rank in the hierarchy; and it seemed meaningless to give the number of persons in that single rank.

sources, the derivation, and the definition of the data were so great that an originally more ambitious project had to be cut down drastically. The number of countries, professions, detail, and types of data covered is much smaller than originally contemplated; and many compromises had to be made in presentation for the sake of better comparability. To compare professional earnings over time and between nations, it would have been desirable to express them as multiples of an over-all national average of personal incomes per employed population —we had to settle instead for multiples of *national* income per *occupied* population or labor force. Another interesting comparison would have been that between the absolute levels of professional real incomes in different countries; but official exchange rates would have been too crude and misleading, purchasing power equivalents were available only for a single year, so here, too, we had to settle on a mere fragment of what was originally contemplated, the few data contained in Tables 3 and 4. We also hoped to present data on all ranks of academic faculties and professional civil servants but the difficulties of international comparability proved insurmountable—hence the limitation of data to full professors and the highest ranking permanent civil servant.

Despite such compromises and narrowing of the scope of the study, full comparability of the data has not been achieved. The most important of the unresolved difficulties in this respect stems from differences in the provenance of the earnings data of the free professions. In the United States, estimates of the professional earnings of physicians, dentists, and lawyers are based on questionnaire surveys, with the re-

Table 3—Average Earnings of European Professionals as a Percentage of the Purchasing Power Equivalent of United States Professionals for 1955[a]
(European weights)

Country	Physicians	Dentists	Lawyers	Professors	Civil Servants	Per Capita Income
Denmark	43		62	*58*	39	49
United Kingdom	67	79	81	110	141	55
Norway	*36*		*67*	72	43	62, *51*
France	65	114		80	52	53
Germany	47	49	84	123	110	57

[a] Where 1955 data were unavailable, comparisons were based on 1950 data. These are in italics. Purchasing Power Parity rates were calculated from Tables 5 and 8 of Milton Gilbert, *Comparative National Products and Price Levels*, Paris 1958.

Earnings data are from the unpublished appendix tables. Earnings of free professionals in Denmark, Norway, and Germany have been increased by 20 per cent, in the United Kingdom by 10 per cent, for an estimated understatement of income.

The estimate for dentists' earnings in France is probably too high.

TABLE 4—AVERAGE EARNINGS OF EUROPEAN PROFESSIONALS AS A PERCENTAGE OF THE PURCHASING POWER EQUIVALENT OF UNITED STATES PROFESSIONALS FOR 1955[a]
(U.S. weights)

Country	Physicians	Dentists	Lawyers	Professors	Civil Servants	Per Capita Income
Denmark	34		42	46	31	39
United Kingdom	51	61	62	83	106	42
Norway	26		49	54	32	46/37
France	46	81		57	37	37
Germany	31	28	58	84	75	39

[a] The notes of Table 3 apply *pari passu* also to this table.

spondents assured of anonymity and guaranteed against disclosure o the information furnished. The French estimates are derived partl from tax, partly from other, data, reconciled and rendered comparabl by correcting for an estimated 33 per cent understatement of incom in tax returns. In the other five countries, the estimates are based o income tax statistics without correction for understatement and ar likely therefore to be too low—in a ratio presumably different for th different countries and perhaps also for different dates. For tax evasio probably increases with the burden of taxation, which is on the in- crease everywhere; and it is likely to be less important in a well-po- liced country like England, where payments into a physician's ban account are a source of information on his gross professional receipts

It seemed impossible to correct the data for such differences; bu their order of magnitude may be gauged by the discrepancy betwee U. S. income-tax and questionnaire-survey data in the few years fo which both are available.[6] The income tax data are consistently lower by about 18 per cent for lawyers and physicians and 13 per cent fo dentists. It is unlikely that the whole discrepancy is due to tax evasion For 1949, the Audit Control Program of the Internal Revenue Servic showed an average discrepancy between net profits "reported" an "disclosable by audit" of 4 per cent for lawyers, 6. 5 per cent for physi cians, and 7.5 per cent for dentists [3, Table 5].[7] While this is not a full measure of tax evasion, the earnings hidden even from audit woul hardly explain the rest of the discrepancy. A more careful and compre- hensive enumeration of deductions for tax purposes than for estimat-

[6] Net earnings of sole proprietorships and partnerships as shown in Schedule C of th income-tax return are available on a per capita basis for 1947 and 1953 and can b estimated with a small margin of error for a few other years for which the number o partners in partnerships is not available.

[7] The percentages in the text are based on the higher figure, while those in Table of the work just cited are based on the lower "net profits reported."

TABLE 5—PROFESSIONAL EARNINGS OF PHYSICIANS AS MULTIPLES OF PER CAPITA INCOME OF OCCUPIED POPULATION

Year	United States	United Kingdom	France	Germany	Canada	Denmark	Norway	Sweden
1911		5.4						
1913								7.2
1915						4.8		
1920								5.9
1921						3.9		
1927		(4.1)		3.7				
1929	2.9			4.3				
1930	3.2					(4.9)	3.9	4.2
1935	3.4			5.3				
1936–1938		(5.2)						
1938				4 6				
1939	3.2		3.3				(3.2)	
1940						3.1		
1947					2.9			
1948	3.2						2.2	
1950	3.3			2 7				
1951	3.2		3.9				(1.5)	
1952			4.3		2 6			
1954						3.0		
1956	3.8	4.3	4.8	2.6	2.9			
1958	4.2							

ing one's income for general information may be part of the explanation; furthermore, the questionnaire surveys, with their low response rates, are believed to have an upward bias, due partly to the reluctance of the less successful professionals to admit, even anonymously, their lack of success, partly to their being less likely to have the secretarial help or leisure needed for answering questionnaires.

In addition to the above derivational differences, there also remain in the data a number of definitional differences. Not only are statistics differently collected in different countries, there are international differences also in the way in which salaries are paid, pension funds set up, and professions defined and separated from neighboring professions. Each of these differences has created problems, none of them serious but many insoluble—at least with the limited resources available to this study.

The data on the earnings of the free professions refer, whenever possible, to independent practitioners only; but in some cases they are over-all averages for the entire profession, which includes independent practitioners and salaried employees. The discrepancies so introduced are probably slight. In the United States, where earnings data are available on both definitions for a part of the period, the earnings of independent professionals fall below in depression and exceed in pros-

perity the average earnings of the entire profession, by a negligible 1 per cent or less for dentists and lawyers, by up to 7 per cent for physicians.

Another problem, only imperfectly resolved, was to find a comparable rank in the civil service. The obvious choice, a cabinet minister, was ruled out by great national differences in the importance of fringe benefits and the difficulties of evaluating them. We adopted the rank immediately beneath him, which minimized these difficulties but at the cost of lesser international comparability. England's permanent undersecretary and Germany's *Staatssekretaer* were natural choices: they are the officials immediately below cabinet rank and at the same time also the highest-paid permanent civil servants. In France, the *secrétaire général* was an uneasy compromise; in the other countries, we have chosen the highest-paid officials outside the cabinet, even when they were political appointees.

International comparability remained an imperfectly resolved problem also in the case of professors. Many changes over time and great national differences in the ranks below the full professor forced us to omit them; but even at the professor's rank there are important differences between Europe and America. Professorial chairs are fixed in number and attained only by the select few at most European universities; in America the number of chairs is not limited and the professorship is a rank all faculty members can and most do attain in time. The data compensate for this difference in a crude way, because national averages of professorial earnings in all other countries are set beside U. S. averages that relate only to the 39 best and highest-paying institutions.

A further difficulty only partly resolved stems from the fact that civil servants—and this includes university professors on the Continent—are paid a fixed salary in some countries; in others their basic salary is augmented by rent, cost-of-living, and family allowances. In these latter cases we have taken, whenever possible, the earnings of a married man with two children, living in the capital.

In Europe, university professors usually receive extra pay for examinations. The difficulties of obtaining reliable and complete data on this extra pay forced us to exclude it from our data. Accordingly, the professional earnings of professors in the European countries are somewhat understated in the tables.[8] In Germany, professors also

[8] Examination fees may be quite an important source of income in some countries. In Germany, where the rector (president) receives a percentage of all examination fees paid at his university, there used to be a saying (before World War I) at the University of Berlin that a year's tour of duty as rector was worth a villa in Gruenewald, Berlin's most exclusive residential district.

receive *Kolleggelder*, fees for each student enrolled in their courses. Earnings from these used to be quite high in the days when all student fees went to the faculty. In the last decades of the last century, a popular professor could earn as much as 100,000 marks (about $100,000 in today's purchasing power), or 70 times the then-average income of the occupied population. This golden age of the German professor came to an end with legislation of 1898 (and of 1918) which gave him a fixed salary but diverted a part of student fees to the general upkeep of his university. But even today, *Kolleggelder*, the professor's share of student fees, amount to DM5.00 ($1.25) for each semester hour per student enrolled; and, with classes of a thousand or more not uncommon, a popular lecturer, or one whose courses are required, can easily double his basic salary [2].[9] Unfortunately, data from which to estimate average earnings from these fees are not available. Often the university guarantees a man's minimum proceeds from *Kolleggelder*; and we have taken the average of this minimum *Kolleggeldgarantie* as our estimate of the average of the actual *Kolleggelder*, although this must clearly be an underestimate.[10]

The earnings of civil servants and professors include the employee's and exclude the employer's contribution to pension funds;[11] and no adjustment was made for the fact that Germany deducts no employee contribution and pays pensions equal to a man's basic salary.

III

The basic data assembled for this study, the time series of net earnings in the different professions and of numbers in the free professions, are shown, separately for each country, in a series of appendix tables not printed here but available on request from the Institute of Business and Economic Research, University of California, Berkeley, California 94720. These tables also contain benchmark data on total and occupied population and national income. The exact definition of each series, the sources of the data, their nature, the way of their derivation, and the assumptions on whose basis some of them had to be estimated are all stated in considerable detail in the footnotes to these tables. This seemed desirable, because the best thing this paper can, and I hope will, accomplish is to draw attention to the existence of these

[9] This book and correspondence with its author were the main source of information about German university professors.

[10] The share of *Kolleggelder* in average earnings as shown for 1952 and 1956 (in the appendix table) is about 10 per cent.

[11] However, the government's contribution to physicians' and dentists' pension funds under Britain's National Health Service *was* included in professional earnings so as to render them comparable to physicians' and dentists' earnings in other countries, where the government does not subsidize their pension plans.

data and to stimulate and facilitate the task of a more ambitious undertaking to collect, render comparable, and publish the kind of data assembled here as a very modest one-man undertaking on an incomplete scale. Estimates made on an exceptionally rough basis or derived from such estimates are within parentheses in both these and all other tables.

The time trends of net professional earnings are shown in this paper as multiples of national income per head of occupation, separately for each occupation in Tables 5 through 9. They are based on the raw data and estimates contained in the unpublished appendix tables. Before looking at these, it is worth considering an international comparison in absolute terms. Tables 3 and 4 show the average net earnings of Europeans, expressed as a percentage of the purchasing-power equivalent of the earnings of their U.S. colleagues, for the year 1955, in a few cases for 1950 (the italicized figures in the tables)—the only years for which purchasing-power equivalents are available. Table 3 is based on European, Table 4 on U.S. expenditure patterns; and if it is true, as is sometimes asserted, that the expenditure pattern of professional people resembles more nearly that of the average European, then Table 3 is the more relevant of the two. Both of them show that it pays to be a high civil servant in England, a university professor in Germany, and a physician in the United States.[12] They also show that the economic

TABLE 6—PROFESSIONAL EARNINGS OF DENTISTS AS MULTIPLES OF PER CAPITA INCOME OF OCCUPIED POPULATION

Year	United States	United Kingdom	France	Germany	Canada	Denmark	Norway	Sweden
1914								
1922								5.9
1927		(3.5)		2.5				4.9
1929	2.4			2.7				
1930	2.7							
1935	2.3			2.8			2.4	3.7
1936–1938		(4.1)						
1938				2.6				
1939	2.4							
1947					2.3			2.8
1948	2.0							2.5
1950	2.0			2.1				
1952	2.5	3.8			1.8			2.3
1954								
1955	2.6							2.2
1956		3.3	5.5	1.9	2.1			
1958	2.8							

[12] The German professor's position is even more favorable than appears from the tables, because the data probably understate his income from *Kolleggelder*, take no account of his exceptionally generous pension plan, and exclude (as in the case of other European professors) his examination fees.

TABLE 7—PROFESSIONAL EARNINGS OF LAWYERS AS MULTIPLES OF PER CAPITA INCOME OF OCCUPIED POPULATION

Year	United States	United Kingdom	France	Germany	Canada	Denmark	Norway	Sweden
1911		(7.3)						
1915						5.8		
1921						(3.9)		
1927		(7.0)		6.3				
1929	3.1			5.8				
1930	3.4					(4.4)	3.9	3.9
1935	4.0			4.5				
1936–1938		(7.5)						
1938				4.4				
1939	3.3						3.0	
1940						3.5		
1947					3.0			
1948	2.3							
1950	2.2			2.8			2.2	
1952	2.1				2.2			
1954						2.6		
1956	2.3	3.2		2.9	2.6			
1958	2.4							

TABLE 8—PROFESSIONAL EARNINGS OF PROFESSORS AS MULTIPLES OF PER CAPITA INCOME OF OCCUPIED POPULATION

Year	United States	United Kingdom	France	Germany	Canada	Denmark	Norway	Sweden
1897				7.3				
1904	3.8							
1910	3.7						4.7	
1914			8.5					
1915						8.5		
1920	2.1						1.3	3.1
1921		3.8						
1927				5.6				
1929		5.4						
1930	3.7		8.9				3.7	4.2
1935	4.4				5.9	4.6		4.2
1936			8.6					
1936–1938		5.6						
1938				5.3				
1939	4.3		5.7				2.8	
1940						3.9		2.5
1941					3.0			
1945						2.7		
1947		3.8	3.8		2.3			1.7
1948	2.1						1.6	
1950	2.0			5.9		2.3		
1951			3.2					1.7
1952	1.9	3.8	3.1		1.8			
1955								2.4
1956	1.9	3.8	2.9	4.1	2.0			
1957							2.1	
1958	2.1							

advantage of the professional over the average man is decidedly greater in England than elsewhere, owing, probably, to the lesser availability in England of academic training and the much smaller proportion of academically trained people.

Another striking fact, shown by these tables and even more by Tables 5 through 9, is the contrast between the limited range within which the free professions', and the very great range over which civil servants', professional earnings vary over time as well as between different countries. A tentative explanation might be that market forces have and always had full scope in determining professional earnings; whereas setting the salaries of high public officials is substantially a political decision.[13] The very high economic status of high civil

TABLE 9—PROFESSIONAL EARNINGS OF CIVIL SERVANTS AS MULTIPLES OF PER CAPITA INCOME OF OCCUPIED POPULATION

Year	United States	United Kingdom	France	Germany	Canada	Denmark	Norway (a)	Norway (b)	Sweden
1900	7.8								
1910	6.1						5.3		
1911		17.8							
1913				14.8					
1914			10.3						
1915						7.1			
1920	3.2						1.4		4.4
1921		15.4				3.9			
1927				10.3					
1929		15.2		10.1	8.2				
1930	6.1		11.1				3.9	6.0	5.9
1935	8.2			10.6		5.2			6.0
1936			10.5		11.0				
1936–1938		15.2							
1938				7.8					
1939	7.1		7.3				2.9	4.5	
1940						4.4			
1941					6.4				3.7
1945						3.0			
1947		8.8	4.5		5.9				2.5
1948	2.8								
1950				8.4		2.5	2.1		
1951			4.0						2.1
1952	3.9	8.5	3.9		4.1				
1954								2.0	
1955			3.9			2.6			2.6
1956	3.5	8.9	3.7	6.7	4.1				
1957							2.1	2.3	
1958	4.1								

Column (a) refers to the earnings of the Ekspedisjonsjef, until 1930 Norway's highest ranking permanent civil servant. Column (b) shows the relative earnings of the Departementsrod, the new highest rank established in 1930.

[13] Market forces reassert themselves and force a raising of salary scales when these are set too low; they do not prevent their being set too high.

servants in England, Germany, and France is probably best explained by Tocqueville. In these countries, society seems to accord high civil servants a higher status than they enjoy elsewhere for reasons that probably lie in their political traditions and class structure.

Coming now to the shape of the time series in Tables 5 through 9, they confirm the general impression of a downward trend of professional earnings in relation to the over-all average of per capita national income. All the long series, i.e., all those that go back to before World War I, show a marked downward trend, with those for high civil servants registering the greatest decline. The shorter series show the same downward trend, with the important exception of physicians in the United States. Data on their average professional earnings go back only to 1929 and show a rise. How this series would look if it could be carried back to 1913 I do not know; but it is worth noting that the number of physicians per population in the United States has been declining at least since 1900, while in all the other countries it has been rising. The data on French physicians' earnings also suggest an upward trend; but here the data are too few, the period too short, and the estimates too crude to attach significance to this finding. In general, however, the downward trend of professional incomes is the least pronounced and least well established in the medical profession. Perhaps the great advance in medicine and the resulting greatly increased demand for physicians' services account for this; an important additional factor in the United States probably is the restriction of entry to the profession.

To explain the prevalence of the downward trend, a number of factors were listed at the beginning of this paper. Some of these, such as the increasing supply of education and educated people, would tend generally to mitigate inequalities of income; others, such as the slower than average rise in the productivity of professional people, would explain a fall in the relative earnings of professionals only. This raises the question to what extent the decline in the economic status of the professions is peculiar to them and to what extent it is part of a general trend toward lesser inequalities of income. Could it be that professional people have more or less maintained their relative position on the income scale and that the fall in their relative earnings merely reflects the general compression of the entire income scale? To answer this question, we would need to know much more than we now know. What scanty information is available on income distribution by size is summarized in Table 10 and does suggest a general reduction in inequalities of income over time. It is impossible, however, to attach much significance or even credence to such evidence, because this information is only available for two periods which are too close together and not very comparable, the first period being one of massive unemployment, the second one of full or overfull employment in most of the

TABLE 10—GINI INDEX OF INCOME INEQUALITY BY SIZE

Year	United States	United Kingdom	Germany	Sweden	Denmark
1935				.54	
1936	.47		.49		
1938		.43			
1939					.50
1945				.48	
1946	.41				
1949		.42			
1950	.40		.45		
1952					.44
1954				.38	
1955		.41			

Sources: Selma Goldsmith, G. Jaszi, H. Kaitz, and M. Liebenberg, "Size Distribution of Income Since the Mid-Thirties," *Rev. Econ. Stat.*, Feb., 1954. *Economic Survey of Europe in 1956*, Economic Commission for Europe, United Nations.

countries considered. Data on secular changes in the inequality of incomes within each of the professions would also be helpful; but they will have to be collected by a future and more ambitious project. I very much hope that this paper will facilitate its coming.

REFERENCES

1. G. S. BECKER, *Human Capital.* New York 1964.
2. A. BUSCH, *Die Geschichte des Privatdozenten.* Stuttgart 1959.
3. M. FARIOLETTI, "Some Income Adjustment Results from the 1949 Audit Control Program," Conference on Research in Income and Wealth, *Studies in Income and Wealth,* Vol. 23, *An Appraisal of the 1950 Census Income Data,* Princeton 1958, pp. 239–84.
4. J. F. HABERER AND F. L. KIM, "The Economic Position of Canadian Scientists and Engineers," *Canadian Mining and Metal. Bull.*, Oct. 1956, 721–25.
5. G. WOLFF, "Der Anteil der freien Berufe am Sozialprodukt," *Das Geistige Kapital,* March 1956, 34–36.

THE DOUBLE DEVELOPMENTAL SQUEEZE ON AGRICULTURE

By WYN F. OWEN*

All genuine development reform movements have to come to grips with the reality of poverty in the conditions of economic underdevelopment. Because it is so obvious, it is almost unnecessary to state that in underdeveloped countries the majority of producer-consumers are poor and not rich. However, one of the major implications of this fact does bear restating and further consideration. This is, namely, that in the economically underdeveloped countries any substantial sacrificing of present consumption opportunities, in the interest of higher rates of investment and of corresponding future consumption possibilities, must necessarily fall primarily on the shoulders of the poor. One recent writer has stated this universal law of development, as follows: "In no society have the monied group alone had enough resources to set in motion the really massive savings that underlies the transformation of a static into a dynamic economy . . . the surplus [has] to come from savings imposed on the mass of the people" [59, p. 26].

However, the reality of poverty also comprises a further fact which is rarely accorded the attention it deserves, even though it, also, is quite obvious. This is that the poor of the world predominantly are peasants. The single most important question in development policy in the poorer countries, thereby, turns out to be the way in which the peasants are involved in the accumulation process. Peasants are strategically important not only because of their numbers, but also because they produce one of the most important material inputs associated with the process of capital accumulation, namely, the food and fibers necessary to sustain any labor which is diverted to any other form of economic activity. Beyond this, they also represent the prime immediate source of foreign exchange earnings in many underdeveloped countries. Restated, therefore, the fundamental dual question is: how can peasants be encouraged to produce a cumulative surplus of food and fibers over and above their own consumption, and how can this surplus largely be channeled to investment activity in the nonfarm sector with-

* The author is professor of economics at the University of Colorado and director of the Economics Institute.

out requiring in exchange an equivalent transfer of productive value to the farm sector?[1]

In reality, all conditions of economic progress inevitably must incorporate an operational answer to this question, and no genuine development reform movement can avoid it. As has been stated elsewhere "a squeeze on agriculture" seems to be a feature "of all developing societies, whether socialist or captalist" [5, p. 31].[2] This is to imply that the difference between the Russian and the U.S. approaches to development lies not in the fact that one exacted or exacts a special contribution from the farmers and the other did, or does, not. Rather the difference lies in the way in which the squeeze has been applied and in the relative efficiency with which the process has operated in each case.

Herein, an attempt is made to examine the differences between these two principal approaches to this essential condition for development. In doing so it will be helpful to differentiate between what might be characterized, respectively, as, first, "The Production Squeeze on Agriculture," and, secondly, "The Expenditure Squeeze on Agriculture." Under the first will be identified intersectoral "requisitioning" of increments to farm production and, under the second, the "requisitioning" of residual farm income for essentially nonfarm-sector, or general social, purposes. The general rule, it will be argued, has been for the impact of this "double squeeze" on agriculture to be positive in both directions in all developing countries. As a result, the cumulative relative saving-investment advantage provided the nonfarm sector has, in all cases, been an important element in the development process.[3]

In the case of the "farm production squeeze" the respective approaches followed by the leading communist and the noncommunist countries have been quite different, and the first objective will be to

[1] Among other recent statements particularly relevant to this question, those of Kuznets [31], Nicholls [44], and Fei and Ranis [10] deserve special mention.

[2] Reference to the "squeeze on agriculture" also can be found in much of the classical literature of economic analysis. For example, the phenomenon was referred to by Adam Smith, as follows: "In every country in Europe we find, at least, a hundred people who have acquired great fortunes from small beginnings by trade and manufactures, the industry which properly belongs to towns, for one who has done so by that which properly belongs to the country, the raising of rude produce by the improvement and cultivation of land. Industry, therefore, must be better rewarded, the wages of labor and the profits of stock must evidently be greater in the one situation than in the other" [53, p. 113]. Karl Marx, in a similar vein, stated: "While the country exploits the town politically . . . wherever feudalism has not been broken down by an exceptional development of the towns, the town on the other hand, everywhere and without exception, exploits the land" [39, p. 940].

[3] For purposes of this analysis "the farm sector" is defined in the traditional way to include only that part of the process of production of farm commodities that, at any point in time, remains within the boundaries of the farm fence. The word "agriculture" is used interchangeably with "farm sector."

identify and contrast these two models. This is particularly warranted because, while the noncommunist model is quite simple and has had quite spectacular results, it, nonetheless, is not widely understood; leaders of developing countries who might wish to emulate noncommunist procedures have never been provided a very clear-cut prescription of what these really are. After attempting to meet this need, the major components of the "expenditure squeeze on agriculture" in the process of economic growth is examined further. This latter phenomenon, it is shown, assumes the same general form in all countries, whether Marxist or non-Marxist, even though it is subject to different degrees and qualities of exploitation within each of these ideological circumstances.

The final section of the article questions the relative degree of effectiveness with which the product of the double squeeze on agriculture has been put to developmental purposes in the recipient nonfarm sectors of countries falling under the respective influences of the mainstream and the Marxist schools of economic thought. Therein, it is suggested that countries following a noncommunist pattern of development generally have fallen far short of realizing the full potentialities for internal capital formation and economic growth that are inherent in this general pattern. This, if true, has obvious implications for those leaders of the poorer countries who genuinely aspire to higher rates of growth in their countries than those characteristic of most noncommunist countries in the past.

I. *The Production Squeeze on Agriculture*

A. *The Basic Communist or "Marx-Leninist" Model*

In relation to the farm output "squeeze on agriculture" the essentials of the communist model are widely understood. However, a brief reiteration of them here will serve as an appropriate background for a contrasting identification of the main noncommunist model. As exemplified by Russian development,[4] the communist approach has involved a threefold attack on the problem of creating and appropriating an adequate agricultural surplus. First, direct state intervention—through the development, organization, and operation of farming on the basis of state farms, collective farms, and until recently machine tractor stations—has been relied upon as the essential means of promoting increased agricultural production. The primary objective has been the exploitation of anticipated economies of scale and technology, as well as of direct labor supervision and central planning. Secondly,

[4] Russia is assumed to represent the most orthodox Marxist economy. There have, of course, been important deviations in the ways in which other professing Marxist countries have attempted to create an adequate agricultural surplus.

the first claim on a substantial part of total farm output has been imposed in behalf of the nonagricultural sector through the medium of required deliveries of allocated quotas of agricultural produce to state procurement agencies at nominal prices. This policy also has been supplemented by administered prices for the majority of commodities marketed outside the quotas. Thirdly, a purposive utilization of the acquired agricultural surplus value has been sought in the development-oriented rationing of much of the resulting supply of food and fibers within the urban-industrial sector through the medium of state controlled or sponsored retail stores, service establishments, and consumer cooperatives, and through related wage and price controls [43, Ch. 7] [46] [58] [4].

Disregarding the arbitrary mistreatment of human beings involved in the application of these methods, the Soviet system has proved to be relatively successful in regard to the second and third objectives. Although the working margin, especially in recent years, has been uncomfortably narrow, at least to date, a substantial and a sufficiently regular supply of farm products has been extracted from Russian agriculture to support an expanding urban-industrial community and an extremely impressive industrial growth rate [47, p. 289]. The peasant, in these circumstances, as the residual claimant on agricultural output, has been a primary risk bearer and primary involuntary saver. In regard to the first objective, however, namely, the stimulation of increasing productivity in agriculture, the results have been much less impressive. Agricultural production in Russia fell substantially following the revolution, and had the communist leaders not been able to draw upon the previous export surplus of farm products to meet domestic needs, this decided agricultural developmental failure would have had much more serious implications. In spite of the impact of several "crash programs" in agriculture in recent years, the record in this sector still remains an obvious cause for embarrassment and internal conflict among the Russian communist leadership [18] [52]. The general stalemate, and in some instances, such as in Poland and Yugoslavia, the actual reversal of collectivization plans for agriculture in communist-dominated countries of Eastern Europe provide further evidence of the general communist dilemma with regard to the agricultural sector. Clearly, in China, where population pressure on the land and an initial food deficiency leaves an even smaller margin for error in the programming of agricultural development, the question of whether or not to follow the Russian prescription in this area is one that the indigenous communist leaders also can avoid raising in that country only in blind ideological defiance of experience and of their own interests.

B. *The Contrasting Major Noncommunist or "Mill-Marshallian" Model*

The analogous approach, which in varying degrees of perfection has been common to the development record of all of the major noncommunist countries, appropriately might be referred to as the "Mill-Marshallian" model, since the major insights upon which it stands were apparent in their essentials to these contemporaries of Marx and Lenin. The model has two primary features. The first of these is the organization of agricultural production activities in family *operational* units;[5] that is, in the form of the individual proprietorship type of economic organization, in contrast to those other "more advanced" forms of organization—in particular the private and public corporation or cooperative—which have assumed such an important position in the nonfarm sectors of economically advanced countries. The model requires that this type of atomistic organization of the farm sector—comprising a large number of relatively small independent production units related to one another in a manner closely approximating the economists' conception of "pure competition"—persist, even though the nonfarm sector acquires increasingly generalized degrees of concentration and of monopolistic tendencies.

The second essential feature of the model is that the farm sector, thus organized in its internal nature, also be oriented substantially, and increasingly so, to production for the purpose of acquisition through exchange of nonfarm-produced factors, commodities and services, in contrast to the traditional orientation of a subsistence economy, namely, production for the direct satisfaction of the producer's economic purposes. The major implications of each of these two essential features of the Mill-Marshallian model—or of what equally well can be referred to as "a market-oriented, family farming system"—will be discussed in turn in some further detail. Illustrative materials will be drawn mainly from the record of the United States since this country, undoubtedly, has achieved the highest level of development of this particular model. However, in a more comprehensive treatment corresponding examples could be cited from the records of such countries as Great Britain, Canada, Australia, Japan, most Western European countries, and many others.

Economies of Scale. In the industrial-urban sector historical evidence and economic analysis overwhelmingly support the assumption

[5] It is not essential, although in many circumstances and stages of development it may be desirable, for the operational unit also to be owned by the operator. It also is emphasized that it is not meant to imply that the model necessarily should be assumed to be relevant to the process of prefarming land development as well as to that of farming itself.

that economic progress and relatively large-scale production, as exemplified by the factory system, go hand in hand. Yet, whether we consider the case of the United States or of any of the other well-developed countries of the noncommunist world, the fundamental and common reality in the basic organization of agricultural production is the family farm. This fact, together with the impressive productivity record of the agricultural sectors of most of these countries, has always presented somewhat of a contradiction in the context of conventional Western economic thought. That the contradiction troubled Alfred Marshall is very apparent in his *footnote* expression of the hope that someday someone might discover the secret of the factory farm.[6] However, Marshall was realistic enough to recognize that the factory system could not easily be applied to agriculture, and in general he seemed to conclude it best to consider agriculture as "a case apart." It is probably fair to say that most economists have since attempted to resolve his dilemma by avoiding it. That is, the main body of economic doctrine in the West emerged in a form that most properly should be referred to as "urban-industrial economics." "Rural-agricultural economics" was left to develop, separately and belatedly, a largely independent and peripheral academic career of its own.

In this setting it followed that the main body of economic thought had only a minor influence on the development and retention of the type of production organization in agriculture which typically became established in most of the more advanced noncommunist countries. The family farm was more a product of physical and political realities[7] than a response to any particular economic doctrine, even though it is true that the implied benefits of classical competition have often been

[6] "The experiment of working farms on a very large scale is difficult and expensive, because it requires farm buildings and means of communication specially adapted to it; and it may have to overcome a good deal of resistance from custom and sentiment not altogether of an unhealthy kind. The risk also would be great; for in such cases those who pioneer often fail, though their route when well trodden may be found to be easiest and best.

"Our knowledge on many disputed points would be much increased and valuable guidance gained for the future if some private persons, or joint-stock companies, or cooperative associations, would make a few careful experiments of what have been called 'Factory farms' " [38, p. 652].

[7] Undoubtedly the primary influences were of a geographical and demographical nature with such political philosophies as the Jeffersonian concept of rural democracy playing complementary roles. Only in these terms can the essentially common patterns of settlement on the frontiers of the United States, Canada, New Zealand, Australia, South Africa, and of several other parts of the world be reconciled one with the other. As another writer [35, p. 360] has put it, "it was largely a happy circumstance that the technical aspects of agriculture coincided with the economic, social and political framework which he [Jefferson] envisaged."

drawn upon for subsequent arguments in its defense. On the other hand, Marxist-Leninist economics and the attempted method of agricultural organization in professing communist countries have had very close connections. Lenin confidently associated himself with Kautsky's conclusion that "the superiority of large-scale farming over small farming even in regard to cooperative organization is fully confirmed" [34, p. 162]. Thereby the collectives and, even more so, the state farms of the USSR became logical, practical expressions of the basic Marxian doctrine.

In marked contrast to Marx and Lenin, John Stuart Mill, after very careful deliberation, earlier had concluded that "the superiority of the large scale system in agriculture is by no means as clearly established as in manufacturing" [41, p. 144]. Likewise, Alfred Marshall, though ever hoping for evidence to the contrary, felt that the only honest conclusion to be drawn was that "the economies of production on a large scale are not quite similar in the two cases," and that agriculture, "even under the English system, cannot move fast in the direction of the methods of manufacturing" [38, p. 651].

Quite clearly Mill's position, as the classical school's champion of the family farm, was largely determined by his insight into the implications of the second limitation to the division of labor, namely, the nature of employment.[8] The key phrase in Mill's statement is the assertion that the different operations of agricultural production "cannot possibly be simultaneous." Rather, they must be performed sequentially.[9] In other words, it is impossible to sow the seed and gather the grain at the same time, while it is possible for the different components of an automobile to be manufactured simultaneously with one another and with the assembly of the final product. Add to this the closely related spatial limitations to the increasing scale of operations in agriculture, explained, among others, by Marshall [38, pp. 290,

[8] The first limitation, of course, was the extent of the market. With regard to the nature of employment in agriculture, Mill wrote: "Agriculture . . . is not susceptible of so great a division of occupations as many branches of manufactures, because its different operations cannot possibly be simultaneous. One man cannot be always ploughing, another sowing, and another reaping. A workman who only practised one agricultural operation would be idle eleven months of the year. The same person may perform them all in succession, and have, in most climates, a considerable amount of unoccupied time. To execute a great agricultural improvement, it is often necessary that many labourers should work together; but in general, except the few whose business is superintendence, they all work in the same manner. A canal or a railway embankment cannot be made without a combination of many labourers; but they are all excavators, except the engineers and a few clerks" [41, pp. 131-32].

[9] This insight of Mill has been developed in an important recent article by John M. Brewster [6].

651]—the inevitability that increasing size of farms will result in the need to send the workmen "over the hill" and beyond the sight and supervision of the operator—and the essential point becomes quite obvious: the factory, meaning that type of large-scale enterprise which is characterized by a large payroll, is, except under quite special conditions,[10] incompatible with the nature of the task of agricultural production.

To seek to impose the factory upon agriculture is, therefore, to defy some rather convincing facts of life. Western countries in general have not deliberately tried to do so, and this quite clearly has been to their advantage in economic development. The continuously evolving family farming system of agriculture in the United States not only remains unsurpassed in productive efficiency by any differently organized agricultural industry anywhere else in the world, but its record also compares very favorably, and increasingly so, with that of the nonfarm economy. For example, one study reports an average annual rate of increase in total factor productivity in the United States between 1889 and 1957 (a 68-year period) of 1.3 per cent in the farm sector compared with 1.8 per cent in the nonfarm sector, but with the rate of increase in the farm sector during the final decades of 1937-48 and 1948-57—namely, 2.7 per cent and 3.7 per cent, respectively—considerably exceeding the 2.0 per cent rate in both decades in the nonfarm sector [28, p. 1560].

It is interesting to note, further, that in the United States, where the possibilities of establishing factory farming are as open to experimental test as any other form of agricultural economic organization, there really has not yet emerged convincing evidence in support of the contention recently voiced by Higbee [25, pp. 77-94] to the effect that the factory farm, or *hacienda*, is in process of generally displacing the family farm at the operational level. Following the English pattern there is likely to develop in the United States an increasing concentration in farm land and farm capital ownership and control, embracing many farm operational units in a single land or capital estate, but among farm operational units, or farm firms, large-scale family farming in the United States remains fully competitive in all of the major lines of agricultural production. Thus according to one recent study there was, between 1949 and 1959, an actual decline of 24 per cent in

[10] In practice the most common "special conditions" have been those incorporating a large supply of unskilled and subservient labor—such as the plantation South and migrant labor regions of the United States and the *latifundia* of South America—and those, normally atypical, types of farming which involve a high spatial concentration of labor-intensive productive activity, such as specialized fruit and vegetable farming, irrigated agriculture, and specialized livestock feeding.

the numbers of larger-than-family farms[11] marketing more than $10,000 worth of farm commodities, while at the same time family farms in the same category increased in number by 104 per cent [29, p. 630].[12]

Market Orientation. It can be concluded, therefore, that it is still much more realistic for developing countries to pursue the potentialities for technological advance inherent in the old-fashioned family farming structure than the imaginary economies of the larger-than-family or "factory" farms. The development record of Japan clearly attests to this truth [21, p. 767]. However, and equally important, is the second essential component of the Mill-Marshallian model, namely, a meaningful degree of market orientation in the economic activities of the family farmer. That is, the farmer's standard of living and his production activities must be significantly dependent upon goods and services and productive resources which he acquires through exchange rather than as a direct result of his own productive efforts.[13]

One of the primary functions of such market orientation in the operation of the model is to provide for an automatic realization of the productive potentialities which are inherent in any given state of the agricultural arts, and this within whatever limits are imposed on the latter by the demographically practicable pattern of family-farm sizes. The mechanism whereby, given a market-oriented family farming structure in the agricultural sector, technological progress and increasing productivity in farming are rendered essentially automatic has been well described in almost all elementary textbooks of economics. The commercial family farmer under the dynamics of a traditional competitive environment represents an individual Ricardian capitalist *par excellence,* forever reaching forward for new technologies in order to keep from losing position on that reverse escalator of declining per unit average real costs, which is a characteristic of any industry that is making a positive contribution to general economic progress. In contrast to the related types of opportunities which are open to many of his principal counterparts in the nonfarm economy, no farmer-manager, of his own volition, can exclude or retard his competitors—other farmers—also

[11] That is, farms on which hired labor exceeds the labor contributed by the operating family. Since the average farm family in the United States contributes the equivalent of one and one-half man-year units of labor, this implies that on the average, family farms, by this definition, normally would not provide employment for more than a total of three full-time equivalent units of labor including that of the operating family.

[12] For further support of the general position that the efficient family farm is not seriously threatened by the factory farm, see H. L. Stewart [54] and M. Harris [19].

[13] The trading partners of such an interdependent family farmer necessarily will include other farmers as well as nonfarm-sector producers, although it is the latter with which this article is primarily concerned.

from adopting and fully exploiting at will any new methods or technologies that arise in the industry, and quite irrespective of the source of the innovation. The best he can hope to achieve is to keep in the lead. Should he become a victim of, rather than a victor over, the circumstances, he more likely than not will be faced with little else than the equally unattractive economic alternatives of descending into the lost world of noncommercial or low-income "farmers," or of drifting into the ranks of their city cousins—the urban unemployables and slum dwellers.

In reality, in most countries in which the Mill-Marshallian model has played an important role in the process of development, only a very imperfect application of the model has really been achieved. Thus, the common characteristic of family farms in most countries is that too many of them tend to be too small to provide for a full realization of their productive potentialities,[14] and in far too many instances established family farming systems have remained substantially, if not primarily, oriented to subsistence economics, rather than becoming an integral part of a national exchange economy. The record of the rural economy of France following the Revolution provides an interesting case in point [40]. Nevertheless, while its limits have nowhere been reached, it is apparent that the model has been quite highly developed in several countries and especially in the United States. Thus, in 1959, 94 per cent of all commercial farms[15] in the United States were family farms according to the preceding definition. Furthermore, farm families by 1960 had become market-oriented to the extent that only 9.5 per cent of their earnings from farm sources comprised directly consumed farm products, including the rental value of farm dwellings, (cf. 1910-14, 23.7 per cent), and 71 per cent of the inputs of commercial agriculture by market value were supplied from off-farm sources (cf. 1919, 53 per cent). Mobility of resources in response to changing market conditions in the United States also had become sufficient in the two decades ending 1960 to accommodate a decline in the farm labor force by 39 per cent, and in the total numbers of farms by 25 per cent [69, pp. 40, 44, 46]. Correspondingly, there was an increase in the average acreage of farms by 65 per cent, and due to a greatly expanded commitment of capital resources,[16] an even

[14] Thus, even in the United States, informed observers claim that (in 1959) family farms of adequate size represent only about 18 per cent of all farms and only about 28 per cent of all commercial farms [19, p. 540].

[15] That is, farms on which the operator worked off the farm for less than 100 days a year, and on which income from farm sources provided more than one-half of the total income of the farm household.

[16] Between 1940 and 1960 total farm usage of machinery and motor vehicles increased by 138 per cent; farm real estate (in spite of a decline in cultivated land) by 13.6 per cent, and total farm livestock by 3.2 per cent [65, p. 3].

greater increase in the total sizes of family farms as business concerns.[17]

"Priming the Pump." The tendency for an atomistic market-oriented agriculture automatically to devour compatible new technologies can be and has been widely exploited in many countries through the adoption of policies designed to accelerate the process. The speed of this "agricultural treadmill" [9, pp. 85-107], which a family farming system inherits along with market opportunity, quite obviously tends mainly to be conditioned by the degree to which knowledge and access to new methods and techniques are generally available to farmers. Thus, the Mill-Marshallian model provides a framework within which increased agricultural production, on a cumulative basis, can be rendered essentially automatic and almost a direct function of public investment in agricultural research and rural education and of the adequacy of complementary rural financial institutions and farmer's financial reserves. It is to be noted in this last connection that, insofar as farm investments are largely self-financed, the subsidization of farm incomes through price support or other methods can be viewed as an alternative approach to the same objective as that served by the improvement of rural financial institutions. That is, farm income supports can represent an important element in the pump-priming process.

The record of the United States may also be cited as evidence of the potentialities inherent in such pump-priming devices, although it should be noted that the United States, in this respect also, has by no means fully exploited the possibilities. In spite of the natural obstacles to research and innovation which are presented by the structure of agriculture—especially its geographical spread and the fragmentation of the managerial and investment functions—and in spite of the related benefits often cited in behalf of large-scale production in industry [12, Ch. 2], there is convincing evidence that in the United States over the period 1909-49 "technical change in agriculture was twice as rapid as in manufacturing" [33, p. 950].

The "Pay-Off." However, there is still a further important question to examine. This concerns the degree to which this model also provides that at least a substantial part of the realized increments in agricultur-

[17] In 1960 the value of total physical farm assets in the United States amounted to $34,648 per farm, which in constant dollar amounts represented an increase of 82 per cent over the corresponding figures for 1940 [65, p. 19]. As an integral part of the process of market orientation and economic interdependence, but still within the context of a family farming system, increasing economies of scale thus have also played a substantial role in the spectacular rate of growth which has characterized U.S. agriculture in recent times. During the period 1940-60 total farm-sector output in the United States increased by approximately 50 per cent [16, p. 341]—and, of this, Griliches estimated that about 40 per cent was "due to the expansion that occurred in the scale of the average farm enterprise" [16, p. 346].

al productivity is diverted to the potential support of economic development in the nonfarm sector in contrast to its being dissipated in immediate farm-sector consumption activities. Compulsory deliveries of food under the Marx-Leninist approach clearly represent a direct tax on the farm sector in behalf of the nonfarm sector. It is not so obvious that a market-oriented family farming system also provides an extremely efficient means to such intersectoral taxation. The form of the tax and its method of collection are quite different than under the Marx-Leninist model, but that it exists cannot be denied.[18] As already stated, its existence constitutes a developmental imperative.

Simply stated, the gain to the nonfarm sector, under the Mill-Marshallian model, takes the form of an intersectoral profit on technological progress. It represents a residual surplus accruing to the nonfarm sector on the rewards of economic development in the farm sector, over and above any equivalent sharing by the farm sector in the corresponding gains from technological progress generated in the rest of the economy.[19] In place of the delivery of a quota of farm production to the "peoples' government" under the political compulsion of commmunism, the market-oriented, family farming system delivers to the nonfarm sector, under the compulsion of the competitive market, progressively increasing supplies of food at progressively lower costs. This is to say that, under these circumstances, the benefits of technological progress in agriculture are automatically and rapidly passed on to nonfarm processors and consumers as the cumulative impact of innovating farmers increases the total market supply of the farm commodities in question and produces a corresponding fall in the average market price [60, pp. 467-86]. This process in turn rapidly erodes away the ephemeral profits of that select minority of farmers who constitute the vanguard in respect of any particular changing technological circumstance in the farm sector.

What is so unique about this? Is it not true that the same kind of sequence of events also applies to the process of technological advance in the nonfarm sector? That is, does not the adoption of innovations in

[18] This general proposition has also been presented by B. H. Johnston [27] and by P. M. Raup [49]. To quote Raup: "The similarities between the effects of the ration system and those of the pioneer development phase in North America should not be overlooked. . . . However dissimilar the political and social climates in the two areas may be, the economic process has been roughly comparable. . . . Whatever the motivational structure, whatever the formal name attaching to the extraction system, the consequence has been the same: a tax, in the broad sense of the term, has been laid on agriculture for the provision of capital needed in industrial growth" [49, pp. 316, 317].

[19] This proposition plays a central role in a recent analysis of current United States agricultural policy by E. O. Heady [22] who states that: "The distribution of gains and losses of technological and economic progress provides the main basis for the policy problems of commercial agriculture" [22, p. 10].

the nonfarm sector likewise tend to be translated into a gain to the consumer in the form of lower real prices and, furthermore, does not the farmer as a consumer in this way get repaid for his contribution to total economic growth? In principle the answer to these questions must, of course, be "yes." However, it is also true that, when viewed in a dynamic sense, the relative total impact of the same general process on the two sectors can be quite different if there are differing overall time lags between the adoption of innovations by the component-producing groups and the realization of cheaper commodities and services by the consumer. The presence of significant degrees of natural and acquired monopoly power in the hands of a large proportion of nonfarm producers and of associated industry-wide positive profit rates means none other than that these producers do have the power in varying degrees to delay the transfer of the benefits of technological progress to the consumer; in other words, they have the power consciously to lay claim for a somewhat longer period of time to the profits which everywhere accrue to the innovator or first adopter. In the farm sector, on the other hand, competitive conditions insure that the corresponding time lag and the cumulative aggregate rate of profit accruing to the farm sector on its own innovations is kept to the absolute minimum.[20]

In the dynamics of growth this phenomenon gives the nonfarm sector, under the conditions of the Mill-Marshallian model, a perpetual advantage in respect of its claim on the total income stream vis-à-vis the farm sector, which really amounts to a dynamic form of intersectoral taxation expertly administered by the "invisible hand." It deserves to be noted in this general connection that Adam Smith's reference to the squeeze on agriculture was conditioned by his belief that there tended to exist a different and higher plateau of prices in the towns than in the country and that this placed the latter at a substantial and cumulative relative disadvantage in a market economy. He viewed the phenomenon itself to be a natural outcome of the inherent tendency toward combination under urban conditions and the inherent opposite tendency under rural conditions.[21]

[20] "The first few farmers who adopt innovations do realize a positive pay-off. The masses who follow in adopting innovations and augmenting the supply function, however, are the ones who make the greatest absolute contribution to lessening the real price of food and to freeing resources from agriculture. Yet these producers are promised negative pay-offs or costs from the contribution, because their incomes are reduced from the process under inelastic demand" [22, p. 10]. In the nonfarm sector the "first few" invariably represent a sizable majority rather than the minority.

[21] In Adam Smith's words: "The inhabitants of a town, being collected together into one place, can easily combine . . . it was the manifest interest of every particular class of them to prevent the market from being over-stocked, as they commonly express it, with their own particular species of industry; which is in reality to keep it always under-

How Much Benefit? Having recognized the phenomenon, the logical question to ask next is: What is the magnitude of this *dynamic intersectoral concealed and cumulative tax* on agriculture in countries, such as the United States, where the Mill-Marshallian model applies? Due to the nature of the phenomenon this question does not lend itself to precise answer. However, it is reasonable to assume that a part of the pronounced income differential which exists in most countries between the farm and nonfarm populations,[22] and especially the failure of this differential to close over time, is subject to explanation in these terms. It also is apparent on the basis of several indicators that, even in a country as highly developed as the United States, this intersectoral tax still represents a very significant flow of funds.

To mention one such indicator, it is instructive to consider the implication of placing the farm sector on a more nearly equivalent basis to the nonfarm sector with respect to aggregate industry profit balances or, in other words, to aggregate rewards for innovation and risk-taking. As already noted, a negative aggregate profit balance, without doubt, is the "normal" condition in the farm sector. By contrast, profit margins, after taxes, of all manufacturing in the United States throughout the 1950's amounted to between 4 and 5 per cent of sales [63]. Even more impressive, the aggregate profits of the larger nonfarm corporate sector, also after taxes, ranged from $16.8 billion to $24.5 billion per annum over the decade of the 1950's, with the average (based on five years) increasing from an annual rate of $16.5 billion in 1950 to $22.3 billion in 1960 [64, p. 281]. Had the farm sector experienced a supplementary return proportional to the profit rates of these two components of the nonfarm sector, this additional return would have increased the net farm income in 1960 by between $1½ and $2 billion.

stocked. Each class was eager to establish regulations proper for this purpose, and, provided it was allowed to do so, was willing to consent that every other class should do the same. In consequence of such regulations, indeed, each class was obliged to buy the goods they had occasion for from every other within the town, somewhat dearer than they otherwise might have done. But in recompense, they were enabled to sell their own just as much dearer, so that so far it was as broad as long, as they say; and in the dealings of the different classes within the town with one another none of them were losers by these regulations. But in their dealings with the country they were all great gainers; and in these latter dealings consists the whole trade which supports and enriches every town. . . . The industry of the town becomes more, and that of the country less advantageous" [53, pp. 108-17].

[22] Clark [8] and Bellerby [2] provide relevant comparative data on this point. For the United States, it might be noted that, based on disposable income figures, it would have taken $14,384 million in 1950, and even after a reduction of 35 per cent in the farm population, $13,182 million—or 3.7 per cent of the total national disposable personal income—in 1960, to raise the average level of income of the farm population to that of the nonfarm population [69, pp. 40, 41].

This figure would appear to be quite conservative, when viewed against the rates of return which have been found to accrue to expenditures on agricultural research and extension. Most related studies indicate an annual rate of return of about 100 per cent [51, pp. 120-22] [15, p. 428] [22, pp. 600-601] [57], which rate, according to one study [57], applies even if expenditures on price support, surplus disposal, and rural education and improvement are all included in the total outlays.[23] For purposes of this present analysis it is necessary to carry these estimates one important step further and recognize that, under the operation of the Mill-Marshallian model, the impressive returns cited are "paid off" in the form of lower prices for farm products. Thus they accrue primarily to nonfarm intermediaries and consumers and represent a handsome profit to the latter on any funds they contribute to the pump-priming operation. It would appear that in recent years in the United States, the nonfarm sector probably has been earning about two dollars for every dollar it has contributed to such expenditures, as well as receiving the major share of the corresponding benefits which have accrued to outlays for similar purposes originating in the farm sector itself. It should be obvious to development planners in all countries that, once a working version of the Mill-Marshallian model has been incorporated into an economic system, these types of outlays represent extremely strategic points for the injection of development-oriented public expenditures.

II. *The Expenditure Squeeze on Agriculture*

The foregoing argument points to the conclusion that a market-oriented, family farming system represents a very promising initial policy objective for leaders of less developed countries; an objective which once achieved will enable them to drive their predominantly agrarian economies toward higher rates of growth without resorting to uncertain and unsavory totalitarian alternatives.[24] In respect of over-all

[23] Tweeten and Tyner [57, p. 1078] indicate that the total annual outlay on these items in the United States, currently amounts to approximately $8 billion, including $0.8 billion for public and private research and extension work and $3.5 billion for price support and surplus disposal programs.

[24] The several problems, likely to be associated with the establishment of an operational version of the Mill-Marshallian model in developing countries, require separate treatment. It is suggested, however, that in most circumstances these problems are of a second order to the initial task of having the establishment of this model included among the objectives of developmental planning. The latter must wait upon the emergence of a wider understanding of the model and of its potentialities among the leaders of developing countries. Once the objective of the Mill-Marshallian model becomes a conscious component of developmental planning such questions as land tenure, farm size, agricultural research and education, credit and price stabilization programs, and planned investment, both in agricultural and in the farm factor supply and farm product processing sectors, can be approached with a new sense of their relevance to the over-all process of economic growth.

growth policy it provides an important part of the answer to the (lemma which was posed at the outset[25] by ensuring that the farm se tor not only will generate a high internal rate of growth, but also f internal structural reasons will be unable to lay claim to more than very limited share of its incremental contribution to the over-all ra of growth.

However, what has been discussed thus far by no means represer the complete story. No doubt, more important from the farm incor point of view are two further considerations which help to clarify t oversimplicity of the historically popular argument that low farm i comes can be corrected through a more rapid out-migration of lab from the farm sector.[26] These considerations are, first, the perpetu loss of capital from the farm sector associated with the historical ph nomenon of off-farm migration and, secondly, the unavoidable inhe tance by the farm sector of the responsibility to compensate lab resources which are rendered redundant in its contribution to over-a economic development, but which fail to find employment elsewhe These considerations will be discussed briefly.

A. *"Emigrant Capital"*

One of the characteristic features of the development process in a countries is the progressive saving of labor resources in farmi through increasing labor productivity and the reciprocal utilization this labor in industrial production and service activities [8]. The s called "drift to the city" of farm population goes hand in hand wi general economic development. What is not so widely recognized is t fact that the costs of rearing and, in countries like the United State the costs of providing a basic education to this stream of migrant lab are very considerable. Insofar as this labor is, in this sense, essential "costless" to the nonfarm sector, it can represent a sizable continuo outflow of "capital" from the farm sector.[27]

The earliest known study made of this phenomenon, relating to 19 data in Tennessee, indicated that the associated annual rate of disi vestment from the farm sector in respect of Tennessee agricultu

[25] Simon Kuznets has expressed this dilemma in these words: "one of the cruc problems of modern economic growth is how to extract from the product of agricult a surplus for the financing of capital formation necessary for economic growth withc at the same time blighting the growth of agriculture, under conditions where no e *quid pro quo* for such surplus is available" [31, p. 70].

[26] This case was restated quite blatantly in a recent publication of the Committee Economic Development [61].

[27] By contrast the inflow of capital resources to the farm economy to support te nological progress therein is fully—some might argue more than fully—paid for by farm economy.

amounted to a total of at least $139 million, or to at least $3,300 per migrant, and to at least $600 per year for every farm in that state [37, p. 368]. If the same estimate of the nonrecoverable costs to the farm economy of rearing and educating emigrating labor resources is extrapolated, the corresponding annual disinvestment rate for the United States farm economy as a whole, during the five years ending 1951,[28] comes to almost $3,000 million and to over $550 per year for every farm in the nation.[29] This was at a time when the average net income per farm, from both farm and nonfarm sources, amounted to $3,490, and when only $44 of this constituted direct government payments [66, pp. 40-41].[30]

These estimates, however, were extremely conservative even for Tennessee. They were based on the costs of rearing potential migrants to the age of only 15 years, and this in farm families having disposable cash incomes of less than $1,250. In the same study comparable estimates are provided for Tennessee farm families with disposable net cash incomes of over $2,200, which no doubt were more representative of the U.S. farmers as a whole. Based on this group the disinvestment rate associated with off-farm migration in 1949 was about $4,425 million for the total U.S. farm sector and about $820 per year for every farm. However, this estimate assumed total direct and indirect schooling costs of only $260 per migrant, and an average of only eight years of schooling. Actually, in the year of the study, the cumulative investments in school training to the age of 15 years for the United States averaged $939 per student [42, p. 50], and average current expenditures, excluding interest, in public day schools were running at $130 per pupil in Tennessee and at $209 per pupil throughout the country [72, p. 28, 29]. Furthermore, the median years of schooling completed by all U.S. farmers between the ages of 22 and 75 years of age was 8.3 years as of 1950 [13, p. 53] [17, p. 23] and for the farm emigrant population undoubtedly it was somewhat higher than this.[31]

[28] The statistical record shows an extremely low out-migration of farm labor in 1949 (371,000 compared with the representative average annual net rate of 890,400 for the 10 years ended 1949, and 838,600 for the 10 years ending in 1959). The average for the five years centering on 1949 was 904,000 [71, p. 613].

[29] That is, for every piece of land that was cultivated independently and which produced $150 of agricultural products or which comprised more than 3 acres (definition of a farm in the 1950 Census of Agriculture). There were 5,382,000 such farms in the United States in 1950.

[30] The average net income per farm *from farming alone* in Tennessee in 1949 was $1,427 and in the United States as a whole it was 68 per cent higher, or $2,401 [68, pp. 17, 25]. The latter figure was further supplemented by $966 per farm (or 40 per cent) from off-farm work [66, p. 34].

[31] One study cites the median years of school completed by farm-urban migrants in 1940 as 8.5 years [26, p. 407].

It is quite realistic to assume, therefore, that in the early 1950's the continuing drain on the total U.S. farm economy represented by its non-recoverable expenditures on the rearing and education of the off-farm migrant stream, probably was at least $5 billion per year and in excess both of $5,000 per migrant[32] and $1,000 per farm per year. Clearly the farm subsidy programs of the various more highly developed countries, though invariably a focus of political controversy, may be expected normally to fall far short of compensating the farm sector for this invidious capital drain which the process of development imposes upon it.

More precise estimates of the capital loss to agriculture associated with off-farm migration are needed.[33] Such estimates should also take account of the qualitative losses which result from the high degree of selectivity which is associated with the market for redundant farm labor, as well as purely quantitative data. It is well established that the farm-nonfarm migrant stream tends to represent a younger working population than that which remains on the farm [26, p. 407] [20, p. 382]. Thus, it comprises a higher proportion of earners relative to dependents; and, although the relationship in this respect has not been so clearly demonstrated, also important is any tendency for the migrant stream to carry with it the more highly trained and experienced [1, p. 82] [20, pp. 382, 383] [36, pp. 430-31], and perhaps also those

[32] A more recent study claims that the *net* cost of rearing and educating farm children to the age of 18 in 1954 averaged as much as $12,927 [56], which is about 87 per cent higher than the Long and Dorner estimate, when the latter is adjusted to take account of price changes and different sample age brackets. This 1954 estimate was an average for both sexes. The figure given is net of earnings of farm children prior to age 18. It excludes any monetary value of the household labor of mothers, losses due to the death of children, "interest charges" on the maintenance costs of the children and public costs associated with health and recreation.

[33] Simon Kuznets has demonstrated one approach to this task. He has estimated that if a hypothetical underdeveloped economy in which the share of farming in the national labor force was 75 per cent and its share in the net national product 60 per cent, were to develop over a decade at such a rate as to result in a reduction in the agricultural share of the labor force to 70 per cent, the transfer of embodied capital in the out-migration of farm labor would represent, in the first year of the decade: 10.1 per cent of the agricultural-sector income, 25.0 per cent of the nonagricultural-sector income, and 6 per cent of the net national product. In presenting these figures he stated that "it would have been easier, without violating the rules of plausibility, to raise this percentage than to lower it." Even so, 6 per cent of the NNP almost equals the total rate of new savings that might reasonably be expected to apply in such an economy. Kuznets acknowledges, of course, that the percentage of the nonfarm-sector income represented by imported embodied capital would tend to fall as the process of development proceeds. However, he also rightly emphasizes that the relative burden of the cost of rearing and educating emigrant labor on the diminishing farm sector also increases as the process of development proceeds [31, p. 74].

members of the rural community with the higher entrepreneurial capacities.[34] That there tends to be a continuing erosion of talent from the farm labor force, over and above numerical estimates of the migration rate, cannot be seriously doubted.

It should be re-emphasized that the phenomenon discussed above is not peculiar to countries whose agricultural sector is organized along the lines of the Mill-Marshallian model. Indeed it is quite apparent that in all developing circumstances a substantial fraction of the basic costs of rearing and educating that part of the nonfarm labor supply that originates in the farm sector will be borne by the latter. It is also apparent, however, that to the extent that public educational and health facilities are financed from general revenue rather than from local revenue sources, the burden upon the farm sector will be mitigated in some degree, and its contribution to the over-all process of capital accumulation will be correspondingly reduced. It might be noted in this connection that, inherent in the U.S. historical emphasis on local responsibility and autonomy in basic public education, has been the tendency to maximize the farm-sector share of the cost of its long-standing policy of compulsory basic education for all citizens. Thus, in 1950, 56.3 per cent of the costs of public elementary and secondary education in the United States was financed by local or county government bodies, with only 39.8 per cent and 2.9 per cent, respectively, being derived from state and federal sources [72, p. 28].

This suggests that the U.S. type of educational system has an additional important advantage for developing countries over and above the political and social advantages of local autonomy which are most commonly cited in its support. It also is of considerable significance that this type of educational system is particularly compatible with a family farming system, and thereby with the Mill-Marshallian model.

B. *Farm-Financed Social Welfare*

Last, but by no means least, there remains to be discussed a second subsequent "nonfarm" claim against that limited part of the total income stream which passes into the hands of the farm population. This is the claim to maintenance at farm-sector expense of any labor that is rendered redundant by the development process in that sector until

[34] This probability and its implications for the farm sector impressed Alfred Marshall, viz., "there is . . . very little inducement for really able men to enter the business of farming; the best enterprise and ability of the country generally avoid agriculture and go to trades in which there is room for a man of first-rate ability to do nothing but high-class work, to do a great deal of it, and therefore to get high earnings of management."

such time as this labor actually realizes an alternative employment opportunity in the nonfarm sector.[35]

Traditionally the major emphasis in economic analysis has been in the direction of implying that the responsibility for the existence of "surplus labor" in agriculture falls on the surplus labor itself. In its most extreme form, this position advances the argument that the farm problem is created by artificial farm-income support which upsets the intersectoral balance between marginal productivity and marginal return and thereby inhibits the flow of labor out of agriculture. However, in recent years there has been increasing support for the more realistic position, especially under conditions of substantial nonfarm unemployment, that the comparative marginal productivity of redundant farm labor *in the nonfarm sector* tends to be zero, or less than zero,[36] and will tend to remain so until such time as the necessary realizable job opportunities become available to it in that sector [55, Ch. 6] [45] [25, p. 105]. This view implies that the existence of surplus farm labor is more a nonfarm or general social responsibility than the primary responsibility of the low-income farm population itself.[37]

But notwithstanding the emergence of such a view, and of certain policies consistent with it, one cannot help but be struck by the "heads-I-win, tails-you-lose" type of circumstance in which the farm sector is placed in the development context. If realizable employment opportunities are created in the nonfarm sector through an adequate development program therein, then the nonfarm sector is able to tap a reservoir of mature labor resources without incurring the "rearing costs" and the basic training costs of this labor, and in most cases also the costs of its relocation. If, on the other hand, the nonfarm sector should fail to make available the needed job opportunities, then this potential labor supply can be left indefinitely where it is at the expense of the farm sector. Redundant farm labor, especially in the context of a family farming system, tends to take the form of *involuntary underemployment* rather than the form of its more politically explosive industrial counterpart of *involuntary unemployment*. Unlike corporate industrial enterprise, agriculture, and especially a family farming system, does not have the organic will or the social influence to plough its redundant workers into the streets, and thereby either of compelling

[35] Fei and Ranis [10] have provided a relevant analytical framework with respect to this redundant farm-sector labor.

[36] This would be the case taking into account the costs of relocation which normally would be incurred by any farm labor that moves to the city in search of a job.

[37] Hendrix [23] [24] has contributed a very useful analysis of the nonfarm or general economy conditions which underlie the lack of alternative opportunities for underemployed labor resources in agriculture.

the innovation of more adequate complementary national development programs or of laying claim to publicly supported unemployment compensation for the human resources concerned.[38]

While the discussion of surplus labor in agriculture normally centers upon the more densely populated underdeveloped countries, in order to complement previous figures it is instructive to consider that if, in the United States, all small-scale family farms were to be reorganized into efficient family farms by modern standards—that is, farms marketing at least $10,000 worth of products—between 1½ and 2½ million farm families would be displaced from farming without any likely loss in aggregate farm production.[39] Undoubtedly, however, if the associated breadwinners were displaced from the farm sector, in the absence of some very dramatic and relevant nonfarm-sector adjustment and development programs, there would be a substantial increase in national unemployment figures. Were only 1½ million of the families which thus might be displaced from the farm sector in any serious attempt to raise the latter nearer to its real technological capacity unable to find employment elsewhere, the total present annual cost of their subsistence, whether financed from savings or from private or public transfer payments, would approximate $4.5 billion at conservative nonfarm-sector costs of living.[40]

Even in such a highly developed country as the United States, the recurrent charge upon the farm sector of the social welfare cost of supporting redundant farm labor, therefore, is quite considerable. However, there can be little doubt that it tends to be of even greater significance in countries at earlier stages of development and especially in such countries which also suffer from dense population ratios. Certainly, in many of these cases, it is of such proportion as to place the

[38] Recently Schultz [50, pp. 53-70] also has emphasized the implications which flow from the fact that redundant farm labor is absorbed into the fabric of the farm productive process and as such is not "free" labor. That is, the marginal productivity of surplus farm labor *in agriculture* may approach zero but, in underdeveloped countries especially, there still remains a problem of freeing it from the industry without a deleterious impact on agricultural production.

[39] The latter figure was suggested recently by Federal Budget Director Kermit Gordon [14, p. 27]. Higbee [25, p. 13] suggests that 2.2 million farms could go out of business between 1959 and 1969 and "their output would not be missed."

[40] Based on $3,000 per annum, or about one half of the estimated, average, "moderate but adequate" cost of living for a family of four in large cities in 1959 [32]. Hendrix [24, p. 525] suggests that in 1959 the median net income of comparable workers in the nonfarm economy to the involuntarily unemployed labor in agriculture was $3,200. Applying this figure to a more conservative estimate of underemployment in agriculture, namely, 1.1 million male farm workers in the 20 to 64 age bracket, he concludes that "if they had been as productively employed as were workers of comparable ability in the non-farm economy, they could have earned in the aggregate an additional income equivalent to about $3.5 billion net farm income."

farm sector under a real, if not a crippling, handicap relative to the nonfarm sector in the matter of growth [48, p. 71]. Where it is so important as to stifle farm-sector growth entirely, the danger is that, through the backwash effects of sustained unbalanced growth, over-all development eventually will also be seriously impeded, if not also stifled.

At the same time it can be argued that, short of its having a crippling effect on over-all agricultural development, programs aimed at facilitating the outflow of labor from the farm sector probably deserve only low priority in the initial investment plans of underdeveloped countries. Indeed, purely on the grounds of comparative welfare costs, in many such cases there may be merit in policies aimed at discouraging a rapid flow of the latter into the narrow-based nonfarm sector,[41] and this even at the cost of some form of welfare subsidy to the farm sector to assist it in the support of its redundant labor. The challenge in most of the underdeveloped countries is to find a way to effect the most efficient working compromise between the advantages of farm-sector-based social welfare for the poor and the developmental potential of adequate-sized family farms. This objective in most cases, no doubt, may best be realized through a deliberate creation of two subsectors in agriculture, one being designed to maximize the output of social welfare for surplus labor and the other to exploit the full dynamics of the Mill-Marshallian model in respect of development. This, of course, is precisely what has happened as a haphazard by-product of the process of development in the United States and other economically advanced countries. By accident more than by design the farm sectors in all of these countries comprise a minority of highly productive commercial farms and a large majority of essentially subsistence farms, with the former producing most of the surplus commodities and the latter supporting most of the surplus people.[42] While there would seem to be little justification for the perpetuation of this phenomenon in countries at such an advanced stage of economic development as the United States, the idea of planned rural "slums"—a system of small holdings with associated, and socially subsidized, educational facilities,

[41] The argument, while especially relevant to poor countries aspiring to a high rate of development, has a measure of general validity. As one writer has pointed out, "Until a nation needs its lands and labor, inefficient farmers may be better off where they are than if they were to head for the city's slums and welfare rolls. . . . To achieve a healthier agriculture at the expense of a sicker urban society is hardly a commendable objective" [25, p. 105].

[42] For example, in the United States in 1959, 21.3 per cent of all farms supplied 71.8 per cent of all farm products sold on the market, while the remainder supplied only 28.2 per cent of the marketed products. These farms clearly supported most of the farm population [70].

public works, rural industries, and even supplementary welfare payments—may have considerable merit in the early stages of development.

III. *Utilizing the Spoils*

The superiority of the Mill-Marshallian model for the incorporation of the farm sector into the over-all development process from the point of view of promoting agricultural productivity appears very clear. It is also apparent that this model, in association with the more general phenomenon of "emigrant capital" and/or the assumption of the costs of supporting redundant farm labor, provides a very effective framework for the appropriation of the major fraction of the gains in farm productivity for other purposes than that of increasing farm incomes. At least in the case of the United States, most of the income differential between the farm and nonfarm population can be explained in these terms, leaving very little to account for purely in terms of economic inefficiencies in the performances of farm-sector factors or enterprises themselves.[43]

The advantage of the Mill-Marshallian framework with regard to the matter of insuring a utilization of the appropriated potential for nonfarm-sector development purposes is not so obvious. Under the Marx-Leninist model the requisitioned food is subject to allocation within the framework of a centralized plan and to this extent can be purposively directed into development forms of utilization according to the general principle: to each according to his contribution to the fulfillment of the development plan.[44] By contrast, under the operation of the Mill-Marshallian model a substantial part of the potential is apt to be scattered among consumers in the nonfarm sector in the form of higher real incomes where, under the conditions which characterize most of the underdeveloped countries, it would appear more likely to be lost in the bottomless pit of underconsumption than translated into a higher rate of investment activity.

Countries successfully following the Mill-Marshallian model have all had to incorporate counteracting institutional influences within their social-economic structures to offset the potential impact of this nonfarm consumption leakage on its ultimate effectiveness as an instrument for capital accumulation and a high rate of over-all economic

[43] In this sense, Professor Schultz's claim [50, pp. 36-52] that "there are comparatively few significant inefficiencies in the allocation of the factors of production in traditional agriculture" [50, p. 37] turns out to be a proposition that can be applied quite generally, almost irrespective of the stage of development.

[44] The investment opportunity represented in food resources has been well argued by Matthew J. Kust [30].

growth.[45] Obviously important in this regard have been historical patterns of income distribution and the role of the corporation and of government in nonfarm savings and investment activities in these countries. However, for the benefit of underdeveloped countries which might aspire in the future to follow this model, especially significant would appear to be an innovation that Yugoslavia introduced in returning essentially to the Mill-Marshallian model after its abortive experiment with the Marx-Leninist model [3]. By placing the emphasis in agricultural production organization on the independent peasant proprietor supported by more voluntary cooperation, the Yugoslavs have been able, even on the basis of miniature farms, to gain the benefit of a superior record in agricultural productivity[46] in conjunction with relatively low food prices. However, the resulting diversion of income to the nonfarm sector has been treated, not as the end of the process, but merely as a means to a more effective mobilization of domestic savings. Thus, by applying a deliberately high differential rate of taxation in the urban-industrial economy, Yugoslavia has been able to raise its domestic savings to about 29 per cent of the social product and, with the aid of supplementary foreign resources, to maintain a level of investment as high as 35 per cent (1961) [11]. If an effective system of differential taxation can in this way be associated with the basic Mill-Marshallian model, its growth potentialities undoubtedly stand to be expanded very considerably over the record of most countries following this model to date.

IV. *Conclusion*

In conclusion, two implications of the above analysis deserve to be emphasized. First, the perpetuation of a substantial backlog of underutilized labor in the farm sector in the United States and in most of the richer countries attests mainly to the fact that the urban-industrial development programs in these countries have fallen far short of their potentialities, and correspondingly of the comparable record of performance provided by their farm sectors. Their record also has been more one of consuming the fat of the land than of utilizing it fully in the

[45] In addition, while the record under the Mill-Marshallian model has not been as abortive in practice as this characteristic of the model might imply, neither has the direct administration of "food investment" proved to be as straightforward as it might appear to be at first sight. Putting this part of the Marx-Leninist model into practice has proved to be much easier said than done. Much light has been thrown on this problem by recent studies, such as that by the F.A.O. [62], related to the possibilities of utilizing accumulated and potential surpluses of agricultural products in the United States and elsewhere for developmental purposes in the poorer countries.

[46] The compound annual rate of growth of total agricultural production in Yugoslavia over the period of 1952-62 was 6.1 per cent [7, p. 1054], a rate exceeded or approached by very few other countries.

promotion of over-all growth and development. It might be presumed that at such high levels of development a conscious effort to establish a higher degree of equality between farm and nonfarm incomes would be both merited and practical, and the fact that, in spite of all the farm programs, so little has been achieved in this way is worthy of serious thought.[47]

However, and secondly, in the developing countries the emphasis clearly should be placed, not on immediate equity between farm and nonfarm incomes, but on the maximization of the growth rate in agriculture and the maximum immediate diversion of the resulting increments to the protection and support of the emerging nonfarm sector and of the generally differentiated and interdependent features of a more highly developed economy. That is, the identified Mill-Marshallian model has been described not primarily to show how agriculture tends to be an unduly exploited sector in economically advanced countries, but rather to identify a tested and relatively painless method whereby the inevitably "painful" or sacrificial process of domestic capital accumulation can be set in self-sustaining motion and progressively accelerated in a traditional, agrarian economy. But, first must come the will and the wit to effect those structural reforms which are necessary in most underdeveloped countries to condition the application of this method of accumulation.

References

1. H. W. Baumgartner, "Potential Mobility in Agriculture: Some Reasons for the Existence of a Labor Transfer Problem," *Jour. Farm Econ.*, Feb. 1965, *47*, 74–82.
2. J. R. Bellerby, *Agriculture and Industry: Relative Incomes.* New York 1956.
3. R. Bicanic, "Economic Growth Under Centralized and Decentralized Planning. Yugoslavia—A Case Study," *Econ. Develop. Cult. Change*, Oct. 1957, *6*, 64–75.
4. M. Bornstein, "The Soviet Price System," *Am. Econ. Rev.*, March 1962, *52*, 64–103.
5. K. E. Boulding and P. Singh, "The Role of the Price Structure in Economic Development," *Am. Econ. Rev. Proc.*, May 1962, *52*, 28–45.
6. J. M. Brewster, "The Machine Process in Agriculture and Industry," *Jour. Farm Econ.*, Feb. 1950, *32*, 69–81.
7. R. P. Christensen and H. T. Yee, "The Role of Agricultural Productivity in Economic Development," *Jour. Farm Econ.*, Dec. 1964, *46*, 1051–61.
8. C. Clark, *The Conditions of Economic Progress.* New York 1940.

[47] Thus, in the United States, the relative size of the average incomes of persons engaged in agriculture to those of nonsupervisory factory workers fell from 62.9 per cent in 1910 to 43.5 per cent in 1960 [67].

9. W. W. COCHRANE, *Farm Prices, Myth and Reality.* Minneapolis 1958.
10. J. C. H. FEI AND G. RANIS, *Development of the Labor Surplus Economy: Theory and Policy.* New Haven 1964.
11. J. M. FLEMING AND V. R. SERTIC, "The Yuogslav Economic System," *Internat. Mon. Fund Staff Papers,* July 1962, pp. 202–23.
12. J. K. GALBRAITH, *American Capitalism: The Concept of Countervailing Power.* Boston 1952.
13. P. C. GLICK, "Educational Attainment and Occupational Advancement," *Low Income Families,* Hearings before the Subcommittee on Low Income Families of the Joint Committee on the Economic Report, 84th Cong., 1st sess., Washington 1955, pp. 251–57.
14. K. GORDON, "How Much Should Government Do?" *Sat. Rev.,* Jan. 9, 1965.
15. Z. GRILICHES, "Research Costs and Social Returns: Hybrid Corn and Related Innovations," *Jour. Pol. Econ.,* Oct., 1958, 66, 419–31.
16. ———, "The Sources of Measured Productivity Growth: United States Agriculture, 1940–60," *Jour. Pol. Econ.,* Aug. 1963, *71,* 331–46.
17. W. L. HANSEN, "Total and Private Rates of Return to Investment in Schooling," *Jour. Pol. Econ.,* April 1963, *71,* 128–40.
18. C. D. HARRIS, "Growing Food by Decree in Soviet Russia," *For. Affairs,* Jan. 1955, *33,* 269–81.
19. M. HARRIS, "The Family Farm in Domestic and Foreign Land Policy; Problems in Domestic Policy," *Jour. Farm Econ.,* May 1962, *44,* 536–45.
20. D. E. HATHAWAY, "Migration from Agriculture: The Historical Record and Its Meaning," *Am. Econ. Rev. Proc.,* May 1960, *50,* 379–91.
21. Y. HAYAMI, "Demand for Fertilizer in the Course of Japanese Agricultural Development," *Jour. Farm Econ.,* Nov. 1964, *46,* 766–80.
22. E. O. HEADY, *Agricultural Policy Under Economic Development.* Ames, Iowa 1962.
23. W. E. HENDRIX, "Income Improvement Prospects in Low Income Areas," *Jour. Farm Econ.,* Dec. 1959, *41,* 1065–75.
24. ———, "Relation of Chronic Low Farm Incomes to Major National Economic Policies," *Jour. Farm Econ.,* May 1962, *44,* 522–33.
25. E. HIGBEE, *Farms and Farmers in an Urban Age.* New York 1963.
26. D. G. JOHNSON, "Policies to Improve the Labor Transfer Process," *Am. Econ. Rev. Proc.,* May 1960, *50,* 403–12.
27. B. H. JOHNSTON, "Agricultural Productivity and Economic Development in Japan," *Jour. Pol. Econ.,* Dec. 1951, *59,* 498–513.
28. J. W. KENDRICK, "Productivity Trends in Agriculture and Industry," *Jour. Farm Econ.,* Dec. 1958, *40,* 1554–64.
29. N. M. KOFFSKY, "Analytic Concepts: Changes Induced by Technological and Economic Developments," *Jour. Farm Econ.,* May 1962, *44,* 625–32.
30. M. J. KUST, "Economic Development and Agricultural Surpluses," *For. Affairs,* Oct. 1956, *35,* 105–15.
31. S. KUZNETS, "Economic Growth and the Contribution of Agriculture:

Notes on Measurement," *Internat. Jour. Agrar. Affairs*, April 1961, *3*, 56–75.
32. H. H. LAMALE AND M. S. STOTZ, "The Interim City Workers Family Budget," *Mon. Lab. Rev.*, Aug. 1960, *83*, 785–808.
33. L. B. LAVE, "Empirical Estimates of Technological Change in the United States' Agriculture," *Jour. Farm Econ.*, Nov. 1962, *44*, 941–52.
34. V. I. LENIN, *Selected Works, Vol 12: Theory of the Agrarian Question.* New York 1938.
35. Q. W. LINDSEY, "The Problem of Periodic Reorganization in American Agriculture," *Jour. Agric. Econ.*, May 1961, *14*, 359–67.
36. E. J. LONG, "Rehabilitation of Depressed Rural Areas," *Low Income Families*, Hearings before the Subcommittee on Low Income Families of the Joint Committee on the Economic Report, 84th Cong., 1st sess., Washington 1955, pp. 428–35.
37. ——— AND P. DORNER, "Excess Farm Population and the Loss of Agricultural Capital," *Land Econ.*, Nov. 1954, *30*, 363–68.
38. A. MARSHALL, *Principles of Economics.* London 1936.
39. K. MARX, *Capital*, Vol 3. Chicago 1909.
40. G. H. MEADE, *Movements of Thought in the 19th Century*, Chicago 1936, Appendix
41. J. S. MILL, *Principles of Political Economy*, W. J. Ashley ed. London 1909 (March 1936 Printing).
42. J. MINCER, "On the Job Training: Costs, Returns and Some Implications," *Jour. Pol. Econ.*, Oct. 1962 (Suppl.), *70*, 50–79.
43. D. MITRANY, *Marx Against the Peasant.* Chapel Hill 1951.
44. W. H. NICHOLLS, "An 'Agricultural Surplus' as a Factor in Economic Development," *Jour. Pol. Econ.*, Feb. 1963, *71*, 1–29.
45. ———, "Industrialization, Factor Markets, and Agricultural Development," *Jour. Pol. Econ.*, Aug. 1961, *69*, 319–40.
46. N. NIMITZ, "Soviet Agricultural Prices and Costs," *Comparisons of the United States and Soviet Economies*, Papers submitted by panelists appearing before the Subcommittee on Economic Statistics of the Joint Economic Committee, 86th Cong., 1st sess., Washington 1959, pp. 239–84.
47. G. W. NUTTER, *The Growth of Industrial Production in the Soviet Union.* Princeton 1962.
48. W. F. OWEN, "Agrarian Reform and Economic Development with Special Reference to Egypt," *Rocky Mtn. Soc. Sci. Jour.*, April 1964, *2*, 62–76.
49. P. M. RAUP, "Land Tenure Adjustments in Industrial-Agricultural Economies," *Modern Land Policy*, Land Economics Institute. Urbana, Ill. 1960.
50. T. W. SCHULTZ, *Transforming Traditional Agriculture.* New Haven 1964.
51. ———, *The Economic Organization of Agriculture.* New York 1953.
52. R. K. SEVERIN, "Realistic Planning for Soviet Agriculture," *Jour. Farm Econ.*, Dec. 1962, *44*, 1401–5.

53. A. SMITH, *An Inquiry into the Nature and Causes of the Wealth of Nations*. London 1812.
54. H. L. STEWART, "The Organization and Structure of Some Representative Farms in 1975," *Jour. Farm Econ.*, Dec. 1960, *42*, 1367–97.
55. A. M. TANG, *Economic Development in the Southern Piedmont. 1860–1950*. Chapel Hill 1958.
56. J. D. TARVER, "Costs of Rearing and Educating Farm Children," *Jour. Farm Econ.*, Feb. 1956, *38*, 144–53.
57. L. G. TWEETEN AND F. H. TYNER, "Toward an Optimum Rate of Technological Change," *Jour. Farm Econ.*, Dec. 1964, *46*, 1075–84.
58. L. VOLIN, "Agricultural Policy of the Soviet Union," *Comparisons of the United States and Soviet Economies*, Papers submitted by panelists appearing before the Subcommittee on Economic Statistics of the Joint Economic Committee, 86th Cong., 1st sess., Washington 1959, pp. 285–14.
59. B. WARD, *India and the West*. New York 1961.
60. W. W. WILCOX AND W. W. COCHRANE, *Economics of American Agriculture*. New York 1951.
61. COMMITTEE FOR ECONOMIC DEVELOPMENT, *An Adaptive Policy for Agriculture*. New York 1962.
62. F. A. O. COMMODITY POLICY STUDY No. 6, *Uses of Agricultural Surpluses to Finance Economic Development in Under-developed Countries: A Pilot Study in India*. Rome 1955.
63. FEDERAL TRADE COMMISSION, SECURITIES AND EXCHANGE COMMISSION, *Quarterly Financial Report for Manufacturing Corporations*. Washington.
64. U.S. CONGRESS, *Economic Report of the President*. Washington 1964.
65. U.S. DEPARTMENT OF AGRICULTURE, *The Balance Sheet of Agriculture*, Agric. Inf. Bull. 232. Washington 1960.
66. ———, *Farm Income Situation*, No. 179. Washington, July 1960.
67. ———, *Farm Income Situation*, No. 183. Washington, July 1961.
68. ———, *Farm Income Situation*, No. 183 (Suppl.). Washington, Aug. 1961.
69. ———, *Farm Income Situation*, No. 195. Washington, July 1964.
70. U.S. DEPARTMENT OF COMMERCE, BUREAU OF THE CENSUS, *Census of Agriculture*. Washington 1959.
71. ———, *Statistical Abstract of the United States*. Washington 1961.
72. U.S. DEPARTMENT OF HEALTH, EDUCATION AND WELFARE, *Biennial Survey of Education in the United States, 1948–1950*. Washington 1954.

HIDDEN UNEMPLOYMENT 1953–62: A QUANTITATIVE ANALYSIS BY AGE AND SEX

By THOMAS DERNBURG AND KENNETH STRAND*

Recent research [1] [2] [3] [4] shows that the loss of jobs owing to declining business activity is accompanied by the withdrawal of workers from the civilian labor force. Because of such withdrawal the labor force figures that are recorded during periods of economic slack are considerably smaller than the figures that would have been recorded had the economy been at full employment. The recorded level of unemployment and the unemployment rate therefore understate the true level of manpower underutilization during such periods. For example, our own work [1] indicates that the unemployment rate for 1962 rises to over 9 per cent when the number of persons who withdraw from (or never enter into) the labor force because of slack economic conditions are added back into labor force and unemployment statistics.

Some workers withdraw from the labor force because of the "discouraged worker" effect. Other workers, however, respond to "additional worker" pressure and therefore enter the labor force. The degree to which the two effects govern labor force participation depends upon the stage of the business cycle. A fall in employment from a cyclical peak is initially accompanied by a sharp decline in labor force participation. Thereafter additional falls in employment are accompanied by a considerably lower level of withdrawal because the discouraged-worker effect is partially offset by the influx of additional workers during later stages of recession. Put differently, as economic adversity continues, pressure builds up on additional workers whose entry into the labor force partially offsets the discouraged-worker effect.

The present paper reports the results of an attempt to answer the following questions: (1) Who are the discouraged workers who leave the labor force when employment declines? (2) Who are the additional work-

* The authors are, respectively, professor of economics at Oberlin College, and economist, Manpower and Social Affairs Directorate, Organization for Economic Co-operation and Development. Initial stages of the research were supported under a Ford Foundation Public Affairs grant to Oberlin College. The major source of support is a Brookings Institution Research Professorship that was awarded to one of the authors for the academic year 1964–65. Additional help came from the Yale-MIT growth project and from the Research Seminar in Quantitative Economics at the University of Michigan. Of the many persons who gave advice and criticism, particular votes of thanks are owed to Daniel B. Suits and James Tobin. The authors are grateful also for the support and hospitality that was extended to them by the Department of Economics at the University of Michigan.

ers who enter the labor force under the pressure of economic adversity? (3) What is the quantitative relationship between employment and labor force participation in these respective groups? (4) What are the additional employment requirements, both in the aggregate and for each group, that would achieve a 4 per cent over-all unemployment rate? (5) What unemployment rates, for the individual groups, would be associated with over-all full employment? (6) Has there been a structural shift in the composition of the labor force of a sort that might render the attainment of full employment more difficult?

I. *Data and Variables*

The analysis utilizes monthly Bureau of Labor Statistics (BLS) data of population, labor force, and employment, unadjusted for seasonal variation, as well as a series of new exhaustions of unemployment compensations of the Bureau of Employment Security.[1] Past experience points to the presence of such subtle and close timing relationships that monthly data are to be preferred to quarterly or annual data.

We are concerned with the adult, civilian, noninstitutional population, which we shall denote simply as "total population." Variables that are accompanied by subscripts denote values for a particular subset of the population. Thus L_i, E_i, and P_i are the number of persons in the labor force, the level of employment, and the population, respectively, in group i. Variables that are unaccompanied by subscripts denote aggregates. Therefore, P, E, L, and X represent total population, total employment, the total labor force, and the total number of new unemployment compensation exhaustions, respectively. The statistical analysis is in terms of ratios. We define:

L_i/P, the "group labor force participation ratio"—i.e., the ratio of labor force in group i to total population;[2]

E_i/P, the group employment ratio—i.e., the ratio of employment in group i to total population;

L/P, the aggregate labor force participation ratio;

E/P, the aggregate employment ratio; and

[1] Most of the data were provided directly from the Bureau of Labor Statistics through the kind cooperation of Mr. Harold Goldstein, Assistant Commissioner of Manpower Statistics, and Miss Gertrude Bancroft, Special Assistant to the Commissioner of Labor Statistics. Monthly series on Population, Labor Force, and Employment may be found in the Bureau's publication *Employment and Earnings* and in its *Monthly Report on the Labor Force*. Data for unemployment compensation exhaustions were provided by Mr. Robert C. Goodwin, Administrator of the Bureau of Employment Security. A rounded series may be found in *Health and Welfare Indicators*.

[2] The ratio of labor force to population within a group is customarily called a "labor force participation rate." Since we utilize group labor force to total population as our dependent variable, we hope to avoid confusion by calling the resulting statistic a "labor force participation ratio."

X/P, the exhaustions ratio—i.e., the ratio of new unemployment compensation exhaustions to total population.

II. *The Total Civilian Labor Force*

The results of the previously cited global study form an integral part of the present analysis. They serve, moreover, to introduce our general procedure and therefore provide a useful starting point. We attempt to explain the aggregate labor force participation ratio by means of an equation that embodies both the discouraged-worker and the additional-worker hypotheses and that also makes allowance for trend and for seasonal variation. The expression is:

$$(1) \quad \left(\frac{L}{P}\right)_t = a_m + a_1\left(\frac{E}{P}\right)_t + a_2\left(\frac{X}{P}\right)_{t+2} + a_3\left(\frac{1}{P}\right)_t + r_{t1},$$

where r_{t1} is a residual-error term. Seasonal variation is taken into account by utilizing dummy variables in such a way that the intercept term, a_m, changes with the month of the year. Since a fall in the employment ratio is indicative of a loosening of the labor market, we expect the resulting relative scarcity of job opportunities to lead to discouragement and withdrawal from the labor force. The coefficient a_1 is therefore expected to be positive. Similarly, a positive value for the coefficient a_2 is taken to be evidence in support of the additional-worker hypothesis. When unemployment-compensation exhaustions of workers rise, pressure on additional workers to enter the labor force increases, and labor force participation therefore increases. Finally, statistical significance of the coefficient a_3 would indicate the presence of a secular trend in the labor force participation ratio.

The two-period lead of the exhaustions ratio reflects a statistical result that can be explained by the fact that the pressure on additional workers to enter the labor force in the face of unemployment on the part of the primary worker undoubtedly begins prior to the actual exhaustion of unemployment-compensation payments. Thus the pressure that exists in month t is, on the average, not reflected in the exhaustions data until two periods later.

Statistical analysis lends support to the hypotheses. For the period November 1952–December 1962, inclusive,[3] we obtain:

$$(1a) \quad \left(\frac{L}{P}\right)_t = a_m + \underset{(.0367)}{.9490}\left(\frac{E}{P}\right)_t + \underset{(.735)}{12.699}\left(\frac{X}{P}\right)_{t+2} - \underset{(695.4)}{5326.1}\left(\frac{1}{P}\right) + r_{t1}.$$

[3] The 122 observations are reduced to 120 by virtue of the two-period lead of the exhaustions ratio. All estimates in this paper are based on 120 observations.

The values in parentheses beneath the regression coefficients are the standard errors of the respective coefficients. The coefficient of determination[4] and the standard error of estimate are, respectively, .8766 and .00206. The coefficients a_1 and a_2 have the expected signs, and both are statistically significant at the 1 per cent level. The coefficient a_3, which permits gauging of secular changes in the participation ratio, is also statistically significant and, oddly enough, the sign of the coefficient is negative, thereby indicating that the labor force participation ratio, after allowance for adverse employment conditions, would have been observed as increasing over the period covered by the data.

Because the exhaustions ratio is a function of past employment (unemployment) levels, the coefficient a_1 cannot be interpreted as the derivative of labor force participation with respect to employment. We therefore add the second equation:

$$\left(\frac{X}{P}\right)_t = b_m + b_1\left(\frac{E}{P}\right)_{t-1} + b_2\left(\frac{X}{P}\right)_{t-1} + r_{t2}, \tag{2}$$

which incorporates the hypothesis that the exhaustions ratio in month t is a function of all past employment ratios with the influence of each employment ratio diminishing geometrically as it recedes into the past. The population variable is not statistically significant. When we replace the coefficients with their numerical estimates, we have:

$$\left(\frac{X}{P}\right)_t = b_m - \underset{(.00175)}{.00419}\left(\frac{E}{P}\right)_{t-1} + \underset{(.0404)}{.8829}\left(\frac{X}{P}\right)_{t-1} + r_{t2}, \tag{2a}$$

with coefficient of determination, .9326, and standard error of estimate, .000115.

Equations (1) and (2) can be combined, and the effect on labor force participation of an instantaneous change from one stationary employment ratio to another can be calculated. The result is:

$$\frac{a_1(1 - b_2) + a_2 b_1}{1 - b_2} = .4951,$$

and we therefore conclude that, over the decade covered by the data, one person appears to have left the labor force for every two who lost their jobs.

[4] R^2 is the coefficient of determination about the monthly means rather than about the over-all mean. If D^2 is the coefficient of determination about the over-all mean and A^2 is the coefficient of determination obtained by regressing the dependent variable only upon the monthly dummy variables, then:

$$R^2 = \frac{D^2 - A^2}{1 - A^2}.$$

III. *The Age-Sex Division: Hypotheses*

In moving from the labor force as a whole to the individual age-sex groups we take equation (1) and replace the aggregate labor force participation ratio with the participation ratio of the specific group under analysis. Because the participation ratio of a specific group will vary if the fraction of the total population in that group changes, we add the variable $(P_i/P)_t$ in order to capture the effect of relative population shifts. The counterpart to equation (1) for group i therefore is written:

$$(3)\quad \left(\frac{L_i}{P}\right)_t = a_{m_i} + a_{1i}\left(\frac{E}{P}\right)_t + a_{2i}\left(\frac{X}{P}\right)_{t+2} + a_{3i}\left(\frac{1}{P}\right)_t + a_{4i}\left(\frac{P_i}{P}\right) + r_{t3}.$$

The pressure on additional workers to enter the labor force is, of course, likely to be reflected by the exhaustion of unemployment compensation of persons in other groups. It is therefore appropriate to retain the aggregate exhaustions ratio as a measure of additional-worker pressure. It is assumed that the two-period lead of the exhaustions ratio remains appropriate.

Variations in labor force participation in group i will be most closely correlated with variations in employment within the group. However, since our purpose is to study cyclical effects on labor force participation and since the discouraged-worker hypothesis relates to total labor market tightness, we prefer to utilize the aggregate employment ratio rather than the group specific-employment ratios. Differential changes in employment may take place between groups without being accompanied by any change in over-all labor market tightness. Consequently a statistical correlation between E_i and L_i would not constitute a clear-cut test of the discouraged-worker hypothesis.[5]

Answers to all of the questions that were posed at the outset of this paper cannot be obtained without an expression that relates total employment to group employment. We therefore specify:

[5] There is another important reason why we should not regress L_i/P on E_i/P. Both variables are derived from the same sample. Because one constitutes a large fraction of the other, the two will show a high degree of correlation in the event that sampling and response errors are present. If the sample value of employment in a group is in excess of its true value, the chances are that the sample value of labor force for the group will probably also be in excess of its true value. Consequently the sample values of the variables will exhibit statistical correlation, the regression coefficients will be biased, and we would not know if a true association between group labor force and group employment exists.

The procedure of regression group labor force participation on aggregate employment helps to liberate us from this difficulty. Although for any subgroup of the population the sampling errors in the labor force and employment values are apt to be correlated, there is no reason to suppose that such correlation exists between the sampling errors in a particular subgroup of the labor force and the labor force as a whole. While L_i and E_i are likely to be correlated if sampling error is present, there is little reason to suppose that L_i and E, or E_i and E should also be correlated.

$$(4)\qquad \left(\frac{E_i}{P}\right)_t = c_{mi} + c_{1i}\left(\frac{E}{P}\right)_t + c_{2i}\left(\frac{1}{P}\right)_t + c_{3i}\left(\frac{P_i}{P}\right)_t + r_{tti},$$

which, in conjunction with equations (2) and (3), permits us to observe the effect of a change in aggregate employment on group labor force participation and employment.

The statistical results for equations (3) and (4), covering the same period as the global analysis, are reported in Tables 1 and 2, respectively. Although there is no need to be concerned with the individual monthly intercept terms, we do need the average values which we report in column 1. The last column of each table gives the coefficients of determination. The intervening columns contain the regression coefficients and their respective standard errors. The purpose of the asterisks is to call attention to the fact that the coefficients they accompany are not statistically significant at the 5 per cent level of significance.

The results reported in Table 1 are our primary concern. Inspection of the a_{1i} coefficients, together with their respective standard errors, indicates that labor force participation is positively associated, in a statistically significant manner, with total employment in all but three of the groups. The discouraged-worker hypothesis is therefore consistent with the data in 11 of the 14 groups. All of the female groups evidently are subject to the discouraged-worker effect, while among men only the very young and the very old seem to be so affected.

The coefficient a_{2i}, as expected, is positive for all groups and is statistically significant in all but three of them. The additional-worker hypothesis is therefore consistent with the data in 11 of the 14 groups. Taking both effects into account, we conclude that labor force participation is virtually autonomous for males between the ages of 25 and 64, while substantial fluctuations in participation are observed in all female groups and especially among the youngest and oldest groups of both sexes.

Statistical significance of the coefficient a_{3i} suggests the presence of a trend in the participation ratios. Since this is the coefficient that accompanies the reciprocal of population variable, the predominantly positive signs for males indicate a secular decline in the male participation ratios, while the negative signs for women reflect the substantial rise in participation that has taken place among women during the decade.

The coefficient a_{4i} of the population ratio, P_i/P, is statistically significant in all but two groups. Because an increase in the population of a group relative to the total would always be expected to raise the group's participation ratio, it would have been most surprising if any of these coefficients had been significantly negative. The coefficients are, of

TABLE 1—EQUATIONS (3): REGRESSION COEFFICIENTS, STANDARD ERRORS, AND COEFFICIENTS OF DETERMINATION

	(1) a_{mi}	(2) a_{1i}	(3) a_{2i}	(4) a_{3i}	(5) a_{4i}	(6) R^2
Male						
14–19	−.0789	.1256 (.0194)	1.05 (.36)	1,380* (1,030)	.4102 (.0838)	.5958
20–24	.0061	.0391 (.0123)	.35* (.22)	−2,350 (680)	.6510 (.0760)	.9635
25–34	−.0078	.0032* (.0057)	.04* (.12)	1,850 (680)	.8682 (.0402)	.9977
35–44	−.0090	−.0023* (.0040)	.01* (.08)	−110* (270)	1.0992 (.0512)	.9897
45–54	−.0139	.0174 (.0052)	.34 (.11)	660 (110)	.9432 (.0520)	.8527
55–64	.0005	.0107* (.0083)	.57 (.17)	1,390 (510)	.5665* (.3709)	.7535
65 and over	−.0965	.0726 (.0068)	.92 (.14)	5,360 (170)	.5540 (.0586)	.9665
Female						
14–19	−.0794	.1138 (.0136)	.91 (.27)	860* (790)	.4471 (.0688)	.7883
20–24	−.0096	.0329 (.0101)	.60 (.20)	−1,140 (230)	.4717 (.0525)	.4570
25–34	−.0004	.0597 (.0114)	.96 (.23)	−6,110 (2,100)	.5369 (.1055)	.9060
35–44	−.0264	.0970 (.0119)	1.40 (.24)	−3,690 (510)	.4593 (.1614)	.6564
45–54	−.0490	.0934 (.0099)	1.10 (.20)	−6,570 (330)	1.0845 (.0664)	.9853
55–64	−.0427	.0695 (.0095)	.43 (.19)	−4,970 (310)	1.0631 (.1303)	.9597
65 and over	−.0017	.0519 (.0068)	.54 (.15)	−2,040 (570)	−.0469* (.0693)	.6371

course, highest for the groups whose members are most likely to be in the labor force.

The results for equation (4) are reported in Table 2. A rise in total employment causes employment in all groups to rise. The lowest c_{1i} coefficients occur for males 55–64 and females 20–24. Presumably the

jobs of this group of males are more completely "fenced in" by seniority and other forms of job protection. The result for females 20–24 is an optical illusion that stems from the fact that the group's class interval is smaller than for the older groups. Actually there seems to be little difference between this group and the 25–34 group, although for both

TABLE 2—EQUATIONS (4): REGRESSION COEFFICIENTS, STANDARD ERRORS, AND COEFFICIENTS OF DETERMINATION

	(1) $\bar{c}_{mi}$	(2) c_{1i}	(3) c_{2i}	(4) c_{3i}	(5) R^2
Male					
14–19	−.0971	.1214 (.0086)	3,210 (680)	.4551 (.0538)	.6796
20–24	−.0072	.0989 (.0086)	−3,820 (690)	.3931 (.0773)	.9244
25–34	−.0872	.0981 (.0093)	9,700 (1,570)	.4035 (.0913)	.9882
35–44	−.0331	.0793 (.0070)	2,480 (680)	.6096 (.1280)	.9583
45–54	−.0016	.0696 (.0049)	550 (150)	.4178 (.0682)	.8111
55–64	−.0289	.0277 (.0057)	1,430 (490)	.8691 (.3534)	.8669
65 and over	−.0768	.0514 (.0052)	5,260 (180)	.4286 (.0616)	.9624
Female					
14–19	−.0652	.0838 (.0075)	1,460 (650)	.3806 (.0547)	.7042
20–24	−.0135	.0280 (.0058)	−230* (200)	.4292 (.0427)	.6812
25–34	−.0095	.0550 (.0074)	−3,470* (1,990)	.4226 (.0997)	.9342
35–44	−.0322	.0797 (.0073)	−3,180 (460)	.5651 (.1410)	.6002
45–54	−.0472	.0755 (.0058)	−5,690 (270)	1.0894 (.0544)	.9820
55–64	−.0389	.0642 (.0054)	−4,720 (250)	1.0119 (.1066)	.9606
65 and over	.0200	.0355 (.0050)	−2,680 (570)	−.1509 (.0684)	.5447

groups employment fluctuates less than in other female groups.

The c_{2i} coefficients are a statistical corroboration of the well-known fact that secularly group employment ratios have declined for males and increased for females. The c_{3i} coefficients, which reflect relative population changes are, as expected, positive. The lone exception is the 65-and-over female group where the coefficient is small both absolutely and relative to its standard error.

IV. *Stationary Derivatives and Elasticities*

Interpretation of the results is facilitated by the computation of a set of stationary derivatives and elasticities. Although the a_{1i} coefficients are generally positive, indicating that participation will rise when employment rises, such a change is accompanied by an offsetting additional-worker effect represented by the exhaustions ratio and we cannot, therefore, tell from inspecting Table 1 what the net effect will be. To remedy this circumstance we calculate the net effect of an instantaneous change in employment upon labor force participation. Substitution of the stationary form of equation (2) into equation (3), given the net effect of a change in total employment upon group labor force participation:

$$\frac{dL_i}{dE} = \frac{a_{1i}(1 - b_2) + a_{2i}b_1}{1 - b_2}.$$

Similarly, the effect of a change in total employment on group employment is:

$$\frac{dE_i}{dE} = c_{1i};$$

and finally, the relationship between group employment and group labor force participation is:

$$\frac{dL_i}{dE_i} = \frac{dL_i}{dE}\cdot\frac{dE}{dE_i} = \frac{a_{1i}(1 - b_2) + a_{2i}b_1}{c_{1i}(1 - b_2)}.$$

Numerical estimates are recorded in Table 3. Instead of reporting the actual derivatives, the table is arranged so that the effect of an increase in total employment of 1,000 may be observed. In column 1 we show the way the change in employment is distributed; column 2 presents the associated change in labor force participation; and column 3 shows the reduction in unemployment.

The asterisks in column 2 indicate that the computation is not based on a statistically significant relationship. Whether we assume that the regression coefficient a_{1i} is zero for these two male groups or is equal to

TABLE 3—EFFECT OF A RISE IN TOTAL EMPLOYMENT OF ONE THOUSAND ON GROUP EMPLOYMENT AND LABOR FORCE PARTICIPATION

	(1) Change in Employment[1]	(2) Change in Labor Force	(3) Reduction in Unemployment
Male			
14–19	125	88	37
20–24	102	26	76
25–34	101	2*	99
35–44	82	−3*	85
45–54	72	5	67
55–64	29	−9	38
65 and over	53	39	14
TOTAL	564	148	416
Female			
14–19	87	81	6
20–24	29	12	17
25–34	57	26	31
35–44	82	47	35
45–54	78	54	24
55–64	66	54	12
65 and over	37	32	5
TOTAL	436	306	130
GRAND TOTAL	1,000	454	546

[1] The c_{it} coefficients have been multiplied by a factor of 1.033 in order to make the total increase in employment equal 1,000.

the computed value makes little difference. The effect on labor force participation of a change in employment for these two male groups is negligible.

Correct interpretation of the results necessitates recognition of the fact that the reported values are averages for the period as a whole. They should not be used to estimate the employment and labor force changes for any specific year. The averages show considerable variation in labor force participation among men aged 14–24 and for the 65-and-over group. The result for the male 55–64 group, although quantitatively small, is extremely interesting.[6] This group is the only one in which the additional-worker effect overcomes the discouraged-worker effect. The result can be interpreted in a number of ways. First, it is possible that recession causes a disruption in the normal flow of retirements. Male workers in the 55–64 age group may utilize their seniority privileges to

[6] The regression coefficient a_{it} is not statistically significant in the male 55–64 age group. If we calculate the change in labor force participation, assuming that the true value is the .0107 reported in Table 1, we get the change in labor force participation of −9 reported in Table 3. On the other hand, if we assume the value of the coefficient to be zero and re-estimate the parameters with the employment ratio eliminated, we obtain a value of −13.

extend their working careers in order to assist hard-pressed relatives during times of economic adversity. Such workers may therefore be thought of as effecting a transition to additional worker without ever losing their jobs. Alternatively, it is possible that recession induces some persons who have retired early to return to the labor force.

For women there are sizable fluctuations in labor force participation in all groups. Among the youngest women, an increase in employment is accompanied by practically a one-for-one change in labor force participation. As is shown in column 3, it is practically impossible by increasing total employment to reduce unemployment in the female 14–19 group. The lowest change in participation is in the 20–24 bracket; the rise in labor force participation with respect to changes in employment then rises steadily as we proceed along the age spectrum.

The rise in employment of 1,000 is accompanied by a rise in employment of males of 564 and an increase in labor force participation of 148. Unemployment therefore declines by 416. Similarly, a rise in total employment of 1,000 increases female employment by 436 and leads to an increase in participation of 306. Thus the reduction in unemployment is only 130. Finally, the totals shown at the bottom of the table indicate that a total of 454 persons enter the labor force when employment rises by 1,000, which implies that unemployment declines by only 546.

To avoid misleading impressions that could arise from different-sized class intervals and from different-class population values, and to estimate the responsiveness of labor force participation to changes in employment, it is useful to convert the derivatives into elasticities. We define:

$$e_1 = \frac{dE_i}{dE}\frac{E}{E_i}, \qquad e_2 = \frac{dL_i}{dE}\frac{E}{L_i}, \qquad \text{and} \qquad e_3 = \frac{e_2}{e_1} = \frac{dL_i}{dE_i}\frac{E_i}{L_i}.$$

The derivatives are the same as before. The absolute values of E, E_i, and L_i are the average values of these variables over the period covered by the data.

The numerical values of the derivatives are reported in Table 4. The elasticity of individual group employment with respect to total employment exhibits the saucer shape that we expect, i.e., it is highest for the very young and the very old and low in the middle ranges. This elasticity is also higher for women than for men. The elasticity of group labor force participation with respect to total employment (e_2) is also saucer-shaped, and the same is true for the elasticity of group labor force participation with respect to group employment (e_3).

The most striking result that emerges from inspection of these elasticities is that where group employment is highly responsive to changes in total employment, labor force participation is also highly responsive. The result suggests that the likelihood of discouragement and with-

TABLE 4—ELASTICITY OF GROUP EMPLOYMENT WITH RESPECT TO TOTAL EMPLOYMENT (e_1), GROUP LABOR FORCE WITH RESPECT TO TOTAL EMPLOYMENT (e_2), AND GROUP LABOR FORCE WITH RESPECT TO GROUP EMPLOYMENT (e_3)

	e_1	e_2	e_3
Male			
14–19	2.918	1.796	.615
20–24	1.978	.460	.233
25–34	.648	.012*	.019*
35–44	.502	−.018*	−.036*
45–54	.525	.035	.067
55–64	.307	−.093	−.303
65 and over	1.471	1.037	.705
TOTAL	.832	.208	.250
Female			
14–19	2.865	2.372	.828
20–24	.797	.308	.386
25–34	.925	.400	.432
35–44	1.097	.598	.545
45–54	1.133	.754	.665
55–64	1.677	1.317	.785
65 and over	3.001	2.543	.847
TOTAL	1.347	.895	.664
GRAND TOTAL	1.000	.431	.431

drawal from the labor force is closely related to the degree of job security that workers normally enjoy.

V. *Full-Employment Labor Force*

We have shown that the recorded magnitude of the labor force during times of economic slack is considerably lower than it would have been had the economy been at full employment. It follows that the recorded level of unemployment for most age groups understates both the magnitude of the productive manpower loss that is created by recession and the additional jobs that are needed to restore full employment.

Following current usage we define "low full employment" and "high full employment" as situations that obtain when the measured seasonally adjusted aggregate unemployment rate is 4 and 3 per cent respectively. Full-employment labor force then becomes the size labor force which is associated with these respective unemployment rates. In what follows we concentrate on the low full-employment "criterion" rate of 4 per cent.

The problem of estimating the full-employment labor force is complicated by the presence of secular trends in the labor force participation ratios. Were it not for these trends we could utilize the stationary derivatives that were reported in Table 3 to compute full-employment labor

force. This could be done as follows: From the definition of the unemployment rate, $u=(L-E)/L$, we can derive:

$$dE = \frac{L}{(1-u)\frac{dL}{dE} - 1}(du).$$

For 1962 $L=71{,}854$ and $u=.056$. The required change in u is therefore $-.016$, and since $dL/dE=.454$, the required change in employment is 2,013 thousand. This would bring total employment to a level of 69,852 thousand. Since the accompanying change in labor force participation would be 914, total labor force would rise to 72,768 and the unemployment rate would be reduced to 4 per cent. The full-employment labor force estimate for 1962 would then be 72,768.

Calculated in the foregoing manner, the full-employment labor force estimates would overstate full-employment labor force for the early years of the period, underestimate it for the later years, and serve as a correct approximation only for the middle years. The cause of the discrepancy is the rising trend in the participation ratios that have been observed. Over the decade labor force participation has risen more rapidly than employment. Consequently, the unemployment rate has shown a secular increase, and the gap between actual labor force and full-employment labor force has grown wider. Had there been no secular trend in the unemployment rate, the procedure subsequently to be introduced would yield results identical to calculations based on average (stationary) values.

To make the project manageable and to convert our figures to a seasonally adjusted basis, we first remove the shifting monthly intercepts and treat them as constants by aggregating the equations over any consecutive 12-month period and dividing the resulting expressions by 12. The procedure, ignoring error terms, yields:

$$\text{(3a)}\quad \left(\frac{\bar{L}_i}{P}\right)_t = \bar{a}_i + a_{1i}\left(\frac{\bar{E}}{P}\right)_t + a_{2i}\left(\frac{\bar{X}}{P}\right)_{t+2} + a_{3i}\left(\frac{\bar{1}}{P}\right)_t + a_{4i}\left(\frac{\bar{P}_i}{P}\right)_t,$$

and:

$$\text{(4a)}\quad \left(\frac{\bar{E}_i}{P}\right)_t = \bar{c}_i + c_{1i}\left(\frac{\bar{E}}{P}\right)_t + c_{2i}\left(\frac{\bar{1}}{P}\right)_t + c_{3i}\left(\frac{\bar{P}_i}{P}\right)_t,$$

and where, for example,

$$\left(\frac{\bar{L}_i}{P}\right)_t = \frac{1}{12}\sum_{m=1}^{12}\left(\frac{L_i}{P}\right)_m, \quad \text{and} \quad \bar{a}_i = \frac{1}{12}\sum_{m=1}^{12} a_{im},$$

and t now dates the beginning of the seventh month of the 12-month period. This procedure transforms all of the variables into seasonally adjusted values obtained by a simple 12-month moving average. When the first month of the summation is January, the result provides an annual average.

If the aggregate employment and exhaustions ratios that are associated with an over-all 4 per cent unemployment rate can be found, they can be substituted into equations (3a) and (4a) in order to obtain the group labor force and employment ratios that are associated with low full employment. In deriving these estimates it is useful to resort to the global model.

After undertaking the same seasonal adjustment procedure as above, equations (1) and (2) become:

$$\text{(1b)} \qquad \left(\frac{\bar{L}}{P}\right)_t = \bar{a} + a_1\left(\frac{\bar{E}}{P}\right)_t + a_2\left(\frac{\bar{X}}{P}\right)_{t+2} + a_3\left(\frac{1}{P}\right)_t,$$

and;

$$\text{(2b)} \qquad \left(\frac{\bar{X}}{P}\right)_t = \bar{b} + b_1\left(\frac{\bar{E}}{P}\right)_{t-1} + b_2\left(\frac{\bar{X}}{P}\right)_{t-1}.$$

When these expressions are combined with the criterion function

$$\text{(5)} \qquad \bar{E}_t = (1 - \hat{u})\bar{L}_t,$$

where $\hat{u}$ is the full-employment criterion unemployment rate, and the population function:

$$\text{(6)} \qquad P_t = P_0(1 + r)^t,$$

there emerges the second-order nonhomogeneous difference equation:

$$\text{(7)} \qquad A_0\left(\frac{\bar{L}}{P}\right)_{t+1} + A_1\left(\frac{\bar{L}}{P}\right)_t + A_2\left(\frac{\bar{L}}{P}\right)_{t-1} = A_3 + \frac{A_4}{P_0(1 + r)^t},$$

where:

$$A_0 = a_2 b_1(1 - \hat{u}),$$
$$A_1 = -\left[1 - a_1(1 - \hat{u})\right],$$
$$A_2 = b_2\left[1 - a_1(1 - \hat{u})\right],$$
$$A_3 = \bar{a}(b_2 - 1) - a_2\bar{b}, \text{ and}$$
$$A_4 = \left[a_3[b_2(1 + r) - 1]\right](1 + r).$$

Equation (7) has no stationary solution. However, it is possible to solve for the dynamic equilibrium path in order to obtain the full-employment labor force participation ratio as a function of time, the

estimated parameters, and whatever unemployment rate we wish to pick as our full-employment criterion. The solution is:

$$(8)\quad \left(\frac{\overline{L}}{P}\right)_t^* = \frac{A_3}{A_0 + A_1 + A_2} + \left[\frac{A_4}{A_0 + (1+r)A_1 + (1+r)^2 A_2}\right]\left[\frac{1}{P_0(1+r)^t}\right].$$

The employment ratio that is associated with full employment follows immediately. It is:

$$(9)\quad \left(\frac{\overline{E}}{P}\right)_t^* = (1 - \hat{a})\left(\frac{\overline{L}}{P}\right)_t^*.$$

Finally, by returning to equations (1b) and (5) we obtain:

$$(10)\quad \left(\frac{\overline{X}}{P}\right)_{t+2}^* = -\frac{A_1}{a_2}\left(\frac{\overline{L}}{P}\right)_t^* - \frac{\bar{a}}{a_2} - \frac{a_3}{a_2}\left(\frac{1}{P}\right)_t$$

for the full-employment exhaustions ratio.

Substitution of equations (9) and (10) into the set of equations (3a) provides the full-employment labor force estimates for the 14 groups. These full-employment labor force estimates, calculated on the basis of a 4 per cent criterion unemployment rate, are reported in Table 6. For purposes of comparison Table 5 presents the actual recorded values for labor force. Similarly, Table 7 reports actual unemployment rates, and the "manpower gap" unemployment rates are reported in Table 8. The manpower-gap rates are computed by subtracting actual employment from full-employment labor force and by dividing the resulting "manpower gap" (the sum of actual plus hidden unemployment) by full-employment labor force.

The main results can be summarized quite simply:

1. For the individual groups the excess of the gap unemployment rates over the actual unemployment rates is greatest in those groups that have the highest elasticity of labor force participation with respect to employment. Accordingly there is almost no difference between the actual and the gap rates for males 25–64. However for men 65 and over the gap rate, in recent years, far exceeds the actual unemployment rate, and for males 14–19 and 20–24 the gap rates exceed the already large actual rates.

2. The very high gap rates in recent years for the 20–24 group are clear evidence that the economy has not been creating new jobs at a rate that is high enough to absorb many of the potential new entrants into the labor force. There is a tendency to discount the enormously high rates in the 14–19 group with the argument that these boys should be

TABLE 5—LABOR FORCE: ANNUAL AVERAGES
(Thousands of persons)

	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Male										
14–19	2,880	2,868	2,933	3,098	3,101	3,104	3,272	3,423	3,512	3,549
20–24	3,054	3,052	3,220	3,485	3,626	3,771	3,940	4,125	4,255	4,279
25–34	10,737	10,771	10,806	10,685	10,571	10,471	10,347	10,252	10,176	9,922
35–44	10,436	10,513	10,595	10,663	10,731	10,843	10,899	10,967	11,012	11,115
45–54	8,570	8,702	8,838	9,002	9,153	9,319	9,438	9,574	9,667	9,715
55–64	5,974	6,104	6,121	6,220	6,222	6,304	6,345	6,400	6,530	6,560
65 and over	2,544	2,525	2,525	2,603	2,477	2,379	2,322	2,287	2,220	2,241
Female										
14–19	1,946	1,933	1,981	2,176	2,191	2,164	2,244	2,402	2,559	2,605
20–24	2,428	2,424	2,444	2,455	2,442	2,500	2,473	2,580	2,697	2,801
25–34	4,162	4,212	4,251	4,276	4,254	4,193	4,089	4,131	4,143	4,103
35–44	4,662	4,709	4,804	5,031	5,116	5,185	5,228	5,302	5,398	5,474
45–54	3,679	3,822	4,153	4,405	4,615	4,859	5,081	5,278	5,403	5,381
55–64	2,049	2,163	2,391	2,610	2,630	2,727	2,883	2,986	3,105	3,197
65 and over	693	665	779	821	813	822	836	907	926	912
Totals:										
Male	44,195	44,535	45,038	45,756	45,881	46,191	46,563	47,026	47,372	47,381
Female	19,619	19,928	20,803	21,774	22,061	22,450	22,834	23,586	24,231	24,473
All	63,814	64,463	65,841	67,530	67,942	68,641	69,397	70,612	71,603	71,854

TABLE 6—LOW FULL-EMPLOYMENT LABOR FORCE: ANNUAL AVERAGES
(Thousands of persons)

	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Male										
14–19	2,939	3,001	3,048	3,102	3,222	3,352	3,476	3,629	3,880	4,011
20–24	3,047	3,099	3,278	3,496	3,675	3,839	3,983	4,141	4,351	4,463
25–34	10,759	10,788	10,781	10,700	10,606	10,508	10,359	10,250	10,173	9,957
35–44	10,436	10,514	10,592	10,699	19,733	10,838	10,903	10,971	11,007	11,100
45–54	8,594	8,717	8,850	8,994	9,155	9,314	9,457	9,608	9,707	9,746
55–64	6,016	6,071	6,122	6,172	6,227	6,276	6,331	6,406	6,483	6,568
65 and over	2,561	2,556	2,558	2,547	2,508	2,465	2,443	2,402	2,333	2,442
Female										
14–19	1,980	2,017	2,059	2,138	2,274	2,377	2,529	2,586	2,839	3,006
20–24	2,463	2,427	2,421	2,422	2,451	2,501	2,559	2,630	2,724	2,831
25–34	4,194	4,235	4,258	4,262	4,256	4,217	4,163	4,187	4,223	4,244
35–44	4,679	4,779	4,885	4,997	5,120	5,232	5,332	5,466	5,589	5,702
45–54	3,726	3,922	4,141	4,384	4,648	4,918	5,164	5,428	5,646	5,633
55–64	2,076	2,255	2,414	2,550	2,701	2,837	2,978	3,150	3,317	3,398
65 and over	701	733	766	802	847	888	922	973	1,036	1,073
Totals										
Male	44,352	44,746	45,229	45,680	46,126	46,592	46,952	47,407	47,934	48,287
Female	19,819	20,368	20,944	21,555	22,297	22,970	23,647	24,420	25,374	25,887
All	64,171	65,114	66,173	67,235	68,423	69,562	70,599	71,827	73,308	74,174

in school anyway;[7] but one cannot dismiss the problem of the 20–24 group in this manner.

3. Among women the gap rates since 1958 have been uniformly higher than the actual rates. The large disparities that occur after 1960 reflect the fact that full-employment labor force participation has grown much more rapidly than female job opportunities.

4. The highest gap rates relative to the actual unemployment rates are found in those groups whose actual unemployment rates are already high relative to other groups. When labor force withdrawal is taken into account, the employment position of groups whose employment position is already unsatisfactory is accentuated relative to other groups.

5. Over the period covered by the data there is a fairly uniform tendency for the gap rates to increase both absolutely and relative to the measured unemployment rates. This results because the potential manpower pool has been growing more rapidly than the available number of jobs. The picture that emerges is one of steady deterioration in employment opportunities relative to requirements. The recovery of 1962 is barely perceptible in the gap rates, and in many cases the gap rates actually increased over the 1961 level.

6. For 1962 the level of unemployment for all men is 2,495 thousand and the manpower gap is 3,401 thousand. Hidden unemployment, defined as the difference between the manpower gap and measured unemployment, is therefore 906 thousand. For women the respective totals are 1,520, 2,934, and 1,414 thousands. For the labor force as a whole, therefore, hidden unemployment comes to 2,320 thousand. And these totals, it should be noted, are based upon the fairly unambitious definition of full employment as occurring when the over-all unemployment rate falls to 4 per cent. Had a 3 per cent unemployment rate been established as the full-employment criterion, the full-employment labor force, gap unemployment rates, and the level of hidden unemployment would all have been much higher.

VI. *Employment Requirements*

How many additional jobs would have been required in each group in order to reduce the over-all unemployment rate for 1962 to 4 per cent? According to our calculations, a 4 per cent unemployment rate implies a total labor force of 74,174. The level of employment would therefore

[7] As far as the economist is concerned, the fact that an individual is productive enough to be gainfully employed when the economy is operating at full capacity ought to be sufficient evidence that if he does not have a job one ought to be found or created for him. Moreover, school is unfortunately not the only alternative to work for young persons. Although the labor force participation rates for 16–17 year old males who are enrolled in school have remained fairly constant, participation rates for those who are not in school have fallen steadily in the last few years.

TABLE 7—UNEMPLOYMENT RATES: ANNUAL AVERAGES

	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Male										
14–19	6.7	11.1	9.4	9.6	11.3	15.3	13.8	14.1	15.3	13.4
20–24	4.3	9.8	7.0	6.3	7.7	12.7	8.7	9.0	10.8	8.9
25–34	1.9	4.4	3.1	3.0	3.3	6.5	4.7	4.8	5.8	4.5
35–44	1.7	3.7	2.8	2.3	2.8	5.1	3.4	3.8	4.6	3.6
45–54	1.9	3.9	3.0	2.7	3.3	5.3	4.1	4.1	4.9	3.9
55–64	2.4	4.0	4.1	3.2	3.5	5.5	4.5	4.6	5.7	4.6
65 and over	2.0	4.1	3.8	3.1	3.4	5.3	4.8	4.2	5.5	4.6
Female										
14–19	6.4	10.3	9.0	9.8	10.1	13.1	12.3	13.0	14.8	13.2
20–24	3.6	6.6	5.5	5.6	5.9	9.1	8.1	8.5	9.8	9.1
25–34	2.9	5.8	4.7	4.2	5.2	7.4	5.9	6.3	7.4	6.5
35–44	2.0	4.6	3.6	3.4	3.8	6.2	5.1	4.8	6.5	5.2
45–54	1.8	4.1	3.1	3.2	3.1	4.9	4.2	4.2	5.2	4.1
55–64	2.0	3.9	3.3	3.2	3.0	4.6	4.1	3.4	4.5	3.5
65 and over	1.3	2.4	1.8	2.3	3.4	3.8	2.8	2.8	4.0	4.1
Totals										
Male	2.4	4.8	3.9	3.5	4.1	6.8	5.3	5.4	6.5	5.3
Female	2.7	5.4	4.4	4.3	4.7	6.8	5.9	5.9	7.2	6.2
All	2.5	5.0	4.0	3.8	4.3	6.8	5.5	5.6	6.7	5.6

TABLE 8—GAP UNEMPLOYMENT RATES: ANNUAL AVERAGES

	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Male										
14–19	8.6	15.0	13.3	9.7	14.7	21.5	18.8	19.0	23.3	23.4
20–24	4.1	11.2	8.6	6.6	9.0	14.3	9.7	9.4	12.7	12.7
25–34	2.1	4.5	2.8	3.1	3.6	6.8	4.8	4.8	5.7	4.8
35–44	1.7	3.7	2.8	2.4	2.8	5.1	3.8	3.8	4.6	3.5
45–54	2.2	4.0	3.1	2.7	3.3	5.2	4.3	4.4	5.3	4.2
55–64	3.1	3.5	4.1	2.5	3.6	5.1	4.3	4.7	5.1	4.7
65 and over	2.7	5.2	5.0	1.0	4.6	8.6	9.5	8.8	10.1	12.5
Female										
14–19	7.6	14.0	12.4	8.2	13.4	20.9	22.2	19.1	23.2	24.8
20–24	5.0	6.7	4.6	4.3	6.3	9.1	11.2	10.2	10.7	10.0
25–34	3.6	6.3	4.8	3.9	5.2	7.9	7.6	7.6	9.1	9.6
35–44	2.3	6.0	5.2	2.8	3.9	7.0	7.0	7.7	9.7	9.0
45–54	3.1	6.5	2.9	2.7	3.8	6.1	5.8	6.9	9.2	8.4
55–64	3.2	7.8	4.2	0.9	5.5	8.3	7.2	8.4	10.6	9.2
65 and over	2.4	11.5	0.1	0.0	7.3	10.9	11.8	9.4	14.2	18.5
Totals										
Male	2.8	5.3	4.3	3.4	4.6	7.6	6.1	6.2	7.6	7.0
Female	3.7	7.4	5.0	3.4	5.7	8.9	9.1	9.1	11.4	11.3
All	3.1	6.0	4.5	3.4	5.0	8.1	7.1	7.2	8.9	8.5

have had to rise to 71,207 thousand. Since the actual level of employment was 67,839 thousand, the total number of additional job requirements was 3,368 thousand. The distribution of these additional job requirements can be calculated by making appropriate substitutions into equations (4a). The calculations are shown in Table 9. Column 1 reports full-employment labor force. Column 2 gives the actual employment values. Column 3 gives the distribution of employment when the overall unemployment rate is 4 per cent. And column 4 shows the distribution of the additional employment requirements.

Inspection of Table 9 indicates that an additional 3,368 thousand jobs are required merely to reduce the unemployment rate to 4 per cent. If a rise in employment is not accompanied by a change in labor force participation so that the 1962 labor force total of 71,854 remains frozen when employment increases, full employment would require 68,980 thousand jobs, which would mean the addition of only 1,141 thousand more jobs. If we call this figure the "overt" additional requirement, the "hidden" additional requirement, defined as the difference between the total additional requirement and the overt requirement, comes to 2,217 thousand.

TABLE 9—FULL-EMPLOYMENT REQUIREMENTS FOR 1962
(Thousands of persons)

	(1) Full Employment Labor Force	(2) Actual Employment	(3) Full Employment	(4) Employment Requirements
Male				
14–19	4,011	3,073	3,548	475
20–24	4,463	3,898	4,292	394
25–34	9,957	9,475	9,756	281
35–44	11,100	10,710	10,893	183
45–54	9,746	9,333	9,580	247
55–64	6,568	6,259	6,350	91
65 and over	2,442	2,138	2,339	201
Female				
14–19	3,006	2,261	2,597	336
20–24	2,831	2,547	2,603	56
25–34	4,244	3,836	4,039	203
35–44	5,702	5,190	5,449	259
45–54	5,633	5,158	5,436	278
55–64	3,398	3,086	3,298	212
65 and over	1,073	875	1,027	152
TOTALS				
Male	42,287	44,886	46,758	1,872
Female	25,887	22,953	24,449	1,496
All	74,174	67,839	71,207	3,368

VII. *Residual Unemployment*

If the over-all unemployment rate had been at the low full-employment level of 4 per cent in 1962, what would have been the status of the individual groups? Subtraction of the entries in the "full employment" column of Table 9 from the corresponding full-employment labor force figures gives the remaining level of unemployment in each group. These "residual" unemployment levels are then divided by the respective full-employment labor force values in order to arrive at the unemployment rates for the individual groups that are associated with an over-all 4 per cent unemployment rate. These "residual unemployment rates" are reported in Table 10.

TABLE 10—RESIDUAL UNEMPLOYMENT RATES AT LOW FULL EMPLOYMENT 1954 AND 1962

	Male		Female		Both Sexes	
	1954	1962	1954	1962	1954	1962
14–19	10.6	11.6	10.2	13.6	10.4	12.4
20–24	5.9	3.8	5.3	8.1	5.6	5.5
25–34	3.4	2.0	4.2	4.8	3.6	2.9
35–44	2.9	1.9	3.6	4.5	3.2	2.7
45–54	2.9	1.7	2.9	3.5	2.9	2.4
55–64	3.4	3.3	3.3	3.0	3.3	3.2
65 and over	3.2	4.3	2.0	4.3	3.0	4.3
TOTAL	3.8	3.2	4.4	5.6	4.0	4.0

A glance at the residual unemployment rates indicates that a reduction of the over-all unemployment rate to 4 per cent will still leave many groups with fairly high levels of unemployment. Although the position for most male groups would be satisfactory, the unemployment rate for males 14–19 would remain alarmingly high. For women the situation is less satisfactory relative to the comparable male class in all but two cases.

A comparison of the distribution of the residual unemployment rates for 1962 with comparable calculations for 1954 brings some important trends to light. The comparisons are presented in Table 10. A glance at the table confirms that for the males in the prime working ages, 20–64, the unemployment rates are lower than they were in 1954, while the opposite is, of course, the case for the extreme age groups. The table also shows the presence of a similar, though less-pronounced, pattern for women and therefore also when the data for both sexes are pooled. The totals in the bottom row show that the spread between the residual unemployment rates for men and women has widened considerably. Therefore if the economy had succeeded in reducing the unemploy-

nent rate to 4 per cent in 1962, additional employment would have had to come to a larger extent from women than from men and from the extremes of the age spectrum as opposed to the middle.

Unemployment rates may be somewhat misleading because a high unemployment rate for a particular group does not necessarily imply the presence of a large pool of available manpower. It is instructive therefore to compute "relative residual unemployment" levels that are associated with an over-all 4 per cent unemployment rate. By relative unemployment is meant that fraction of total unemployment that will be present in the group. As is shown in Table 11, the comparison between 1954 and 1962 for males becomes even more dramatic than before. In 1954 almost 36 per cent of residual unemployment would have

TABLE 11—RELATIVE RESIDUAL UNEMPLOYMENT AT LOW FULL EMPLOYMENT 1954 AND 1962

	Male		Female		Both Sexes	
	1954	1962	1954	1962	1954	1962
14–19	12.3	15.6	7.9	13.7	20.1	29.3
20–24	7.0	5.7	5.0	7.7	11.9	13.4
25–34	13.9	6.8	6.9	6.9	20.8	13.7
35–44	11.9	7.0	6.6	8.5	18.5	15.5
45–54	9.8	5.6	4.4	6.7	14.2	12.3
55–64	7.8	7.4	2.8	3.4	10.7	10.7
65 and over	3.2	3.5	.6	1.5	3.8	5.0
TOTAL	65.9	51.6	34.1	48.4	100.0	100.0

been men between the ages of 25 and 54, whereas for 1962 the total would have been only 19 per cent. Similarly, when the totals for men and women are computed, it appears that in 1954 residual unemployment would have been split in a ratio of 66 to 34 among men and women respectively, whereas by 1962 the division would have been almost equal.

Over the period for which the comparison has been made we have had a relative rise in full-employment labor force participation in those parts of the labor force that may be relatively less employable. On the one hand, this means that the achievement of a 4 per cent unemployment rate would put males in the prime working ages into an extremely favorable employment position. On the other hand, these trends may be interpreted as indicating that it might have become somewhat more difficult to reduce the over-all unemployment rate. The case is by no means clear-cut; however, if manpower must be drawn from the young (who probably require more training than the average worker), the old

(whose productivity may be lower), and from women (who legally can no longer be paid lower wages than men), the expansion of employment might raise costs and prices more rapidly than would have been the case had there been an available pool of males in the prime working ages.

We must emphasize most strongly that the discussion of this section is based on the pattern that would have emerged had the economy been at full employment. Since we remain a considerable distance away from that goal, it would be a mistake to infer from the discussion that demand expansion will pull up costs and prices because of a change in the structure of the labor force. Indeed, if a policy conclusion emerges from the analysis, it is that the enormous employment requirements that we face call for demand expansion on a large scale.

VIII. *Summary*

We have attempted to test the discouraged- and additional-worker hypotheses of labor force participation and have found each hypothesis to be consistent with the data in 11 of the 14 groups. Labor force participation is virtually autonomous for males 25–64. However, for the younger and older male groups and for all female groups, participation responds to changes in the level of employment. The direction of change in all cases except for males 55–64 is such that a rise in employment is accompanied by a rise in labor force participation. The responsiveness of labor force participation to changes in employment is greatest for the groups in which the elasticity of group to total employment is high. An increase in employment of 1,000 brings forth additional labor force participation of 454, on the average, so that the fall in unemployment is only 546.

Calculations of full-employment labor force and gap unemployment rates confirm the presence of considerable discrepancies between actual and full-employment labor force and between actual and gap unemployment rates for many groups. For 1962 the over-all gap rate is 8.5 per cent as opposed to an actual rate of 5.6 per cent. The discrepancies are particularly great for the very young and the very old and show a rising trend for females.

The attainment of a 4 per cent unemployment rate for 1962 would have necessitated the creation of over three million additional jobs. Had this additional employment been forthcoming, the remaining unemployment would have been split about evenly between males and females. This contrasts with a split of 66 per cent to 34 per cent for 1954. Also since 1954 there has been a relative decline in the residual unemployment rates for the middle ranges of the age spectrum and a corresponding increase at the extremes. The economy has not yet been able to

adjust to the sizable shifts in the structure of the labor force that have taken place over the decade.

REFERENCES

1. W. G. BOWEN AND T. A. FINEGAN, "Labor Force Participation and Unemployment," in A. M. Ross, ed., *Employment Policy and the Labor Market*, Berkeley 1965, pp. 115–61.
2. K. STRAND AND T. DERNBURG, "Cyclical Variation in Civilian Labor Force Participation," *Rev. Econ. Stat.*, Nov. 1964, *46*, 378–91.
3. A. TELLA, "The Relation of Labor Force to Employment," *Indus. Lab. Rel. Rev.*, April 1964, *17*, 454–69.
4. ———, "Labor Force Sensitivity to Employment by Age, Sex," *Indust. Rel.*, Feb. 1965, *4*, 69–83.

CONCENTRATION AND LABOR EARNINGS

By LEONARD W. WEISS*

There have been periodic suggestions in the literature that concentrated industries may tend to pay exceptional wages [3] [6] [8] [10] [14] [16] [17]. If such a hypothesis could be given convincing support, it would have important implications for the evaluation of industrial performance. It would mean that the sum of monopoly rents would exceed greatly those identified in examinations of corporate profits. The resulting misallocation of resources would be understated even more seriously since the welfare loss due to monopoly pricing increases as the square of the margin between price and the opportunity cost of factors employed [7].

It has proved difficult to test for monopoly rents accruing to labor in concentrated industries because of the large number of variables pertinent to wage determination, many of which are correlated with industry structure. A number of studies have attempted to circumvent this difficulty by investigating wage changes [6] [8], but these really tested the distinct hypothesis that concentrated industries continuously increase wages relative to unconcentrated industries. Another investigator attempted to test the "monopoly wage" hypothesis by comparing the ratios of Canadian to U. S. wages in industries that were concentrated in Canada but not in the United States with other industries that were unconcentrated in both countries. His whole argument ultimately depended on nine mostly minor industries [13]. Altogether, it is doubtful whether many have been convinced by the evidence to date of either the presence or absence of monopoly rents in the wage payments of concentrated industries.

This paper attempts a new, direct test of the "monopoly wage" hypothesis using the 1/1000 sample of the 1960 Census of Population. The paper is divided into five parts. The first briefly reviews possible hypotheses. Part II describes the data; Part III presents the detailed results of multiple regression studies for semiskilled labor; and Part IV summarizes similar results for a number of other occupation groups. Part V presents some conclusions.

* The author is professor of economics at the University of Wisconsin. This paper was prepared under a grant from the Graduate School of the University of Wisconsin. Computations were partially financed by the Social Systems Research Institute and by the National Science Foundation. Comments by Glen Cain, Hirschel Kasper, H. Gregg Lewis, and Gerald Somers were very helpful in the preparation of the study.

I. *The Hypothesis*

The notion that monopoly on product markets leads to high payments for hired factors does not arise automatically from the concept of a profit-maximizing firm. Indeed, simple theory might suggest low rather than high wages in concentrated industries since output restriction would carry with it a restriction of employment. It is quite possible to find plausible arguments to support the opposite hypothesis, however. The profit-maximization assumption need not yield equally good predictions in all areas. Pecuniary profit motives might well be weaker in wage policy than in other business decisions. While high profits won by technical advance or effective market forecasts often enjoy general approval, high profits attained by paying meager wages almost certainly will win censure for the entrepreneur. A relatively profitable firm might well pay above-average wages to avoid such opprobrium, i.e., firms with exceptional profits might choose to use part of their earnings to buy public approval. This might be particularly true of firms with monopoly power because of their vulnerability to a bad press.

Even on the assumption of unequivocal profit-maximization, a high-wage policy might be plausible. Industries with high profits, whether due to monopoly or other causes, might attract trade unionism since they offer large prizes to the successful organizer. High wages might then result from the unionization of profitable industries or from managerial attempts to forestall such organization. Less-profitable industries might pay lower wages because the threat of organization might be less and/or because the firm might be less willing to pay to keep the union weak.

The increase in costs associated with a high-wage policy would probably not be as great as the increase in hourly wage rates in any event, since low wages would tend to result in high turnover, poor discipline, and poor-quality labor. A large, profitable firm which did pay high wages to buy public favor and/or to limit union power would have to pay less per "productivity unit" than per man-hour for its purchase.

In a perfectly competitive labor market an employer paying a high annual wage would get correspondingly superior workers. High earnings would not imply monopoly rents since laborers would still earn the values of their alternative products. While the tendency for labor quality to be related to earnings has long been recognized, some economists have doubted that it could fully account for interindustry wage differences. For instance, Sumner Slichter argued that if employers actually believed that higher wages brought them proportionately higher quality labor, the employers under the greatest competitive pressure would pay high rather than low wage rates [15, p. 90].

This paper tries to test two hypotheses: (1) that concentrated industries pay high annual rates for labor of particular "occupations"; and

(2) that these high earnings are more than can be accounted for by the personal characteristics of the labor employed. Most previous studies have been limited by the data to the first hypothesis. The 1/1000 Census tape makes it possible to test both.

Since concentration and unionization are closely related, they must both be considered. The effect of unionism with varying degrees of product market monopoly may be complex. In unconcentrated industries, strong union power might yield high wages, but in concentrated industries, where wages may already be high, unions may not add much. For one thing, if high wages in unorganized but concentrated industries result from the threat of unionism, then concentration and unionization would represent the same force. If this is true, the combined effects of concentration and unionization would be less than the sum of the effects of the two taken separately. In addition, unions seem likely to have greater bargaining power where they are the source of monopoly power on product markets—e.g., coal or construction—than where market power would exist without them—e.g., automobiles or steel [11, p. 138].

On the basis of these arguments, this study uses the model:

$$Y = b_0 + b_1 CR + b_2 U + b_3 U \cdot CR \quad (1)$$

where Y is annual earnings, CR is the concentration ratio, and U is the extent of collective bargaining coverage, to test for the existence of "monopoly wages" in concentrated and/or unionized industries.[1] The above argument suggests positive signs for b_1 and b_2 and a negative sign for b_3. The monopoly-wage hypothesis would be supported in some form if b_1 were positive and large relative to b_3. It would also be supported if b_1 and b_3 were both positive or, even if b_1 were negative, if b_3 were large and positive. The monopoly-wage hypothesis would have to be rejected if b_1 and b_3 were both negative or if either b_1 or b_3 were negative and large relative to the other.[2]

Concentration and/or unionization may be proxies for other industry characteristics. These other variables must also be introduced.

1. One might expect industries with rapidly increasing employment to show higher earnings than others, on the assumption that relative changes in employment opportunities are not perfectly foreseen by workers and that the short-run supply of labor in particular industries is not perfectly elastic. In effect, we assume that an increase in employ-

[1] This model had been formulated before the author saw the similar results of H. G. Lewis, [9, pp. 161, 178].

[2] Since the partial derivative of Y with respect to CR is $\partial Y/\partial CR = b_1 + b_3 U$, the net effect of CR on Y will change sign when $U = -b_1/b_3$. If this number falls between 0 and +100 (the range of U as it will be measured here), the monopoly-wage hypothesis will be confirmed over only a portion of the feasible range of U. It will be confirmed over the higher values of U where b_3 is positive and the lower values of U where b_1 is negative. By similar reasoning, the effect of unionization on earnings would change sign where $CR = -b_2/b_3$.

ment in any one industry represents an increase in demand rather than supply. This appears reasonable when large groups of workers employable in many different industries are involved.

2. There may be some justification for distinguishing durable- from nondurable-goods industries. The former are less stable over the cycle and would yield higher earnings in boom years to offset long periods of unemployment. In addition, there is a tradition that durable-goods industries require heavier work presumably implying better pay.

3. It is commonly supposed that large employers pay high wages. At first glance one might expect large employers to be associated with monopsony power, but this need not be so because they tend to be located in large labor markets. Many of the arguments for the monopoly-wage hypothesis can also be used to argue for high earnings in large shops.

4. Some argue that the over-all labor force characteristics affect industry wage rates paid [15]. E.g., earnings of a male worker would be lower in an industry with a large proportion of female workers than in one where most workers were male. This might occur if personnel policies or collective bargaining agreements apply the terms appropriate to the main core of the work force to all workers in a shop. Again, if all firms in an industry must meet one another's product prices, they may be forced by competition into similar wage policies. E.g., a firm in the North may pay low wages if a large proportion of its competitors are in the South. The five labor force characteristics for which this sort of argument seems most plausible are the percentages of the labor force that are (a) male, (b) skilled, (c) white, (d) in the South, and (e) of nonurban residence.

Finally, there are many personal characteristics important in the determination of labor earnings. There seem to be good reasons to expect that a person's age, race, region, urban or rural residence, mobility, education, intelligence, health, strength, manual dexterity, and appearance will affect his potential earnings in alternative employments, and therefore the minimum payments that his employer must make to get his services. Since the purpose of this paper is to determine the extent of monopoly rents included in labor earnings, it is important to introduce these personal variables. Even if earnings were high in concentrated or heavily unionized industries, workers would not be earning monopoly rents if their exceptional incomes could be wholly accounted for by exceptional personal attributes.

II. *The Data*

The dependent variable throughout this study is the private wage and salary income in 1959 reported by individuals in the 1/1000 sample

of the 1960 Census (see [4]). The populations used consist of all persons of specified occupation and sex employed for more than 13 weeks during 1959 and reporting their main employment in April, 1960 in (a) mining, construction, or manufacturing, or in (b) mining, construction, manufacturing, transportation, communications, or public utilities.[3] The regulated industries were excluded from population (a) because concentration and unionization seemed likely to have different effects on wages with and without regulation. When population (b) was used, a dummy variable was introduced with a value of one when the industry involved was regulated. Agriculture, distribution, and services were excluded from the study because the large numbers self-employed and, at least in agriculture, the large amounts of nonwage income made it difficult to compare earnings with other industries.

The year 1959 is an adequate, though not perfect, year for our purposes. It falls at the end of a long, stable prosperity, so adjustments to the open inflation of the 1940's should be largely complete. There is evidence that neither monopoly nor union power was fully exploited during the 1940's and that they were re-exerted during the 1950's [1] [3] [5] [8] [9, p. 193] [19]. If there were monopoly-wage advantages, therefore, they were more likely to show up in 1959 than in earlier years. On the other hand, 1959 annual earnings are affected by the unemployment remaining after the 1958 recession and, in certain industries, by the long 1959 steel strike. The effect of unemployment is at least partially corrected by including the numbers of weeks worked in 1959 as variables.

Some fuzziness is introduced because characteristics are reported as of April, 1960, while earnings refer to 1959. Most personal characteristics (e.g., sex or education) remain stable from one year to the next, but in some cases incomes are reported for persons with different 1959 industries, occupations, or residences from those used. The most serious error of this sort is probably in the number of hours worked. These refer to "last week" (usually in March or April, 1960) and can differ sharply from the average weekly hours in 1959.

Each individual is assigned the characteristics of the industry that he reported. For instance, everyone employed in the shoe industry is assigned the shoe industry's indexes of concentration, unionization, growth, plant size, durability, and labor force characteristics as well as his own personal characteristics. The five labor force variables are derived from the 1/1000 sample itself, and the employment-growth variable is the 1960 industry employment divided by the 1950 industry employment, both taken from Censuses of Population. Industry defini-

[3] Persons employed in industry 249, not specified metal industries, 326, not specified food industries, 459, not specified manufacturing, and 579, not specified utilities, were excluded.

tions involve no problems for these six variables.[4] However, the indexes of concentration, unionization, and employer size are based on weighted averages of the component three- and four-digit industry figures using employment reported in the 1960 *Annual Survey of Manufactures* or *Employment and Earnings* as weights.

Two concentration indexes are used. The simplest is just a weighted average of 1958 four-firm concentration ratios by four-digit product class. These are imperfect indexes of market power because of the varying regional character of the markets for some commodities and the varying extent of noncompeting subproducts for others. A second concentration index identified hereafter as CCR (corrected concentration ratio) is also computed using weighted averages of five-digit concentration ratios for noncompeting subproducts and an arbitrary scaling-up of national concentration ratios in industries with clearly regional or local markets.[5] These corrections are far from perfect, but the values of CCR should measure market power on a more nearly uniform basis in interindustry comparisons than those of the uncorrected average concentration ratios.[6]

The employer-size index is the percentage of employees in establishments with 250 or more employees in 1958. Establishments are used instead of firms because (1) employer size seems to be mainly a matter of his impact on local labor markets which depends on the size of his plants rather than his whole firm; (2) establishments are more accurately assigned to industries than firms are; and (3) because plant-size data are available for more industries than are firm-size data. The cut-off size of 250 employees is selected as a point by which impersonal employment relationships are likely to have developed. A further reason for using establishments rather than firms and for using 250 rather than a larger number of employees is to reduce the colinearity between the concentration and plant-size variables. There is undoubtedly some effect of employer bigness that cannot be distinguished from market concentration, but the measure used here does separate out important elements of employer size.

The existing measures of union power by industry are incomplete and often conflicting. Special estimates were developed for this study based on a wide range of sources. The BLS *Industry Wage Surveys* were taken as the basic source where available, and estimates based on other ma-

[4] In industry 347, "Textile Dyeing and Finishing, Except Wool and Knit Goods," the population Census figure is wildly different from the Annual Survey of Manufactures figures for the same years. In that case a special figure derived from the Annual Survey of Manufactures was used.

[5] The procedures used were approximately those used in [18].

[6] The corrections used are described in more detail in an appendix available on request from the writer, Economics Department, University of Wisconsin, Madison, Wisconsin 53706.

terials were adjusted to the concepts used in those Surveys.[7] Conceptually, union power in an industry is measured by the percentage of employees in establishments where more than half of the production workers are covered by collective bargaining agreements. The collective-bargaining-coverage approach does not distinguish between plants organized by locals of national unions and those organized by independent locals, nor between plants where all production workers are union members and those with collective bargaining agreements covering many nonunion members. Moreover, all employees in an industry are assigned the same collective-bargaining-coverage index regardless of whether or not they are themselves covered. There may be a tendency for our index to overstate unionism in industries with many small firms [9, pp. 273–75 and correspondence with Lewis], though the employer-size variable should pick up much of the effect of unionism missed for this reason.

An industry-wide measure of unionism seems appropriate in industries selling on national markets. Earnings at nonunion plants in highly unionized industries would tend to be high owing to the threat of unionism, and earnings at organized plants in industries where unions are weak would tend to be low because of competitive pressures. In localized industries, however, the *national* extent of collective bargaining coverage is, in effect, a weighted average of local-contract-coverage percentages. Such an average may be a poor union power variable if unionization varies greatly from locality to locality. Happily, the majority of observations in our study are in industries selling on national markets.

Regressions are computed for (a) unregulated industries and (b) regulated and unregulated industries together for the following four broad Census occupation and sex classes:

301–370 Clerical and Kindred Workers (Females only)
401–545 Craftsmen, Foremen, and Kindred Workers (Males only)
601–775 Operatives and Kindred Workers (Males only)
960–985 Laborers Except Farm and Mine (Males only)

In addition, regressions are computed for the unregulated industries only for the following, more narrow Census occupation-sex classes:

074 Draftsmen (Males only)
085 Mechanical Engineers (Males only)
290 Managers, Officials, and Proprietors, N.E.C. (Males only)
360 Typists (Females only)
465 Machinists (Males only)
715 Truck and Tractor Drivers (Males only)

[7] The sources and techniques used are described in an appendix available from the writer on request (see n. 6).

775 Operatives and Kindred Workers, N.E.C. (Males only)
775 Operatives and Kindred Workers, N.E.C. (Females only)
834 Janitors and Sextons (Males only)
985 Laborers, N.E.C. (Males only)

Most other detailed occupations are specific to particular industries. Indeed, even this list contains several lopsided samples: 42 per cent of the "Laborers, N.E.C." and 30 per cent of the "Truck and Tractor Drivers" are in industry 196 "Construction"; 34 per cent of the machinists, 21 per cent of the mechanical engineers, and 18 per cent of the draftsmen are in industry 258 "Miscellaneous Machinery," and 22 per cent of the female operatives are in industry 359 "Apparel and Accessories." The distribution of the four broad occupation classes among industries is closer to the distribution of the entire work force, but here the diversity of included occupations may introduce a great deal of unexplained variability. These problems are both near a minimum in the case of occupation class 601–775 "Operatives and Kindred Workers." In view of the relative ease with which new semiskilled trades can be learned, it seems more appropriate to treat operatives as a homogeneous group of workers than to treat craftsmen or clerical workers that way. Occupation class 601-775 also avoids the overweighting of construction that occurs in the case of "laborers" (960–985). Moreover, the estimates of unionism which refer to percentages of production workers seem to apply more accurately to operatives than to other occupations. The unregulated industries probably provide the more useful sample for our purposes because concentration has a clearer meaning there, since our estimates of the unionism variable are more solid there, and because 47 per cent of the "operatives" in the regulated industries are in the rather special group "Truck and Tractor Drivers." Male operatives in unregulated industries are used in Part III of the paper to illustrate the statistical analysis used. Results for other populations are summarized in tabular form in Part IV.

III. *Regression Results for Semiskilled Males*

Regression coefficients relating total 1959 private wage and salary earnings to concentration and unionization for male operatives in unregulated industries are shown in Table 1. Two sets of coefficients are shown, one using uncorrected average concentration ratios (*CR*) and the other using corrected average concentration ratios (*CCR*). The second set of regressions seems most reliable on a priori grounds. Although the adjustments are rough, *CCR* surely comes closer to measuring market power on a basis comparable among industries than the uncorrected index. All of the regression coefficients are statistically significant and have the initially expected signs. The effect of concentration

TABLE 1—REGRESSION AND CORRELATION COEFFICIENTS RELATING 1959 WAGE AND SALARY INCOME TO CONCENTRATION AND UNIONIZATION FOR MALE OPERATIVES AND KINDRED WORKERS IN UNREGULATED INDUSTRIES (OCCUPATIONS 601–775 IN INDUSTRIES 126–438)

$N=5187$

Mean Wage and Salary Income	Regression Coefficients				R^2 (Degrees of Freedom; Corrected)
	b_0 (Constant)	b_1 (CR or CCR)	b_2 (U)	b_3 ($U \cdot CR$ or $U \cdot CCR$)	
Using *CR*: 4419	3872 (62.51)	16.70 (1.664)			.0189
	3248 (110.2)	11.04 (1.851)	11.75 (1.712)		.0275
	2691 (246.9)	31.90 (8.468)	19.26 (3.434)	−.2659 (.1053)	.0285
Using *CCR*: 4419	3358 (84.34)	26.53 (1.967)			.0337
	3039 (112.8)	21.40 (2.304)	7.606 (1.790)		.0369
	1936 (280.5)	53.47 (7.811)	23.74 (4.159)	−.4426 (.1030)	.0401

is unequivocably positive. The marginal effect of unionization is positive if CR is less than 72 or if CCR is less than 54.

The apparent effect of unionization and concentration are computed in Table 2 for two illustrative values of CCR and U. The particular values of CCR and U are selected as "high" and "low" values in each case. Values of CCR are below 20 for only 9 per cent of the male operatives in unregulated industries and above 60 for only 8 per cent. Values of U are below 50 for 17 per cent of these observations and above 90 for 14 per cent. Unionization seems to raise annual earnings by about 16 per cent when concentration is low, but to have no effect when CCR is high. Concentration seems to raise earnings by about 33 per cent when

TABLE 2—ESTIMATED VALUES OF Y FOR CERTAIN VALUES OF CCR AND U

	Value of U: 50	90
Value of CCR 20	3750	4345
Value of CCR 60	5003	4891

unions are weak, but by only 13 per cent when they are strong. Disregarding other variables, the monopoly rents included in labor income could be very large.

When other industry characteristics are introduced as additional variables, the effects of CCR and U on Y are reduced. The equation becomes:

$$
\begin{aligned}
Y = \underset{(349.7)}{1751} + \underset{(9.259)}{20.13CCR} + \underset{(4.845)}{10.15U} - \underset{(.1268)}{.08907U \cdot CCR} + \underset{(.9169)}{5.494G} \\
+ \underset{(1.929)}{4.253L} - \underset{(83.47)}{401.8D} + \underset{(2.694)}{7.372M} + \underset{(3.278)}{5.997Sk} \qquad R^2 = .0491
\end{aligned} \tag{2}
$$

where G is 1960 employment/1950 employment, L is the percentage of employees in establishments with 250 or more employees, D is a dummy with a value of 1 if the product is durable, M is the percentage of the labor force that is male, and Sk is the percentage of production workers who are skilled (the number in occupations 401–545 as a percentage of the number in occupations 401–985). All of these industry characteristics have significant effects on earnings except for the interaction term ($U \cdot CCR$) and the percentage skilled (Sk). All have the expected signs except durability. Taken by itself, durability is positively related to earnings, but it becomes negative when other industry variables are introduced. This is partly due to the relatively depressed conditions and the steel strike of 1959. Although the coefficients of CCR and U are both reduced, they remain statistically significant. Earnings now rise with either CCR or U over the entire observed range. Values of Y computed from this new equation are shown in Table 3 for the same illustrative values of CCR and U that were used in Table 2, assigning mean values to G, L, M, and Sk and zero to D. Taking other industry variables into account, unionization seems to add about 9 per cent to annual earnings when concentration is low and 4 per cent when it is high. Concentration adds 16 per cent to earnings when unions are weak, but 11 per cent when they are strong.

Thirty-one personal characteristics and three more industry characteristics are next added to form a third regression. The new industry characteristics are the white, rural, and Southern shares of the labor force. The sex and skill composition of the labor force are pure industry

TABLE 3—VALUES OF Y IN NONDURABLE-GOODS INDUSTRIES USING CERTAIN VALUES OF CCR AND U AND MEAN VALUES FOR OTHER INDUSTRY CHARACTERISTICS

	Value of U: 50	Value of U: 90
Value of CCR 20	4215	4576
Value of CCR 60	4869	5061

characteristics because only semiskilled males are in our population, bu the white, rural, and Southern shares of the labor force include element of personal as well as industry characteristics until the correspondin personal characteristics are introduced.

The third equation with descriptions of the new variables used i listed below:

$$(3)\quad Y = -\underset{(623.9)}{156.2} + \underset{(8.195)}{.2930}CCR + \underset{(4.293)}{6.167}U + \underset{(.1110)}{.06872}U \cdot CCR + \underset{(.8668)}{.6150}G + \underset{(1.930)}{2.622}L$$

$$-\underset{(86.43)}{211.5}D + \underset{(2.683)}{6.192}M + \underset{(3.368)}{5.309}Sk \text{ plus}$$

Coefficient	Variable
1.952 (6.075)	Percentage of industry employees white with othe than Spanish surnames (Variable 9)
−3.937 (3.375)	Percentage of industry employees with residence out side SMSA's or urbanized areas (Variable 10)
6.276 (2.957)	Percentage of industry employees in the South (Vari able 11)
−247.8 (77.54)	Dummy with a value of one if residence in the South (Variable 12)
−483.6 (70.54)	Dummy with a value of one if residence outside SMSA or urbanized areas (Variable 13)
−668.1 (185.80)	Dummy with a value of one if nonwhite, non-Negro or if white with Spanish surname (Variable 14)
−681.3 (93.10)	Dummy with a value of one if Negro (Variable 15)
342.2 (67.13)	Dummy with a value of one if North Central residence (Variable 16)
633.6 (96.53)	Dummy with a value of one if Western residence (Variable 17)
−83.00 (85.75)	Dummy with a value of one if residence in a SMSA of less than 250,000 population (Variable 18)
141.60 (13.19)	Age (Variable 19)
−1.410 (.1609)	Age squared (Variable 20)
−438.3 (146.2)	Dummy with a value of one if less than five years of schooling (Variable 21)
−300.6 (100.0)	Dummy with a value of one if five to seven years of schooling (Variable 22)
−285.5 (68.57)	Dummy with a value of one if eight to eleven years of schooling (Variable 23)
165.0 (141.10)	Dummy with a value of one if one to three years of college (Variable 24)
− 65.02 (183.40)	Dummy with a value of one if presently in school (Variable 25)
−1134. (83.97)	Dummy with a value of one if not the principle source of support of the family (Variable 26)

291.9 (46.22)	Family size (in numbers of persons) (Variable 27)
−23.12 (4.463)	Family size squared (Variable 28)
56.21 (117.70)	Dummy with a value of one if moved since 1955 from an urban county in the same or contiguous state (Variable 29)
−441.5 (145.9)	Dummy with a value of one if moved since 1955 from an urban county in another state that is not contiguous to present state of residence (Variable 30)
−223.4 (69.83)	Dummy with a value of one if moved since 1955 from a non-SMSA residence (Variable 31)
243.10 (70.32)	Dummy with a value of one if native born with at least one foreign born parent (Variable 32)
−392.2 (107.6)	Dummy with a value of one if worked 15–29 hours "last week" (usually in the first half of April, 1960) (Variable 33)
−214.0 (134.3)	Dummy with a value of one if worked 30–34 hours "last week" (Variable 34)
− 35.10 (130.5)	Dummy with a value of one if worked 35–39 hours "last week" (Variable 35)
131.0 (70.27)	Dummy with a value of one if worked 41–48 hours "last week" (Variable 36)
349.8 (101.8)	Dummy with a value of one if worked 49–59 hours "last week" (Variable 37)
250.1 (133.6)	Dummy with a value of one if worked 60 or more (Variable 38)
−2095. (120.7)	Dummy with a value of one if worked 14–26 weeks in 1959 (Variable 39)
−1501. (91.62)	Dummy with a value of one if worked 27–39 weeks in 1959 (Variable 40)
−768.4 (80.15)	Dummy with a value of one if worked 40–47 weeks in 1959 (Variable 41)
−93.80 (95.34)	Dummy with a value of one if worked 48–50 weeks in 1959 (Variable 42)

$R^2 = .3421$

A large proportion of the personal characteristics are significantly elated to Y, and many that are not have the expected signs and roughly he expected values (e.g., the time- and weeks-worked variables number 4, 35, 36, 38, and 42). A few of the coefficients have unexpected signs.)ne wonders why migration from distant cities should have a negative ffect on earnings or why second-generation Americans should do better han those who are native-born of native parents. Perhaps the first eflects lack of seniority or incomplete knowledge of employment opporunities and the second is a partial proxy for prolonged urban residence.

A more serious problem is that the line of causation between earnings and personal characteristics may be obscured. The positive effect of mobility from nearby urban residences may mean that more mobile persons have better opportunities or it may mean that the better paid more often moved to the suburbs during 1955–60. Again, the low earnings of persons who are not the main sources of support in their families may reflect a lower productivity of part-time workers, but a worker with very low earnings is apt to fall into this category almost by definition. However, when this regression was repeated with variables 26, 29, 30, and 32 excluded, the signs and statistical significance of the coefficients for the industry variables did not change.

Once the personal characteristics are introduced, the coefficients relating Y to U and CCR drop to nonsignificance. Table 4 shows estimated values of Y with the same values of CCR and U used in Tables 2 and 3,

TABLE 4—COMPUTED VALUES OF Y WITH GIVEN VALUES OF CCR AND U ASSUMING ZERO VALUES FOR ALL DUMMY VARIABLES AND MEAN VALUES FOR ALL OTHER VARIABLES

		Values of U	
		50	90
Value of	20	5165	5488
CCR	60	5314	5747

assigning zero to all dummy variables and mean values to all others. In other words, Table 4 illustrates the effects of concentration and unionization on the earnings of urban, white males with high school diplomas living in the Northeast who worked 51–52 weeks in 1959 and 40 hours in a nondurable-goods industry "last week" (April, 1960) and had mean values for age, family size, and the various industry characteristics. Concentration increases the earnings of such workers by only 3 to 5 per cent, and unionization increases their earnings by 6–8 per cent. When the same regression is run without the interaction variable ($U \cdot CCR$), a 40 point increase in U increases Y by 8 per cent and a 40 point increase in CCR increases Y by 5 per cent. The coefficient for U becomes highly significant ($t = 5.0$) when $U \cdot CCR$ is dropped, but the coefficient for CCR is barely so ($t = 2.1$). The implication seems to be that firms in concentrated industries do pay their employees more, but that they get higher "quality" labor in the bargain. The incomes won by unions for their members more clearly exceed what those workers would earn in their best alternative employments.

Most other industry characteristics drop to nonsignificance when

personal characteristics are introduced, and two of the remaining significant coefficients have unexpected signs.[8] This is partly due to measurement errors. While concentration and important elements of union power are correctly measured on an industry-wide basis, employer size should ideally be measured on a personal basis, and employment growth on a SMSA-industry basis. However, the decline in the effect of industry characteristics as personal characteristics are introduced is consistent with labor market theory. Employers who, for any reasons, are in special positions which lead them to pay exceptional wages might be expected to get exceptional labor in the bargain.

The number of weeks and hours worked is understandably among the most important of personal characteristics in determining earnings. Leaving out all other personal characteristics, the equation becomes:

$$
\begin{aligned}
(4)\quad Y = {} & \underset{(324.3)}{2736} + \text{variables 33-42} + \underset{(8.420)}{9.162}CCR \\
& + \underset{(4.411)}{8.961}U + \underset{(.1154)}{.04811}U \cdot CCR + \underset{(.8403)}{2.167}G + \underset{(1.771)}{2.619}L - \underset{(76.23)}{261.6}D \\
& + \underset{(2.567)}{7.222}M + \underset{(2.973)}{8.551}Sk \qquad R^2 = .2203
\end{aligned}
$$

The time variables reduce the effect of concentration, growth, employer size, and durability by about half compared with the equation (2) where only industry characteristics were used. The effects of unionization and the sex composition of the industrial labor force are roughly unchanged. The effects of concentration and unionization on earnings as other variables are introduced are summarized in Table 5. The time variables reduce the effect of concentration sharply but enhance the effect of unionization, if anything.[9]

[8] Perhaps the time and weeks worked as reported in the Census do not identify the full effect of 1959 unemployment on earnings in durable-goods industries, or it may be that the often-observed high earnings in durables are due wholly to the unionization, concentration, employer size, or skill and sex mix in those industries. Another regression using average hourly earnings (from *Employment and Earnings*) also yielded a significant negative effect of durability when other industry variables were considered.

[9] The results of this analysis hold in spite of several possible revisions in the sample or the model. The effects of CCR and U are both significantly positive when taken by themselves or with an interaction term whether we (1) eliminate apprentices (occupations 601–630) from the sample; (2) eliminate construction and mining (industries 126–196) from the sample; and/or (3) use estimated average hourly earnings as the dependent variable. The introduction of industry characteristics weakens but usually does not remove the effect of CCR on earnings. The introduction of only a few personal characteristics (age, race, and Southern and rural residences) does eliminate the effect of concentration in most cases. On the other hand, these revisions enhance the effect of U even when all the personal characteristics in equation (4) are included. Coefficients for these alternative samples and income variables are shown in the appendix available from the author.

TABLE 5—PERCENTAGE INCREASES IN ANNUAL EARNINGS WITH INDICATED INCREASES IN *CCR* AND *U* TAKING DIFFERENT VARIABLES INTO ACCOUNT

	Effect of Increase in *CCR* from 20 to 60		Effect of Increase in *U* from 50 to 90	
	if *U* is 50	if *U* is 90	If *CCR* is 20	if *CCR* is 60
Using Only *CCR* and *U*	33.4%	12.6%	15.9%	−2.2%
Using Other Industry Characteristics	15.5	10.6	8.6	3.9
Using Industry and Time Variables	10.0	10.7	8.6	9.3
Using Industry, Time, and Other Personal Variables	2.9	4.7	6.3	8.1

Concentration has less effect on the price of labor than on total earnings. This implies that, even if the earnings of labor in concentrated industries do contain an element of monopoly rent, the allocative effect is small. High earnings due to more time worked do not mean misallocation of resources, as high wage rates uncompensated by superior "quality" of labor might. The effect of unionization on earnings, on the other hand, seems to operate through wage rates rather than time worked, implying that resource misallocation would occur to the extent that high earnings in unionized industries do not simply reflect personal characteristics.

Time variables are in part proxies for other personal variables. Table 6 shows the distribution of various personal and industry characteristics by the number of weeks worked in 1959. The persons with short periods of employment were younger, more poorly educated, more likely to be currently in school, more likely to live in the South or in rural areas, and the more likely to have recently moved from a rural residence or from a distant SMSA. They were particularly likely to work short hours and to be a secondary source of family income. They were employed more often in slowly growing industries with small plants. In other words, short-time employment was not distributed randomly. It was most noticeable among the less-desirable employees, and as a result, the effect of time variables on earnings is at least partially due to the "quality" of labor involved.

IV. *Regression Results for Other Occupation-Sex Groups*

The results spelled out in detail for male operatives in manufacturing, construction, and mining in Part III are similar to those found for some but not all of the other populations studied.[10] The coefficient for *CCR*

[10] Coefficients for *CCR*, *U*, and *U*·*CCR* for all occupations and industries studied are included in the statistical appendix which is available on request.

TABLE 6—CHARACTERISTICS OF MALE OPERATIVES AND KINDRED WORKERS BY NUMBER OF WEEKS WORKED IN 1959

Characteristic	Number of Weeks Worked in 1959									
	14–26 Weeks		27–39 Weeks		40–47 Weeks		48–50 Weeks		51–52 Weeks	
	No.	%	No.	%	No.	%	No.	%	No.	%
Southern Residence	89	29	135	27	159	25	107	27	835	25
Rural Residence	81	27	145	28	150	23	94	23	642	19
Negro	35	12	50	10	65	10	94	14	265	8
Moved from Distant SMSA since 1955	17	6	23	5	25	4	13	3	90	3
Moved from Non-SMSA since 1955	73	24	110	22	142	22	97	23	636	19
1–5 Yrs. of School	36	12	41	8	49	8	37	9	204	6
6–8 Yrs. of School	58	19	98	19	160	25	79	19	594	18
9–11 Yrs. of School	117	39	247	48	278	43	189	46	1560	47
12 Yrs. of School	63	21	100	20	130	20	94	23	812	24
Currently in School	35	12	22	4	13	2	2	—	43	1
Less Than 30 Hours Worked Last Week (April 1960)	111	37	104	20	65	10	35	8	65	2
Not Chief Income Recipient in Family	140	46	114	22	86	13	39	9	286	9
Employed in a Durable-Goods Industry	178	59	374	73	446	69	262	63	1911	58
Total Observations	301	100	510	100	642	100	415	100	3315	100
	Mean Values									
Age	34.5		37.4		39.1		39.2		39.8	
Family Size	3.9		3.9		3.9		3.8		3.9	
U	68		74		71		68		68	
CCR	38		40		41		39		40	
L	51		56		55		56		57	
G	117		114		117		123		130	

alone is always positive and almost always significantly so when no other variables are used. It remains positive in all cases except typists and retains its significance in most cases when U is introduced.

Table 7 summarizes the effects of concentration and unionization on Y when only CCR, U, $U \cdot CCR$ are used. The great majority of cases are consistent with the monopoly-wage hypothesis. The effects of CCR and U on Y are statistically significant for most male production workers. The only occupations that are unequivocably inconsistent with the monopoly-wage hypothesis are clerical workers when regulated indus-

TABLE 7—OCCUPATIONS CLASSIFIED BY MARGINAL EFFECT OF *CCR* AND *U* ON *Y* DISREGARDING OTHER VARIABLES

	Range of *U* Where *CCR* Has Positive Marginal Effect on *Y*	Range of *CCR* Where *U* Has Positive Marginal Effect on *Y*
Cases that support the "monopoly wage" hypothesis:		
b_3 positive		
401–545, Craftsmen, Unregulated Industries	0–100*	0–100**
960–985, Laborers, Unregulated Industries	0–100	0–100**
301–370, Clerical, Unregulated Industries, Female	30–100	31–100
715, Truckdrivers	0–100	0–100
074, Draftsmen	32–100	34–100
775, Operatives, N.E.C., Female	47–100	18–100
b_3 negative		
601–775, Operatives, Unregulated Industries, Male	0–100*	0– 54**
601–775, Operatives, Regulated and Unregulated Industries, Male	0–100*	0–100**
401–545, Craftsmen, Regulated and Unregulated Industries	0–100*	0– 98**
960–985, Laborers, Regulated and Unregulated Industries	0–100*	0–100**
775, Operatives, N.E.C., Male	0– 92*	0– 67**
465, Machinists	0–100	0– 50
834, Janitors	0– 88	0– 82
985, Laborers, N.E.C.	0–100*	0– 81**
085, Mechanical Engineers	0–100	0–100
290, Managers	0–100	0– 42
Cases that conflict with the "monopoly wage" hypothesis:		
b_3 positive		
301–370, Clerical Workers, Regulated and Unregulated Industries, Female	66–100	12–100
360, Typists	78–100	0–100

* Coefficient of *CCR* more than twice its standard error.
** Coefficient of *U* more than twice its standard error.

tries are included,[11] and typists. In addition, female clerical workers in unregulated industries, female operatives, and draftsmen, all have substantial numbers of observations in ranges where *CCR* and *U* have negative marginal effects, and managers have a substantial number of ob-

[11] This result is dominated by the large number of telephone operators included in female clerical workers in regulated industries.

TABLE 8—CLASSIFICATION OF OCCUPATIONS BY DIRECTION OF EFFECT OF CONCENTRATION ON EARNINGS WITH OTHER INDUSTRY AND PERSONAL VARIABLES TAKEN INTO ACCOUNT

	Range of U Where CCR has Positive Marginal Effect on Y	Range of CCR Where U has Positive Marginal Effect on Y
Cases that support the "monopoly wage" hypothesis:		
b_3 positive:		
601–775, Operatives, Unregulated Industries, Male	0–100	0–100
401–545, Craftsmen, Unregulated Industries, Male	49–100[a]	0–100[b]
985, Laborers, N.E.C.	49–100	0–100
775, Female Operatives, N.E.C.	0–100	0–100
b_3 negative:		
401–454, Craftsmen, Regulated and Unregulated Industries, Male	0– 79[a]	0–100[b]
775, Male Operatives, N.E.C.	0– 76	0–100[b]
074, Draftsmen	0–100	0–100
465, Machinists, Unregulated Industries	0–100	—
Cases that conflict with "monopoly wage" hypothesis		
b_3 positive:		
601–775, Operatives, Regulated and Unregulated, Male	—[a](neg)	0–100[b]
960–985, Laborers, Unregulated, Male	66–100	18–100
960–985, Laborers, Regulated and Unregulated, Male	93–100[a](neg)	0–100
300–370, Clerical, Unregulated Industries, Female	58–100	45–100
300–370, Clerical, Regulated and Unregulated, Female	63–100[a](neg)	43–100[b](neg)
360, Typists, Unregulated Industries	65–100	0–100
b_3 negative:		
085, Mechanical Engineers, Unregulated Industries	—	—
290, Managers, Unregulated Industries	0– 10	0– 31
715, Truck and Tractor Drivers, Unregulated Industries	0– 1	0–100

[a] Coefficient of CCR is more than twice its standard error; "neg" if significant coefficient has negative sign.

[b] Coefficient of U is more than twice its standard error; "neg" if significant coefficient has negative sign.

servations in a range where U has a negative marginal effect. None of the coefficients that result in these equivocal cases is statistically significant.

The introduction of industry and personal characteristics changes the

findings sharply once more.[12] Table 8 classifies occupations by their degree of support for the "monopoly wage" hypothesis when all these other variables are used. Concentration now has a negative marginal effect on earnings over some range for most occupations, and the over-all effect of concentration is negative in as many cases as it is positive. The effect of unionism is more consistently positive. The positive effect of concentration and unionization on earnings seems to hold up best in the case of "craftsmen," perhaps because of greater power of the union there.

One intriguing result is the *negative* effect of concentration and unionization on the incomes of female clerical workers. Their earnings are increased if both *CCR* and *U* are high, but concentrated nonunion industries or unconcentrated, unionized industries seem to pay low clerical incomes. The high *CCR*-low *U* industries are mainly rapidly growing "high brow" industries (drugs, professional equipment and supplies, office, computing and accounting machines, photographic equipment and supplies, and at the borderline, miscellaneous chemicals). A possible explanation is that these industries must (and can) pay high wages to keep their production workers from organizing, but they face little such threat with their female clerical workers. The high *U*-low *CCR* industries are such fields as construction, trucking, coal mining, and beverages. Unions in such industries often organize the major occupations involved on an interfirm basis. They may not be much concerned with the pay scale of clerical help outside their membership. Indeed, to the extent that they secure high wages for the male production workers they represent, they may actually depress the demand for and, therefore, the price of complementary services such as those of female clerical workers. These suggested reasons for the apparent "exploitation" of clerical help in certain industries is blatant after-the-fact reasoning. We make no claim to have tested this "exploitation" hypothesis as we have the monopoly-wage hypothesis.

V. *Conclusions*

This paper examined two forms of the monopoly-wage hypothesis: (1) that concentrated industries pay high incomes for given occupations, and (2) that these incomes exceed the alternative costs of the labor involved. The first hypothesis holds up well, as it has in previous studies. The relationship is strongest for male production workers where the threat of unionization is undoubtedly greatest. Moreover, the interac-

[12] The same personal characteristics were used for each occupation as for operatives and kindred workers except that two more education dummies (for 4 years of college and 5 or more years of college) were introduced in the cases of "mechanical engineers" and "managers, officials, and proprietors, N.E.C."

tion between concentration and unionism most commonly has a negative effect for such workers. Both results give support to the notion that it is unionism or the threat of unionism that produces high wages in concentrated industries.

The second hypothesis should apparently be rejected. Once personal characteristics are introduced, the relationship between concentration and earnings is no longer significant and is negative about as often as it is positive. The monopolistic industries do get superior "quality" for the incomes they offer. Moreover, a substantial portion of the relationship between the earnings of labor in concentrated industries and their quality is due to a relationship of both variables with the number of weeks worked. The laborers in concentrated industries seem to receive no more for their services than they might in alternative employments for persons with similar personal characteristics. Their earnings contain little or no monopoly rent.

This does not necessarily imply that no misallocation results from high-wage payments in concentrated industries. Labor "quality" in this study includes such personal characteristics as race, which may be quite irrelevant to the objectively evaluated productivity of the laborer involved. It has been suggested that firms with monopoly power use part of their "profits" to hire congenial or socially acceptable employees [2], an option not available to employers subject to more stringent competitive pressures. If so, the earnings of labor in monopolistic industries may still exceed its marginal-revenue product, even though they apparently approximate the value of its alternative product.

The conclusion with respect to the effect of unionism on labor earnings is more equivocal than the results for concentration. The relationship between unionism and earnings does not decline greatly when personal characteristics are introduced for nonregulated industries. Unions that organize their entire jurisdictions seemed to raise earnings by 7 to 8 per cent for craftsmen and 6 to 8 per cent for operatives, compared with poorly organized industries. The relationship between unionism and earnings is often nonsignificant after personal characteristics are introduced, but this may well be the result of measurement errors. U was certainly the most difficult variable to estimate in this study. It has been claimed that the BLS *Industry Wage Surveys* overstate the extent of contract coverage because small employers are underrepresented. If this is correct, some of the effect of differing degrees of unionization may actually be included in the coefficients for the establishment-size variable instead of the coefficient for U. Finally, the only effect of unionism accounted for in this paper is its industry-wide impact. Differences within an industry between persons covered by collective bargaining contracts and those not covered had to be ignored. All of these consid-

erations seem to point toward an understatement of the effect of unionism on earnings.

A possible case of misallocation due to the effects of concentration and unionism on earnings is the particularly low income of female clerical workers in concentrated *or* unionized industries (but not in concentrated *and* unionized industries). Some sort of "monopsonistic exploitation" hypothesis may apply here. Little can be claimed for this hypothesis, however, since, unlike the monopoly- and union-wage hypotheses, it was not developed in advance of the empirical study.

The effects of most industry characteristics are nonsignificant and often of unexpected signs after personal characteristics are introduced. In general, employers who for any reason pay high salaries receive "superior" labor in the bargain. The general picture is one of fairly efficiently working labor markets, even where substantial monopoly may exist.

All of the conclusions of this paper are necessarily tentative because the indexes of concentration used are imperfect, because industry definitions are arbitrary, because weights used in combining markets to match Census industries are arbitrary, and because the Census places some persons in the wrong industries. It might be argued that the nonsignificance of concentration as a factor in income determination once personal variables are introduced is due merely to measurement errors. On the other hand, the significant results before personal characteristics are introduced suggest that much of the effect of monopoly power has in fact been identified, and that at least this identified portion is almost entirely accounted for by personal characteristics. Moreover, a great number of personal characteristics could not be adequately measured, either. Such obviously pertinent variables as health, intelligence, appearance, strength, manual dexterity, sobriety, and responsibility could only be approximated by distant proxies such as age, sex, education, and family status if they were included at all. If the high-wage employers were as successful at hiring and retaining intelligent and responsible persons as they were in hiring educated ones, it would seem as likely that the residual effects of concentration are exaggerated in this study as that they have been missed.

REFERENCES

1. M. ADELMAN, "Steel, Administered Prices, and Oligopolistic Inflation," *Quart. Jour. Econ.*, Feb. 1961, *75*, 16–40
2. A. A. ALCHIAN AND R. A. KESSEL, "Competition, Monopoly, and the Pursuit of Money," in *Aspects of Labor Economics*, Princeton 1960, pp. 70–81.
3. W. G. BOWEN, *Wage Behavior in the Postwar Period: An Empirical Analysis*. Princeton 1960.

4. BUREAU OF THE CENSUS, *U.S. Censuses of Population and Housing: 1960 1/1000 and 1/10,000, Two National Samples of the Population of the United States* (Processed).
5. O. ECKSTEIN AND G. FROMM, "Steel and the Postwar Inflation," Joint Economic Committee, *Study of Income, Employment, and Prices*, Study Paper No. 2, Washington 1959.
6. J. W. GARBARINO, "A Theory of Inter-Industry Wage Variation," *Quart. Jour. Econ.*, May 1950, *64*, 299–305.
7. A. C. HARBERGER, "Monopoly and Resource Allocation," *Am. Econ. Rev., Proc.*, May 1954, *44*, 77–87.
8. H. M. LEVINSON, "Postwar Movements in Prices and Wages in Manufacturing Industries," Joint Economic Committee, *Study of Income, Employment, and Prices*, Study Paper No. 21, Washington 1960.
9. H. G. LEWIS, *Unionism and Relative Wages in the United States*. Chicago 1963.
10. M. REDER, "Wage Differentials, Theory and Measurement," in *Aspects of Labor Economics*, Princeton 1962, pp. 285–86.
11. A. REES, "Union Wage Gains and Enterprise Monopoly," in *Essays in Industrial Relations Research*, Institute of Labor and Industrial Relations, University of Michigan-Wayne State University 1961, pp. 125–39.
12. A. M. ROSS AND W. GOLDNER, "Forces Affecting the Inter-Industry Wage Structure," *Quart. Jour. Econ.*, May 1950, *64*, 254–81.
13. D. SCHWARTZMAN, "Monopoly and Wages," *Can. Jour. Econ.*, Aug. 1960, *25*, 428–38.
14. M. SEGAL, "The Relation Between Union Wage Impact and Market Structure," *Quart. Jour. Econ.*, Feb. 1964, *78*, 96–114.
15. S. H. SLICHTER, "Notes on the Structure of Wages," *Rev. Econ. Stat.*, Feb. 1950, *32*, 80–91.
16. G. STIGLER, "The Statistics of Monopoly and Merger," *Jour. Pol. Econ.*, Feb. 1956, 33–40.
17. L. W. WEISS, *Economics and American Industry*, New York 1961, pp. 506–7.
18. ———, "Average Concentration Ratios and Industry Performance," *Jour. Ind. Econ.*, July 1963, *11*, 240–41.
19. ———, "Business Pricing Policies and Inflation Reconsidered," *Jour. Pol. Econ.*, April 1966.

THE EFFECT OF INCOME ON DELINQUENCY

By BELTON M. FLEISHER*

Recent government policy is evidence of considerable public interest in the relationship of young people to the labor market. Delinquency problems arise in this context because decisions to engage in illegitimate activity seem interconnected with decisions about schooling, labor force participation, and even occupational choice. Thus a study of factors affecting delinquency is of interest not only to criminologists and to crime prevention agencies, but to all those concerned with the allocation of youth's time among alternative pursuits.

This study is an attempt to discover whether low income is a cause of juvenile delinquency, and if so, to present evidence of its importance. Economists have often investigated relationships between income and expenditures—Engel's being perhaps the earliest well-known work of this type; in addition they have recently begun studying quantitatively the effects of income on other behavior. Two novel analyses of income-behavior relationships are the recent studies of Becker on fertility and of Mincer on the labor force participation of married women [1] [7]. So, while my subject matter may seem strange to economists, it is not really deviant from an important tradition involving the application of economic analysis to a rather wide variety of social phenomena.

The literature on delinquency contains a wide range of estimates (both negative and positive) of the effect of income. However, there is little consensus regarding their meaning. This lack of consensus is largely due to the absence of an accepted theoretical framework for analyzing delinquency and to a lack of agreement about which empirical observations best represent the analytical concepts that have been employed. I think the approach to the problem employed in this study can eventually promote considerable progress toward an understanding of the effect of economic conditions upon delinquent behavior.

* The author is assistant professor of economics at Ohio State University. Research for this paper was carried out under United States Department of Health, Education and Welfare Grant No. 62216 at the University of Chicago. Professor Morris Janowitz, Department of Sociology, University of Chicago, originally interested the author in the subject matter of the paper. The author has received substantial benefits from the comments of H. Gregg Lewis. The paper has also benefited from comments of Margaret G. Reid and from presentation to the Research Group in Labor Economics of the University of Chicago. The author bears sole responsibility for any errors.

I. *An Economist's View of Delinquency*

Most studies of the effect of income on behavior purport to present estimates of demand or supply functions, and this study is no exception because it is an attempt to estimate a net causal relationship running from income to delinquent behavior. The economist's ordinary tools of supply and demand appear useful in constructing a model for the empirical investigation of delinquency. I shall use the concept of demand to mean a causal relationship between economic and other characteristics of persons on the one hand and their tendencies to commit delinquent acts on the other. But the number of delinquent acts committed should depend also on the number or value of delinquency opportunities. I assume that the supply of delinquency opportunities depends on the economic and social characteristics of the environment. I further assume the interaction of these two forces—the propensity to be delinquent and the opportunities available—determines the number of delinquent acts actually committed. The use of an economist's tools to analyze delinquency does not in any way presuppose that "economic" variables such as income are the most important factors determining delinquency rates. It still remains to estimate their importance in the context of a model which allows us to view separately the possible effects of income, opportunities, and other factors.

The question of just what constitutes these "other" factors is of crucial importance. Intuitively one believes that personality traits and family characteristics must play an important role in determining delinquent behavior within rather broad economic and social constraints. It is these to which I refer in the concept of "taste for delinquency." Insofar as these tastes are correlated with incomes, it is necessary to specify them correctly so that their influence is not included in the estimates of the effect of income, and vice versa. Also at the intuitive level, one suspects that those personal traits which cause some individuals, other things the same, to earn more than others—stability, industriousness, etc.—also contribute to low delinquency rates. It therefore appears likely that taste for delinquency and income levels will be rather highly correlated.[1]

[1] It is perhaps risky for someone trained in economics to try to explain what has been going on in another discipline, such as sociology. However, it is desirable to attempt to relate the "economist's view" of delinquency to the "sociologist's view." In my opinion, the concept of taste is closely related to sociological concepts such as "anomie." When sociologists argue the relative merits of "commitment to the existing social structure" vs. "opportunities" or "material well-being" as variables explaining delinquency, they refer to questions which I hope to be able to answer at least partially with a model taking into explicit consideration both tastes and economic variables. The problem that criminologists and sociologists have not solved, and for which it will become apparent that I have not provided the definitive solution, is the identification of those observable phenomena which

While tastes are undoubtedly important determinants of legitimate vs. illegitimate behavior, it is intuitvely appealing, and casual inspection leads one to suspect, that economic factors are also an important cause of delinquency. The principal theoretical reason for believing that low income increases the tendency to commit crime is that it raises the relative cost of engaging in legitimate activity. In the first place, youngsters probably view their families' income as an index of their own long-run legitimate earning possibilites. Thus, so long as there is not substantial positive covariation between the expected returns to legitimate and illegitimate activity, individuals with low incomes (or whose parents have low incomes) probably expect relatively large payoffs for committing delinquent acts. To such individuals, the probable cost of getting caught is relatively low, since because they view their legitimate lifetime earning prospects dismally they may expect to lose relatively little earning potential by acquiring criminal records; furthermore if legitimate earnings are low, the opportunity cost of time actually spent in delinquent activity, or in jail, is also low. A related possible effect of income on delinquency is due to the lifetime pattern of income. Since most legitimate careers entail lower earnings during their earlier stages (especially during formal schooling) than in their later stages, youngsters who can rely upon little help from parents or other family members in providing desired goods and services may well be tempted to engage in illegitimate activities on a "part-time" basis.

As stated above, casual observation also leads one to suspect that poverty causes high delinquency rates. Particularly within cities, it has long been observed that areas of low incomes, high unemployment, and the like are the areas of highest delinquency rates.[2] It is also true that a disproportinontely large number of serious crimes for which youth are arrested are crimes against property. In 1961, for instance, 93 per cent of the serious arrests of people aged less than 25 were for the crimes of robbery, burglary, larceny, and auto theft [4, p. 544]. Although this age group constituted only 43 per cent of the 1960 population of the United States, in 1961 it accounted for almost 90 per cent of the arrests for auto theft and over 60 per cent of arrests for the remaining property crimes [4, p. 544].[3]

While incomes are conceptually negatively related to the cost of en-

---

are the operational equivalents of conceptual tastes for delinquency. In statistical jargon, we are not reasonably certain how to specify properly a model of delinquency. Consequently all interpretations of the results of empirical investigations are weakened by our not knowing whether we are observing the effects of the variable we conceive as relating to delinquency or whether we are observing the effects of misspecification of the model of delinquency.

[2] A classic study is [8].

[3] I think it is advisable to point out that it is by no means inconsistent with the above

gaging in delinquent activity, they are positively related to the expected gains. This is true of crimes against property, at least, which constitute a disproportionately large number of the delinquent acts of youth. The greater the income or wealth of potential victims, the greater will be the expected value of theft. Consequently, income has two conceptual influences on delinquency which operate in opposite directions, although they are not necessarily equal in strength. Thus it is desirable to attempt to estimate these influences separately. In terms of an economist's view of delinquency, the expected negative relation between income and delinquency can be expressed as a demand relationship, and the expected positive relation as supply.

II. *A Simple Model of Delinquency*

Variables Employed

I present now the list of variables employed in the empirical investigation. I shall discuss what theoretical concepts the variables are thought to represent, how they fit into a simple model of delinquency, and some biases that may be included in their estimated effects.

1. The dependent variables employed are not, by necessity, actual juvenile crimes or crime rates. Since it is necessary to know the ages of offenders in order to identify crimes of youth, arrest and court-appearance data are used in forming dependent variables. It is generally thought to be a fault of arrest and court data that the probability of one's appearing in these statistics, given one's actual offense record, is negatively correlated with economic and social standing. Thus the use of arrest and court data may impart a negative bias to the estimated (demand) effect of income on delinquency. Since this economic or social bias is thought to be stronger, the farther removed is the measure of crime from the actual commission of the crime, arrest data may yield more reliable estimates of the income effect than do court-appearance data. The influence of social standing on the number of reported arrests and court appearances is probably greatest within police

discussion of the relationship between economic factors and delinquency rates to observe that incomes and crimes of violence are also negatively correlated. Insofar as violent activity has entertainment value, or whatever, to those who engage in it, the economic cost of such activity in terms of the penalties associated with apprehension will be low if youths expect only minor effects on their lifetime earnings. A youngster who aspires to become a lawyer cannot afford to have a police record for assault or rape. However, such a record would probably be relatively unimportant to a potential manual laborer. It is also conceivable that any pleasure derived from violent crimes may be a substitute for market goods and services. They may also be complements, however, as one supposes that higher incomes are associated with increased consumption of alcoholic beverages, and through this medium may be positively associated with crimes of violence. Finally, one should not ignore the possibility that crimes of violence and property crimes may be complements in the economic sense.

and court jurisdictions, as it is probably relative levels of income and social influence that are important.

I think one must beware of exaggerating the importance of socioeconomic bias in arrest and court-appearance data. Undoubtedly one of the causes of bias is that the influence of parents of relatively high socioeconomic standing on the behavior of their children is generally thought to lead toward legitimate behavior. Thus, the probability of a child's being punished at home on account of a complaint from the police is thought to be smaller, the lower the social or economic standing of the family from which he comes. It is therefore probable that the low-status child who gets into trouble will commit more crimes in the future if he is not brought to justice formally when he is apprehended. I do not mean to pass judgment at this point on the efficacy of our methods of dealing officially with juvenile offenders; I merely wish to point out that biases in arrest and court-appearance data may well reflect real differences in the tendencies toward delinquency of different segments of the population.

The actual dependent variables used in this study depend upon the sample from which they were taken. They are:

Y_{cc}, the number of court appearances of males, aged 12 through 16 years, during the years of 1958-61, for the census tract communities in the city of Chicago. (Census tract communities are aggregates of census tracts which purport to represent useful subdivisions of the city into areas of rather stable community structure.)[4] The number of appearances is expressed as an annual average number, per thousand males aged 12 through 16.

Y_{cs}, the same as Y_{cc} for 45 suburbs of Chicago in Cook County with populations in excess of 10,000. These data were provided by Henry D. McKay of the State of Illinois Institute for Juvenile Research.

Y_{usp}, the number of arrests of males, aged less than 25 years, for crimes of robbery, burglary, larceny, and auto theft, in each of 101 cities of the United States with populations greater than 25,000. The number is expressed as the rate per thousand males less than 25 years of age. Information is available for most of the cities for the years 1960, 1961, and 1962, but not for all of them. The data are expressed as annual average rates.[5]

Y_{usv}, the same as Y_{usp}, except that the arrests are for crimes of

[4] See [5]. The Chicago delinquency and other variables are taken from [5].

[5] These data are based on information provided by the United States Federal Bureau of Investigation. They are not published, as the FBI assures reporting cities of the anonymity of the information provided by them. While they do not represent an exhaustive list of all U.S. cities with more than 25,000 population, they do represent all size classes in this range.

violence, namely homicide, rape, and assault.

2. Although a number of income variables were experimented witl in this study, only three of them are used in the results reported here Two variables represent income as usually conceived.

MEINC2 is the mean family income of the second lowest quartil of families which receive income. This variable is intended to stand fo the economic level of a community as it affects the tendency to commi delinquent acts.

MEINC4 is the mean income of the highest quartile of families This variable represents an attempt to measure the payoff for certai kinds of delinquent behavior. These two measures of income are gen erally used together in the regressions, as follows:

$$(1) \qquad Y = a + b\ MEINC2 + c\ MEINC4 + \ldots$$

Equation (1) can be rewritten by adding and subtracting $c\ MEINC$ to the right-hand side, yielding

$$(2)\ Y = a + b\ MEINC2 + c\ MEINC4 + c\ MEINC2 - c\ MEINC2 + \ldots$$

When equation (2) is rearranged, we find that (1) is the equivalent o

$$(3)\ Y = a + (b + c)\ MEINC2 + c(MEINC4 - MEINC2) + \ldots$$

Thus, the sum of the coefficients of the two income variables yields a estimate of the effect of family income on delinquency. The coefficien of the fourth quartile yields an estimate of the income dispersion o delinquency. The sum $(b + c)$ can be thought of as the demand re lation between income and delinquency, and the coefficient c can b thought of as the supply relation. Other measures of family incom and dispersion yielded regression results so close to those obtaine using equation (3) that it does not seem worthwhile to present them

The income data are all taken from the 1960 Census of Populatio and refer, of course, to incomes during the calendar year 1959.[6]

3. In attempting to estimate the effect of income on delinquency, i is important to consider the effects of both normal family incomes an deviations from normal due to unemployment. Both normal incom and deviations from normal may affect delinquency, but the effect may be different.

UNEMMC is the male civilian unemployment rate taken from th 1960 Census of Population.

4. Four variables are employed to represent tastes. *SPDVFM* is th

[6] Mean income of the lowest quartile is not employed, as it is most likely to be infl enced by family structure and other characteristics of the samples which are highly corr lated with taste. Since low-income groups tend to be more delinquent than other *MEINC 2* is probably a good measure of the economic opportunities of potential d linquents.

proportion of females over 14 years of age who are separated or divorced. This is supposed to reflect the proportion of broken families in the community. I shall refer to it as the family-structure variable. There are several means by which family structure may influence delinquency. An obvious means is that families with only female heads tend to have lower incomes (by about 50 per cent) than do families with both male and female heads. However, the multiple regression technique, which estimates the effects of income and family structure simultaneously, is an attempt to remove the income component from the estimated effect of family structure on delinquency. Another means by which family structure may affect delinquent behavior is through parental supervision and guidance. This works in two ways, both of which are related to the fact that children in broken families usually live with only their mothers as adult family members. The first is that the mother is likely to have to supplement the family's income with her own earnings if the father is not present. This means that she has less time to supervise the activities of her children. Secondly, it is a widely held belief that boys (who constitute the bulk of delinquents and with whose behavior we are exclusively concerned in this paper) learn about the economic facts of life primarily through their relationship with the father and his connection with the economic system. To the extent that legitimate endeavor requires a longer time horizon—a clear view of relatively distant payoffs—than does illegitimate behavior, absence of a male family head probably works in favor of delinquency. Of course, the male family head probably helps reduce delinquency by participating in the pure supervisory role, along with the female head.

It is important to consider whether, supervisory roles and labor market connections aside, the family-structure variable has still an additional influence on delinquency. If this variable truly reflects what I have referred to as the taste for delinquency, it will affect behavior not only through the means discussed in the preceding paragraph, but also because, given the amount of supervision, etc., parents are able to provide their children, the attitudes of broken families will be oriented less toward what is generally regarded as legitimate behavior and more toward delinquent behavior. If the family-structure variable does reflect such a taste factor it may serve the purpose of holding tastes constant so that the true income effect may be estimated. The supervision and market connection phenomena are analogous to tastes in this context and are additional reasons for including the family-structure variable in the regression analysis.

But there is an important way in which family structure may be related to delinquency which would be a reason for excluding it from the

regression analysis. Suppose that causation runs from delinquency to divorce, rather than vice versa. For instance, consider the possibility that delinquency is partly caused by economic variables, which are included in the regression analysis. Suppose also that delinquent populations tend to have higher divorce rates than others. Then by putting the family-structure variable in the regression we may rob the economic variables of their effects; which variable "wins" may depend principally upon which has the least serious errors of measurement.[7] I can do no more than point out this possible source of error in interpreting the regression results and then go on; without a well-developed theoretical model of delinquency and a great deal of testing, the probability is always high that empirical work will be hampered by doubts regarding specification.

MOBILR is the proportion of the population over five years of age that lived somewhere other than the community of current residence five years previous to the 1960 census. The variable is an attempt to capture, and to hold constant, the effect on delinquent behavior of residing in a strange community. Presumably new residents are less likely to observe the established codes of behavior of the community than old residents. While the mobility variable may also reflect any temporary losses of income associated with moving, this effect should be mostly eliminated by the presence of the income variables in the regression equations. The expected influence of mobility is to increase delinquency, except for one possibility.

If a large proportion of a community's recent in-migrants arrived only in the year before the census, then the income variable for these people will in part reflect income of a previous job which, if the in-migrants are moving toward improved economic opportunities, may be lower than current income. In this case, the mobility variable may pick up the tendency of current income to be higher than measured income; this influence on delinquency rates should be negative.

[7] It is possible, but factually less appropriate, to apply this same argument to the income variable. It could be held, for instance, that delinquent populations have lower legitimate earnings than do other populations, and that this is reflected in reported family incomes. However, the income variables used reflect income of the whole community; it seems likely that more conventional economic factors outweigh any effects of delinquency on the measured family incomes of communities. This, I think, is even true of the Chicago census tract communities, which are likely to be more homogeneous than the suburbs or 101 cities. In any event I would argue that the use of the taste variables in the regressions tends to hold constant the effect of delinquency on earnings. In the extreme case that both the income and taste variables are "caused" by delinquency, we are in the unfortunate situation of not having a properly specified model of delinquency at all. We are merely doing correlation analysis in the most literal sense. I feel that it is likely that some element of reverse causation is present and confounds the regression results presented here. However, I think the importance of this is greatest for the family-structure variable and of negligible importance for the income variables.

NWHITE is the proportion of the population that is nonwhite. Its use is an attempt to pick up any taste factors, associated with race, which are not reflected in the other taste variables. One hardly need mention that the racial composition of a community is correlated with its income and family characteristics.

NSDUMM is a regional dummy variable, equal to one for Southern cities and equal to zero for all other cities. It is employed only in the 101 cities analysis.

The independent variables and their symbols are listed in Table 1.

The source of all variables not specified elsewhere is the 1960 Census of Population.

III. *Results of the Regression Analysis*

Since the delinquency rate is conceptually bounded by zero and one thousand, it is not unlikely that a nonlinear relationship exists between

TABLE 1—SELECTED SAMPLE STATISTICS OF SAMPLE GROUPS AND SUBGROUPS USED IN THE REGRESSION ANALYSIS

Group	*SPDVFM*		Delinquency		*MEINC* 2		*MEINC* 4		*UNEMMC*	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
101 Cities	.062	.019	17	8.7	5172	881	14091	2892	.052	.017
101 Cities (High)	.083	.011	24	8.8	4659	686	13827	1886	.056	.015
101 Cities (Medium)	.060	.0042	14	5.6	5234	774	14107	3096	.053	.016
101 Cities (Low)	.040	.0070	12.8	6.7	5650	902	14354	3540	.046	.019
Chicago Com.	.070	057	25	20	5940	1430	15721	5854	.051	.036
C. Com (High)	.13	.054	46	19	4491	1196	13968	5702	.085	.043
C. Com (Med.)	.046	.0065	18	7.4	6187	543	15803	5890	.039	.012
C. Com (Low)	.027	.0043	11	4.1	7236	818	17534	5645	.027	.009
Chicago Suburbs	.027	.012	7.4	4.4	8214	2043	22563	11530	.017	.009

List of independent variables employed in the regressions.

MEINC 2 Mean income of the second lowest quartile of income recipients (1959).
MEINC 4 Mean income of the highest quartile of income recipients (1959).
UNEMMC Male civilian unemployment rate (1960).
NWHITE Proportion of the population that is nonwhite (1960).
MOBILR Proportion of population of five-year-olds that lived in another county in 1955 (1960).
SPDVFM Proportion of females over 14 who are separated or divorced (1960).
NSDUMM Regional dummy variable equal to one for Southern cities, zero otherwise.

the rate and some of its causes. Also, it has been suggested that the influence of incomes on delinquency will be greater in the presence of a high proportion of separation and divorce. That is to say, the response of potential delinquents to economic incentives may be greater in communities that have high tastes for delinquency than in communities with strong tendencies toward legitimate behavior.

Thus, for purposes of estimating the effects of the income and taste variables, the two largest samples (101 cities and Chicago communities) were divided into three subgroups of approximately equal size, according to the values of the family-structure variable. The observations in a high subgroup have the highest proportions of separated and divorced females, and so on. The regressions were estimated using both the entire samples and the subgroups. The division into subgroups is an attempt to capture any effects of nonlinearity in the true relationships and/or interactions between variables and effects that may exist. It is also a method of holding constant tastes as reflected in the family-structure variable. I think it is realistic to consider the Chicago suburbs as corresponding to either of the lowest subgroups of the two other samples. Table 1 presents some of the sample statistics for the samples and their subgroups. There are substantial differences among the subgroups in the level and variance of the family-structure variable. There are also notable differences in the levels of delinquency and income.

The regression results are shown in Tables 2 through 6. Most of the regression coefficients are large enough to be interesting from the economic or sociological point of view, and most of the coefficients of economic variables are statistically significant.[8] If one is interested in comparing the size of the effects of the variables included in the regressions, the elasticities, which are calculated at the mean values of the variables, are of interest. These are estimates of the percentage change in the dependent variable which occurs when there is a 1 per cent change in the independent variable.

Table 2 shows the regression results for the 101 cities sample and subgroups. Examination of the results of the regressions based on the entire 101 cities sample shows that adding the family structure variable to the regression has a substantial effect on the multiple R^2 and on

[8] I think it is appropriate to apply a one-tailed test of significance to the regression estimates. With 19 degrees of freedom (the smallest number for any of the regressions) a t-ratio of .86 indicates statistical significance at the 20 per cent level.

A one-tailed significance test seems appropriate because of the a priori considerations which suggest the signs of the regression coefficients. While a 20 per cent level of significance is high by some standards, I believe it is not inappropriate, considering the desirability of reducing both "type I" and "type II" errors. The reader is of course free to establish whatever criteria for statistical significance he chooses.

TABLE 2—101 U.S. CITIES > 25,000

Dependent Variable Y_{usp}

Regression	Variable	Regression Coefficient	t-Ratio	Elasticity	Multiple R^2
101 Cities					
#1	*MEINC* 2	−.0044	−2.5	−1.3	
	MEINC 4	+.00061	+1.5	+ .51	
	UNEMMC	+108	+1.7	+ .33	.20
	MOBILR	+27	+1.9	+ .13	
	NSDUMM	−3.0	−1.0		
#2	*MEINC* 2*	−.00094	−0.6	− .28	
	MEINC 4*	−.00016	−0.4	− .13	
	UNEMMC	+16.0	+0.3	+ .05	
	NWHITE	+6.9	+0.7	+ .05	.48
	MOBILR	+8.2	+0.6	+ .04	
	SPDVFM	+279	+4.6	+1.0	
	NSDUMM	−5.9	−2.4		
#3	*MEINC* 2	−.0014	−1.2	− .42	
	UNEMMC	+18.7	+0.4	+ .06	
	NWHITE	+5.8	+0.6	+ .04	.48
	MOBILR	+8.0	+0.6	+ .04	
	SPDVFM	+278	+4.6	+1.0	
	NSDUMM	−6.0	−2.5		
Grouped by SPDVFM					
High	*MEINC* 2	−.0093	−2.6	−1.8	
	MEINC 4	+.0013	+1.4	+ .77	
	UNEMMC	−80	−0.9	− .19	
	NWHITE	−13.5	−0.9	− .14	.59
	MOBILR	−48.0	−1.9	− .20	
	SPDVFM	+548	+4.0	+1.9	
	NSDUMM	−11.8	−3.1		
Med.	*MEINC* 2	−.0011	−0.6	− .43	
	MEINC 4	−.00012	−0.2	− .12	
	UNEMMC	+130	+1.5	+ .49	
	NWHITE	+12.2	+0.7	+ .08	.34
	MOBILR	+31.9	+2.2	+ .21	
	SPDVFM	+227	+0.9	+ .97	
	NSDUMM	−2.8	−0.8		
Low	*MEINC* 2	−.00026	−0.1	− .12	
	MEINC 4	−.00031	−0.6	− .35	
	UNEMMC	+36	+0.4	+ .13	
	NWHITE	−31.9	−0.9	− .07	.36
	MOBILR	+43.2	+1.3	+ .23	
	SPDVFM	+322	+1.6	+1.0	
	NSDUMM	−15.2	−2.0		

* The sum of coefficients of *MEINC* 2 and *MEINC* 4 is −.0011; the t-ratio is 0.83.

TABLE 3—101 CITIES
REGRESSIONS FOR AGES 17 AND UNDER

Regression	Variable	Regression	t-Ratio	Elasticity	Multiple R^2
Property Crimes					
	MEINC 2	−.0021	−1.5	−.75	
	UNEMMC	−9.6	−0.2	−.03	
	NWHITE	+9.7	+0.8	+.08	.34
	MOBILR	+18.2	+1.2	+.11	
	SPDVFM	+213	+2.9	+.89	
	NSDUMM	−8.6	−2.9		
Violence Crimes					
	MEINC 2	−.000051	−0.8	−.61	
	UNEMMC	−.31	−0.10	−.04	
	NWHITE	+1.6	+2.7	+.46	.35
	MOBILR	+.72	+0.9	+.14	
	SPDVFM	+5.4	+1.5	+.76	
	NSDUMM	−.23	−1.6		

TABLE 4—101 CITIES REGRESSIONS

Dependent Variable Y_{uv}

Regression	Variable	Regression Coefficient	t-Ratio	Elasticity	Multiple R^2
Communities Grouped by *SPDVFM*					
High	*MEINC* 2	+.00033	+0.46	+.80	
	MEINC 4	+.000018	+0.10	+.13	
	UNEMMC	−13.1	−0.76	−.38	
	NWHITE	+3.4	+1.1	+0.45	
	MOBILR	−0.81	−0.17	−.04	.21
	SPDVFM	+31.4	+1.2	+1.4	
	NSDUMM	−0.21	−0.28		
Medium	*MEINC* 2	−.00020	−0.85	−1.1	
	MEINC 4	+.000039	+0.66	+0.60	
	UNEMMC	+7.7	+0.78	+0.45	
	NWHITE	+2.8	+1.5	+0.3	
	*MOBILR**				
	SPDVFM	+33.0	+1.2	+2.2	.30
	NSDUMM	+0.64	+1.7		
Low	*MEINC* 2	+.000060	+0.28	+.73	
	MEINC 4	−.000022	−0.50	−.69	
	UNEMMC	−2.3	−0.33	−.22	.21
	NWHITE	+5.0	+1.7	+0.31	
	MOBILR	−3.5	−1.3	−0.51	
	SPDVFM	+6.9	+0.42	+0.60	
	NSDUMM	+.094	+0.15		

* F-level in stepwise regression <.0001.

TABLE 5—74 CHICAGO COMMUNITIES

Dependent Variable Y_{cc}

Regression	Variable	Regression Coefficient	t-Ratio	Elasticity	Multiple R^2
All 74 Communities					
	MEINC 2*	−.0051	−2.8	−1.2	
	MEINC 4*	+.00038	+1.9	+ .24	
	UNEMMC	+187	+2.3	+ .38	
	NWHITE	+2.69	+0.4	+ .02	.85
	MOBILR	+160	+3.5	+ .32	
	SPDVFM	+22.5	+0.31	+ .06	
Communities Grouped by *SPDVFM*					
High	*MEINC* 2	−.0053	−1.0	− .57	
	MEINC 4	+.00039	+0.6	+ .12	
	UNEMMC	+215	+1.0	+ .40	
	NWHITE	+1.40	+0.1	+ .02	.60
	MOBILR	+166	+1.4	+ .30	
	SPDVFM	−15.9	−0.1	− .05	
Medium	*MEINC* 2	−.0074	−2.4	−2.6	
	MEINC 4	+.00029	+1.5	+ .26	
	UNEMMC	+186	+1.5	+ .41	
	NWHITE	+10.9	+0.7	+ .02	.63
	MOBILR	+126	+0.8	+ .26	
	SPDVFM	−111	−0.4	− .28	
Low	*MEINC* 2	−.0021	−0.9	−1.4	
	MEINC 4	+.000037	+0.1	+ .06	
	UNEMMC	+259	+2.1	+ .64	.72
	NWHITE	+1.1	+0.1	+ .0009	
	MOBILR	+21.9	+0.4	+ .06	
	SPDVFM	−116	−0.6	− .28	

* The sum of coefficients of *MEINC* 2 and *MEINC* 4 is −.0047. The t-ratio is −2.7.

the estimated coefficients of the other variables. The results of regressions including the family-structure variable imply that there is a non-negligible negative effect of income on delinquency, but they fail to show the predicted effect of the income dispersion, or supply variable. Removing mean income of the highest quartile (*MEINC*4) from the regression results in an increase in the statistical significance of mean income of the second lowest quartile (*MEINC* 2) and in the size of the income (demand) effect.[9] The estimated effect and statistical significance of unemployment are small, as are the estimated effects of the proportion nonwhite (*NWHITE*) and mobility (*MOBILR*). The

[9] This is true even when the coefficient and standard error of *MEINC* 2 estimated alone are compared with the sum of the coefficients of *MEINC* 2 and *MEINC* 4 and the standard error of the sum.

TABLE 6—45 CHICAGO SUBURBS >10,000

Dependent Variable Y_{c_4}

Regression	Variable	Regression Coefficient	t-Ratio	Elasticity	Multiple R^2
All suburbs					
#1	*MEINC* 2	−.000085	−0.08	− .09	
	MEINC 4	−.000069	−0.4	− .21	
	UNEMMC	+25.8	+2.7	+ .61	
	NWHITE	+12.2	+1.0	+ .045	.48
	MOBILR	+7.07	+0.8	+ .11	
	SPDVFM	+5.60	+0.07	+ .02	
#2	*MEINC* 2	−.00047	−1.6	− .53	
	UNEMMC	+25.0	+3.4	+ .59	
	NWHITE	+11.9	+1.2	+ .04	.48
	MOBILR	+7.11	+0.9	+ .11	

North-South dummy variable (*NSDUMM*) coefficient indicates that, other things the same, delinquency tends to be lower in the South than it is in the North.

Rather different inferences can be drawn when the subgroup results are examined. Since a priori considerations suggest that stratifying the samples will improve the specification of the model, I shall base further interpretation of the results primarily on the stratified regressions. The highest subgroup yields large, significant estimates of both the supply and demand aspects of income. The family structure variable and *NSDUMM* have estimated effects absolutely larger than those in the aggregate sample. The estimated effects of *MOBILR* and *NWHITE* are negative. The lower groups yield estimated effects of income dispersion similar to the combined sample. The estimated effect of income declines in value and in statistical significance as one moves from high to low subgroups.[10]

Examination of the Chicago communities reveals a striking

[10] Table 3 shows the regression results for the 101 cities' property crimes, individuals aged 17 and under. Since the coefficient of *MEINC* 4 was insignificant, I have shown only the results with *MEINC* 4 eliminated from the regressions. This regression may be compared with regression number 3 in Table 2. The principal difference is that the regression for the younger age group has a lower R^2, and the coefficients are less significant statistically. A possible interpretation is that the variables included in the regressions affect delinquency of both age groups in a similar manner; however, the response of the younger age group is less consistent.

Table 4 shows the regression results for the 101 cities subgroups violence crimes, individuals aged 24 and under. The violence-crimes regression for the 101 cities sample, individuals aged 17 and under, is reported in Table 3. The statistical significance of economic factors is lower in the violence-crimes regressions than in those for property crimes; the coefficient sometimes have the "wrong" signs as well. This is evidence consistent with the hypothesis that the estimates of the income effects do not include the effects of tastes.

difference from the 101 cities; the estimated effect of *SPDVFM* is relatively small and tends to be insignificant. The estimated effect of income dispersion is positive in all subgroups, but highest in the medium. Similarly, the effect of income appears highest in absolute terms in the medium group. The statistical significance of both income variables is also highest there. Since the difference between the coefficients of the income variables in the two highest Chicago subgroups is small compared with the standard errors of either subgroup, it seems fair to conclude that the evidence of the Chicago subgroups does not contradict the 101 cities evidence that income effects are most pronounced in the highest subgroups. The estimated effect of unemployment is positive in all three subgroups. Its size is large, and the coefficients are statistically significant. The estimated effect of mobility in the Chicago subgroups is positive and significant, and the effect of racial composition appears to be negligible.

I now turn to the regressions based on the Chicago suburbs sample, once again drawing attention to the similarity between this sample and the lowest subgroups of the Chicago communities and 101 cities samples. The effect of income on delinquency appears negative. The estimated effect is much more sensitive than in the other samples to the covariation between the two income variables, and when *MEINC* 4 is removed, the estimated effect is similar in significance to the lowest 101 cities subgroup.[11] It is smaller than the estimate in the lowest Chicago communities subgroup. The estimated effect of unemployment is both substantial and significant. The estimated effect of mobility is positive, but of low significance, and the effects of race and family structure appear to be negligible.

I think one can discern a general pattern of behavior that is reflected in these regression estimates. The over-all effect of income on delinquency appears to be negative and more pronounced among "delinquency prone" groups than among others. The negative effect of income on delinquency may be somewhat offset by the positive influence of income on the payoff for property crimes. The offsetting effect also appears to be most pronounced in the most-delinquent groups. Unemployment appears to be a cause of delinquent behavior, but the intersubgroup pattern of the unemployment effect does not seem to be the same as the pattern of income effect. That is, there is no tendency for the high subgroups to exhibit an especially strong response to temporary deviations from normal incomes as measured by unemployment.

[11] The regression results in Table 4 show the effect of *MEINC* 2 alone, *UNEMMC*, *NWHITE*, and *MOBILR* in the *absence* of *SPDVFM*. However, inclusion of *SPDVFM* appears to have only a negligible effect on the regression coefficients derived from this sample.

This study is mainly concerned with the effects of economic factors on delinquency, but it is interesting to note that holding economic and taste factors constant, the addition of a racial variable (*NWHITE*) adds almost nothing to the explanatory power of the regression model in any of the samples.

IV. *Evaluation and Conclusion*

Specification of the Model

In evaluating the results of these regressions, probably the most important question is whether the model has been correctly specified. In fact, there is not a great deal of evidence that it has. The erratic behavior of the family-structure variable is not strong evidence of its appropriateness in the regressions. Futhermore, *SPDVFM* is correlated

TABLE 7—101 COMMUNITIES

Dependent Variable *SPDVFM*

Variable	Regression Coefficient	*t*-Ratio	Elasticity	Multiple R^2
MEINC 2	−.000010	−3.1	−.88	
MEINC 4	+.0000023	+2.9	+.53	
UNEMMC	+.30	+2.5	+.26	.38
MOBILR	+.070	+2.6	+.10	
NSDUMM	+.0076	+1.4		

with the income variables, *MOBILR* and *NSDUMM*, in much the same way as delinquency. Compare the regression results in Table 7 with those of Table 2 (Regresson #1); the elasticities are similar, and the multiple R^2 is higher. Thus it is impossible to rule out the possibility that *SPDVFM* is either caused by delinquency or is so closely related as to be virtually the same phenomenon. While there is no suggestion at present regarding which alternate taste variable would improve the estimates, my experience with the effect of employing alternate taste variables indicates that there is little effect on the estimated income effects.[12] Thus, it is possible that, although we still cannot pro-

[12] The literature on delinquency contains a number of empirical studies in which alternate taste variables have been used. This is not to say that the writers of these studies used such terminology; I am imposing my own framework on what I think these authors were attempting to do. Three important studies were those of Lander for Baltimore [6], Bordua for Detroit [2], and Chilton for Indianapolis [3]. Variables which were used, I think, primarily to hold tastes constant were proportion nonwhite, proportion foreign-born, proportion of homes owner-occupied, proportion of homes substandard, and the proportion of unrelated individuals to families plus unrelated individuals. These studies never reported regression coefficients, but only partial correlation coefficients and/or beta

fess to know all the important variables that should be included in a model of delinquency, we nevertheless have obtained "reasonable" estimates of the effect of economic conditions.

The Effect of Income

Given the reliability of the estimates of the effect of economic conditions on delinquency, I should now like to explore some of their implications. To begin with, it appears that the effect of income on delinquency is not a small one. In extremely delinquent areas, a 1 per cent rise in incomes may well cause a 2.5 per cent decline in the rate of delinquency. The effect of income dispersion—the supply effect of income—probably operates only in very delinquent areas, and it is probably not large relative to the demand effect of income.

Remember that the estimated income effects are net of the effects of other variables in the regressions. If it is true, as seems likely, that family structure is a function of income, then changes in income not only will operate directly upon delinquency rates, but indirectly through family structure, as well. One must not be too mechanical in applying the indirect relationship to policy questions, however. It is unlikely that changes in income transfer payments, such as unemployment compensation, ADC payments, and the like, would have the same effect on family structure as would changes in the earning power of individuals—especially males. Nor would all the indirect income effects be desirable ones. Increased community income would probably attract in-migrants, and through this means rising incomes would probably cause delinquency to increase.

To illustrate the possible influence of economic conditions upon delinquency, consider the following simple example based on the regression estimates of the high 101 cities subgroup. Suppose that the average family income in this subgroup is raised by $500 per year for all income classes. (The effect of the change in income of the first and third quartile is assumed to be reflected in the coefficients for the second and fourth.) The pure income effect on delinquency would be $(-.0093 + .0013)\ 500 = -4.00$, which is to say that the rate would fall by 4 arrests per thousand population, a reduction of somewhat more than 15 per cent of the average rate in the sample. Now suppose that the increase in income has an effect on family structure equivalent to one-half the estimated effect shown in Table 7. I assume only half, because

coefficients. Usually, variables explicitly representing incomes did not pass a conventional t-test for significance. However, I have performed regression analyses of the data for Detroit census tracts (1950) and for Indianapolis census tracts (1960; the Indianapolis data were provided by Roland Chilton). The effect of income appears similar in both the Indianalopis and Detroit samples; I virtually duplicated the Detroit study, obtaining an estimated income effect of $-.0018$ (elasticity $= -.50$), and a standard error of .00091.

income changes cannot be expected to change tastes for delinquency, but to operate on delinquency through family structure via the supervision and market connection effects. From Table 7, we can estimate that a $500 increase in family income will cause a decline in the number of women over 14 who are separated or divorced of about 2.7 per thousand women. One-half of this effect times the estimated impact of family structure on delinquency would imply a decline in the delinquency rate of one. This is 25 per cent of the direct impact of income. In addition, I estimate that the delinquency rate will be reduced by still another one-half of 1 per cent because normal families have higher incomes than broken ones. These rough calculations indicate that a 10 per cent rise in income may be expected to reduce delinquency rates by between 15 and 20 per cent when the income change occurs in highly delinquent areas and is of the type that will reduce the number of broken families as well.

The Effect of Unemployment

The estimated elasticities of the unemployment-delinquency relationship are in general smaller than those of the effect of income. However, aside from the highest 101 cities subgroup estimate, they are all positive and between .05 and .60; this range includes most of the estimates from a previous time-series analysis of the effect of unemployment on delinquency [4]. There is no evidence that the effect of unemployment is higher in the high delinquency subgroups than in the low, as is the effect of income. One may only speculate at this point as to why this may be. Two possible explanations come to mind. One is that, in the groups which have high rates of separation and divorce, high unemployment substantially increases the amount of parental supervision. I think this is intuitively an unappealing explanation. For one thing, the high separation and divorce group probably has high tastes for delinquency; it is problematical whether increased parental supervision in such circumstances would do much to reduce delinquency rates. For another, it seems likely that family morale is low during periods of high unemployment and that parental discipline is not likely to be vigorous.

What seems a more plausible explanation arises from the correlation between family structure and income. Broken families occur more frequently among low-income groups than among high.[13] There are two

[13] This is not only because broken families usually have only one principal earner. It is also because broken families would have lower incomes, on the average, even with two principal earners. A rough indication that this is true is that the differences in mean incomes (as measured by *MEINC* 2) among the subgroups in Table 1 are considerably larger than can be explained by the hypothesis that the differences are solely due to differences in family structure, assuming that families with only female heads have incomes equal to one-half those of families with both male and female heads.

implications of this observation: (1) unemployment compensation is likely to be a more powerful offset to the effects of unemployment for low-income families because there are ceilings on unemployment benefits; (2) in the high subgroup families, unemployment is likely in part to constitute a permanent feature of earnings; this means that differences in unemployment among observations may not consistently reflect differences in deviations of actual income from normal income.[14]

It would be misleading, I feel, to conclude on the basis of the relatively low estimated elasticities of the relationship between unemployment and delinquency that unemployment is consequently a less important explanatory or policy variable than income. For instance, the speculations of the preceding paragraph (and the regression estimates) are consistent with a positive effect on delinquency of changes in the general level of unemployment, even for the high subgroups. Another important consideration is that the variation (as measured by the coefficient of variation) of unemployment rates is much higher than the variation of incomes. This is true in the samples used in this study, and I think it is true in general for these kinds of samples. Thus, to the extent that the "normal" variation of the variable is an index of the constraints within which policy-makers must operate, there is more scope for changing unemployment than for changing income levels directly. Perhaps the most important consideration is that social policy which permanently reduces unemployment will also raise normal levels of income, and reducing unemployment in high-delinquency areas will probably have relatively large effects on normal income levels. This is because unemployment rates in these areas are high to begin with and probably constitute part of the environment determining normal earnings; a 50 per cent reduction in unemployment rates in an area where nearly 10 per cent of the labor force is unemployed may raise average incomes by as much as 5 per cent (perhaps less if the chronically unemployed are less productive than other earners). In the light of the numerical example illustrating the income effect, such a reduction of the unemployment rate could possibly reduce the delinquency rate by 10 per cent.

Finally, while changes in incomes alone do not explain the behavior of delinquency over time, if one assumes an elasticity of the relationship between income and unemployment of .15 (which is consistent with the estimates presented in this paper and with those of a previous study of the relation between delinquency and unemployment over

[14] Another possibility is that, since current income is being held constant in the regressions, residual unemployment among observations is therefore negatively correlated with permanent income. This could lead to a negative coefficient for unemployment if the permanent income effect were strong relative to the unemployment effect.

time) [4] one can "explain" about 15 per cent of the measured increase in juvenile delinquency from 1952 through 1960.[15]

REFERENCES

1. GARY S. BECKER, "An Economic Analysis of Fertility," *Demographic and Economic Change in Developed Countries*, a Conference of the Universities—National Bureau Committee for Economic Research, New York 1960, pp. 209–31.
2. DAVID J. BORDUA, "Juvenile Delinquency and 'Anomie'—An Attempt at Replication," *Social Problems*, Winter 1959, *6*, 230-38.
3. ROLAND J. CHILTON, "Continuity in Delinquency Area Research: A Comparison of Studies for Baltimore, Detroit, and Indianapolis," *Am. Soc. Rev.*, Feb. 1964, *29*, 71-83.
4. BELTON M. FLEISHER, "The Effect of Unemployment on Juvenile Delinquency." *Jour. Pol. Econ.*, Dec. 1963, *71*, 543-55.
5. EVELYN M. KITAGAWA AND KARL E. TAEUBER (eds.), *Local Community Fact Book Chicago Metropolitan Area 1960*. Chicago 1963.
6. BERNARD LANDER, *Towards an Understanding of Juvenile Delinquency*. New York 1954.
7. JACOB MINCER, "Labor Force Participation of Married Women: A Study of Labor Supply." *Aspects of Labor Economics*, a Conference of the Universities-National Bureau Committee for Economic Research, Princeton 1962, pp. 63–97.
8. CLIFFORD R. SHAW AND HENRY D. MCKAY, *Juvenile Delinquency and Urban Areas*. Chicago 1942.
9. UNITED STATES FEDERAL BUREAU OF INVESTIGATION, *Uniform Crime Reports* (various issues), Washington annually.

[15] This statement is based on the change in unemployment rates over this period and changes in per capita juvenile arrests for property crimes, according to [9].

EMBODIED PROGRESS, INVESTMENT, AND GROWTH

By H. A. JOHN GREEN*

The aggregate production function, despite its shortcomings, has been widely used in discussions of economic growth. Among the problems to which it has been applied are the identification of sources of increased productivity and the relationship between investment and growth. In connection with both problems the distinction between "embodied" and "disembodied" technical progress has achieved wide currency. In this paper, after a discussion of some of the properties of an aggregate production function and of "efficiency units," it will be argued that the concept of embodied progress has not received a precise definition. Three alternative interpretations of the concept are given; it is shown that on only one of these interpretations—a somewhat unorthodox one—is embodied progress directly relevant to the relationship between investment and growth.

I. *The Aggregate Production Function*

In a model which incorporates the aggregate production function:

$$Y = F(J, L, t) \tag{1}$$

each of the aggregates Y (gross output or income), J (capital), and L (labor), requires close examination. Gross output is composed, to consider only the first step in disaggregation, of consumption and gross investment. There are therefore two production functions:

$$C = F_C(J_C, L_C, t); \Delta J = F_I(J_I, L_I, t). \tag{2}$$

We make the following four common assumptions: (a) the production functions in (2) are homogeneous of degree one in capital and labor; (b) the amounts of capital employed in the two sectors may be *added* to obtain the total capital employed in the economy (and similarly for labor), so that

$$J = J_C + J_I; L = L_C + L_I; \tag{3}$$

(c) given totals of J and L are efficiently allocated between the two

* The author is professor of economics at the University of Toronto. In revising earlier drafts of this paper, he has benefited greatly from correspondence with Professor R. M. Solow, from discussions with Professor H. Rose, and from the comments of two referees.

sectors; (d) aggregate output Y is some function of the outputs of the two sectors:

$$Y = U(C, \Delta J, t) \tag{4}$$

with $\partial U/\partial C$ and $\partial U/\partial \Delta J$ both positive. Then it can be shown[1] that the aggregation implicit in equations (1)–(4) requires that at any given time t one of the sector production functions is simply a multiple of the other:

$$C = F(J_C, L_C, t);\ \Delta J = R(t) \cdot F(J_I, L_I, t). \tag{5}$$

It follows that at a given time the production possibility curves relating C and ΔJ for different totals of J and L are parallel straight lines with a slope equal in absolute value to $R(t)$. $R(t)$ is the ratio of the marginal costs, and in competitive conditions the ratio of the prices, of the two goods. The ratio of capital to labor employed will always be the same in one sector as in the other.

If the relative prices of C and J remain constant over time, we can use either as *numéraire* in comparing values of Y at different periods. But if they do not, we follow the spirit of many theoretical discussions in taking consumption goods as *numéraire*. Thus:

$$Y = C + p_t \Delta J \tag{6}$$

where p_t is the price of investment goods in terms of consumption goods at time t.[2] It follows from equation (5) that if we set $p_0 = R(0) = 1$, then:

$$p_t \cdot R(t) = 1, \tag{7}$$

since the price of investment goods in terms of consumption goods is inversely proportional to the productivities of given resources in the two sectors.

In considering the capital variable J, we disregard the fact that in any period many different kinds of capital goods are produced.[3] We assume that the single type of capital good produced in a given period can be measured in units each of which is a perfect substitute in production for another unit of the same "vintage." We recognize the existence of distinct capital goods of different vintages, and assume [11] that in

[1] The problem is similar to that of community indifference curves [4]. The result in the text is proved in [5, pp. 47–51].

[2] Equation (6) requires deflation of money income by a price index of consumption goods rather than, as in conventional measures, by a price index of goods in general. The difference between the two measures can be shown to depend on the fraction of income invested (s) and the annual percentage increase in $p_r(\mu)$. If s were 20 per cent and μ were 1 per cent [3], and in year 0 both measures gave the same value for real income, then in year 1 real income according to equation (6) would be approximately $1+s\mu$ ($=1.002$) times real income as conventionally measured.

[3] This problem is considered in [5, pp. 81 ff.].

each period the labor force[4] is distributed among capital goods of different vintages in such a way as to maximize total output, Y. Hence the quantities of capital goods of different vintages $E_1, \cdots, E_n$ are represented by the single variable J in the aggregate production function (1), which is homogeneous of degree one in J and L. If we also assume that the output obtainable from each vintage of capital goods is homogeneous of degree one in E_r and L_r (the amount of labor employed with E_r), it can be shown that the relationship between J and $E_1, \cdots, E_n$ must have the following simple form:[5]

$$J = \sum_r \beta_r E_r. \tag{8}$$

This result implies that in any given period the marginal rate of substitution between capital goods of any two vintages must be a constant, independent of the amount of labor employed and of the quantities of capital goods of these or any other vintages. If we are not to have to recalculate these marginal rates of substitution continually as time

[4] The labor force may be assumed for the moment to consist of homogeneous units; but see the second paragraph of Section II.

[5] The proof is as follows. We have assumed:

$$Y = \sum_r F_r(E_r, L_r, t) = F[J(E_1, \cdots, E_n), L, t].$$

To determine the implications of the aggregation involved, consider the total differentials [5, pp. 12–13]:

$$dY = \sum_r \left(\frac{\partial F_r}{\partial E_r} dE_r + \frac{\partial F_r}{\partial L_r} dL_r\right) = \frac{\partial F}{\partial J} \sum_r \frac{\partial J}{\partial E_r} dE_r + \frac{\partial F}{\partial L} \sum_r dL_r.$$

Since the E_r's are free to take any values, we have for each r:

$$\frac{\partial F_r}{\partial E_r} = \frac{\partial F}{\partial J}\frac{\partial J}{\partial E_r} \quad \text{or} \quad \frac{\partial J}{\partial E_r} = \frac{\partial F_r}{\partial E_r} \div \frac{\partial F}{\partial J}.$$

Now $\partial F/\partial J$ is uniquely determined by J and L, as is $\partial F/\partial L$. With efficient allocation of labor, $\partial F/\partial L = \partial F_r/\partial L_r$ for each r. With constant returns to scale (and smoothly convex-to-the-origin isoquants), $\partial F_r/\partial L_r$ determines E_r/L_r and therefore $\partial F_r/\partial E_r$. Thus $\partial J/\partial E_r$, being the ratio of two expressions each of which is uniquely determined by J and L, is itself uniquely determined by J and L.

But E_r can change arbitrarily (provided that some other E_s also changes) without affecting the value of J or, therefore, the value of $\partial J/\partial E_r$. It follows that for each r, $\partial J/\partial E_r$ is a constant, so that we may write:

$$J = k + \sum_r \beta_r E_r.$$

A proportional change in each E_r and L_r will change Y and L in the same proportion. If Y is homogeneous of degree one in J and L, this must also increase J in the same proportion (unless $\partial F/\partial J = 0$). Hence J must be homogeneous of degree one in the E_r's, so that k in the last equation is zero, and:

$$J = \sum_r \beta_r E_r \qquad Q.E.D.$$

passes (which no one has shown any inclination to suggest), we must assume that changes in technology do not affect the relative values of the β_r's, and that in any given period physical depreciation affects in the same proportion the values of capital goods of all vintages.

If technical change increases the output obtainable from a given amount of labor and a given collection of capital goods, we may choose to represent it in one of two ways. We either may say that the same capital and labor represent more "efficiency units" than before, and that the production function has not changed or, alternatively, we may continue to represent the same capital goods and labor by the same number of efficiency units as before (after allowing for physical depreciation of capital), and say that the production function has shifted. We shall adopt the latter course, since it facilitates the distinction among different sources of increase in productivity [2] [7].

Hence we take as our "standard unit" of capital a unit of a particular vintage (e.g., one produced in year 0) in a particular condition (e.g. brand-new). Let a brand-new capital good of vintage r be a perfect substitute for γ_r standard units; then it represents γ_r efficiency units. Of course, $\gamma_0 = 1$. If at the beginning of period t we have $E_0, E_1, \cdots, E_{t-1}$ capital goods of vintages $0, 1, \cdots, t-1$, and capital goods suffer physical depreciation at a rate δ per period (beginning in the period following that in which they are built), then:

$$J = \sum_{r=0}^{t-1} \beta_r E_r = \sum_{r=0}^{t-1} \gamma_r e^{-\delta(t-1-r)} E_r. \tag{9}$$

We take as our standard unit of labor a particular man-hour performed in year 0, and assume that the marginal rate of substitution between any other man-hour and the standard unit is a constant, independent of the quantity of capital employed and of the state of technology. A man-hour which is a perfect substitute for x standard units represents x efficiency units of labor.

In common with many writers, we assume that technical progress is Hicks-neutral in each sector, and increases the output obtainable from given numbers of efficiency units of capital and labor. The rates of technical progress are assumed for ease of exposition to be constant in each sector, but not necessarily equal. Thus:

$$C = e^{at} f(J_C, L_C); \; \Delta J = e^{bt} f(J_I, L_I). \tag{10}$$

The output of the consumption sector is measured in units which are assumed to be comparable over time. The output of the investment sector is measured in *efficiency units* of capital as defined above.

It follows from our earlier argument that the only thing that can change the relative prices of consumption goods and efficiency units of

capital is a difference between the rates of technical progress in the two sectors. From equations (5), (7), and (10):

$$R(t) = \frac{1}{p_t} = e^{(b-a)t} \tag{11}$$

and from equations (6), (10), and (11):

$$Y = C + p_t \Delta J = e^{at} f(J_C, L_C) + \frac{e^{bt}}{e^{(b-a)t}} f(J_I, L_I)$$

$$= e^{at}[L_C \phi(J_C/L_C) + L_I \phi(J_I/L_I)] = e^{at} L \phi(J/L),[6]$$

so that:

$$Y = e^{at} f(J, L). \tag{12}$$

Thus technical progress increases the output Y obtainable from given numbers of efficiency units of labor and capital at a rate equal to the rate of technical progress in the *consumption* sector. This is a consequence of our decision to measure Y in consumption units; if technical progress is faster (slower) in the investment sector, its effect on Y is reduced (increased) by a compensating fall (rise) in p_t.

II. *Embodied Technical Progress*

In the remainder of this paper we shall be concerned with two questions in the discussion of which the aggregate production function has been used. The first problem is to identify sources of increased productivity, the second to determine the effect on an economy's growth path of changes in the proportion of national income invested.

Of the sources of productivity increase, one will be discussed briefly, as it is not our major concern. Let productivity be defined as output per man-hour. It may be that as a result of improved education [2], experience [1], and training, a man-hour of labor comes to represent, as time passes, a greater number of efficiency units of labor as defined in the last section. Specifically, let labor measured in efficiency units (L) and in man-hours (M) be related by $L = Me^{mt}$. Then it is not difficult to show that the production function $Y = e^{at} f(J, L)$ can be written as $Y = e^{ct} \psi(J, M)$ only if both $f(\ )$ and $\psi(\ )$ have the Cobb-Douglas form, so that:

$$Y = e^{at} J^{\alpha} L^{1-\alpha} = e^{at} J^{\alpha} (Me^{mt})^{1-\alpha} = e^{[a+m(1-\alpha)]t} J^{\alpha} M^{1-\alpha}. \tag{13}$$

Nelson appropriately terms this source of increased productivity "technical progress embodied in labor"; he also points out complexities sur-

[6] As is noted in the third paragraph of Section I above, it is always true that $J_C/L_C = J_I/L_I = J/L$.

rounding the notion, among them that one should consider labor, as well as capital, as classifiable into "vintages" [7, pp. 587–88].

We take labor henceforth to be measured in efficiency units, and turn to the concept of technical progress embodied in capital. This is the part of technological advance that can be exploited only if it is incorporated in new capital goods. Embodied technical progress results in a greater output from labor employed with new capital goods only; disembodied (or more accurately unembodied)[7] technical progress affects equally the productivity of all labor, irrespective of the vintage of the capital goods with which it is equipped. In the special case considered in the previous section, where an aggregate production function exists and efficiency units of capital can be defined, embodied progress is said to be "purely capital-augmenting" [10, p. 59]. The number of efficiency units of capital, γ_r, represented by a brand-new capital good of vintage r is greater, the larger is r—that is, the later the date at which the capital good is produced.

Now the quantities $\gamma_r E_r$ of equation (9) are measured in efficiency units. The quantities E_r themselves are not. Both Solow [11] and Phelps [8] speak of the E_r's as being measured in "physical units," so that when embodied progress is purely capital-augmenting it takes the form of an increase over time in the number of efficiency units of capital corresponding to a newly produced physical unit. But if, as is presumably the case, later models differ in size, form, and mode of operation from earlier models, what "physical unit" has economic significance?[8]

There seem to be at least three possible answers to this question. Firstly we may assume, as Solow does in his latest contribution to the field [10, p. 29], that consumption goods and capital goods are physically identical, but that a more recently produced unit of output represents more efficiency units of capital (but no more "quality units" of consumption) than one produced earlier. This is equivalent to defining a physical unit of capital as that quantity which costs the same amount to produce as a unit of consumption goods. On this interpretation, the rate of embodied technical progress can be expressed in any one of four (approximately) equivalent ways: (i) as the rate of decline in the price of an efficiency unit of capital in terms of consumption goods; (ii) as the rate

[7] My colleague S. G. Triantis points out that the word "*dis*embodied" suggests that the technical progress in question was embodied at one time, and is so no longer.

[8] The problem of defining physical units would still exist if embodied progress were not assumed to be purely capital-augmenting. In the more general case embodied progress would mean only that with equal amounts of labor, $L_r = L_{r+1}$, applied to equal numbers of "units," $E_r = E_{r+1}$, of (brand-new) capital goods of two different vintages, we should have $Y_{r+1} = F_{r+1}(E_{r+1}, L_{r+1}) > Y_r = F_r(E_r, L_r)$. But until we know the units in terms of which $E_r = E_{r+1}$, we cannot know whether embodied progress has taken place, or at what rate. One set of units might give $Y_{r+1} > Y_r$, another $Y_{r+1} < Y_r$.

of increase in the number of efficiency units of capital obtainable for the sacrifice of a unit of consumption goods; (iii) as the rate of increase in the ratio of the stock of capital measured in efficiency units to the cost, measured in consumption units, of replacing it with current models of capital goods representing the same number of efficiency units; (iv) as the difference between the rates of technical progress in the investment sector, b, and the consumption sector, a, as these rates were defined in the preceding section.

It may be argued that the foregoing is unacceptable as a general definition of embodied progress, corresponding as it does to a special interpretation of a special model.[9] It remains true that the four expressions above are equivalent to the rate of embodied progress in at least the theoretical part of Solow's work [10]. Moreover, this is the interpretation that must be used in the analysis of the relationship between investment and growth and in the definition of Solow's one-period "rate of return," as will be shown in the next section.

A second interpretation of the rate of embodied progress is purely statistical. In the empirical part of [10], Solow builds up a number of series of the stock of capital in efficiency units by applying different "improvement factors" to a deflated investment series. The deflator is a price index of capital goods, so that the rate of embodied progress, or the improvement factor, is simply a correction of the capital-goods price index for unmeasured quality changes, and capital measured in physical units is a total of deflated, uncorrected investment figures. If our first interpretation of embodied progress had been applied, the deflator would have been a price index of consumption goods. It will be shown in the next section that, since the price indices of capital goods and consumption goods diverged, some adjustment is necessary in Solow's calculations of the "rate of return."

A third interpretation would distinguish between improvements in methods of production and improvements in models.[10] Let us suppose that in year 0 given numbers of efficiency units of capital and labor can produce 100 standard units of capital. In year 1 the same efficiency units of labor and capital can produce either 102 standard units or 51 units of a new model; each unit of the new model represents 2.06 efficiency units. Then the productivity of a given number of efficiency

[9] I defined embodied progress in this way in [5, p. 94], and in an earlier draft of this paper. Professor Solow has insisted in correspondence that this is not what he and others have meant by embodied progress in general, and J. K. Whittaker of the University of Bristol has pointed out to me that if production-possibility curves are not straight lines (as they are assumed to be in this paper), the relative prices of capital goods and consumption goods can change for reasons quite unconnected with technology.

[10] If we take into account the improvements in the quality of the labor force discussed at the beginning of this section, we may distinguish improvements in *methods*, improvements in *models*, and improvements in *men*.

units of capital and labor in the investment sector has increased from 100 to $51\times 2.06 \doteq 105$ efficiency units, so that $b=5$ per cent. This can be broken down into an improvement in methods, which would have increased output by $b'=2$ per cent (from 100 to 102 efficiency units) if there had been no model improvement, and a further $b''=3$ per cent because of the new model. The rate of model improvement in the investment sector seems to me to correspond, within this framework, to a common-sense notion of technical progress "embodied" in new capital goods.

But once we have distinguished improvements in models and methods in the investment sector, is there not an analogous distinction in the consumption sector? May we not also split a into a' (unembodied improvements in methods of production) and a'' (improvements in quality)? If this is done, the relationship between our first interpretation of capital-embodied technical progress as $(b-a)$ and our third as b'' becomes interesting. They clearly come to the same thing only if $b''=b-a$, so that $a=b'$. A special case is Solow's model in [10], where *all* technical progress is embodied in new capital goods, so that $b''>0$ and $a=a'=a''=b'=0$. But what of the "purely disembodied" case of Solow's celebrated 1957 article [12]? If our two interpretations are to be reconciled, we must have $a=b$ and $b''=0$. This requires that the rate of unembodied progress in the investment sector (b') equal the over-all rate of progress in the consumption sector (a).

These, however, are special cases. There is no reason why a', a'', b', and b'' should obey such restrictions. And I do not see that there is any great advantage, either theoretical or empirical, in distinguishing between improvements in methods and improvements in models. But there is no doubt that very great economic significance attaches to $(b-a)$, the difference between the over-all rates of technical progress in the two sectors. This, I contend, is what Solow and Phelps mean implicitly by the rate of embodied technical progress, though it corresponds to the common-sense meaning of the term only in special cases. Moreover, since there is no reason why the rate of technical progress in the consumption sector should not exceed that in the investment sector, embodied technical *regress* $(b-a<0)$ is perfectly possible, on this definition.

But whether we call it the rate of embodied technical progress, or the excess of the rate of technical progress in the investment sector over that in the consumption sector, is it true that $(b-a)>0$? If we had price indices of capital goods and consumption goods which made proper allowance for quality changes, the question could be easily settled. Moreover, deflation of investment by the capital-goods price index would yield an estimate of the stock of capital in efficiency units, and

the value of a could then be estimated by the method of Solow's 1957 article [12].

Published price indices for the United States [3] show the prices of capital goods rising faster than those of consumption goods at the rate of about 1 per cent per annum. If these indices were acceptable, they would of course imply that $a > b$. It is widely believed that the capital-goods price indices underestimate quality improvements. But presumably price indices of consumption goods err in the same direction—and this error, while it would not in itself affect estimates of the stock of capital measured in efficiency units, would certainly lead to underestimates of the rate of growth of real income and of the value of a. Until it is possible to determine the sign of $(b-a)$, we do not know whether to second Solow's endorsement of the view [11, p. 62] that our children's saving will be more fruitful than our own.

III. *Savings, Growth, and the Rate of Return*

The aggregate production function and the concept of embodied technical progress have been used in discussions of the effect of a change in the investment ratio on the rate of growth, and of the rate of return on investment. It will be shown in this section that the two sectoral rates of technical progress a and b are crucial in these discussions, and that, in so far as the rate of embodied progress is relevant, it is only when it is interpreted as $(b-a)$.

Let us specialize the production function (12) to the Cobb-Douglas form:[11]

$$Y = e^{at} J^{\alpha} L^{1-\alpha}. \tag{14}$$

J is measured in efficiency units. Let us write K for capital measured by its replacement cost in terms of consumption goods, i.e., the cost of replacing it by J efficiency units of capital of the latest model, produced by the latest methods. Then, as was seen in the last section:

$$J = e^{(b-a)t} K. \tag{15}$$

Let us assume that L grows at the constant rate n, so that $L(t) = L_0 e^{nt}$. Then, writing G for rates of growth:

$$G_Y = a + (1 - \alpha)n + \alpha G_J = a + (1 - \alpha)n + \alpha(b - a) + \alpha G_K. \tag{16}$$

Of the many assumptions we could make about savings, we shall select two. One is Phelps's [8, p. 553], that gross savings and gross investment are a constant fraction of gross income, Y. To obtain net invest-

[11] Matthews [6] has argued that some of the substantive results of this section will not hold if the production function has some other form, but this does not affect the point we seek to make.

ment from gross investment, or net income from gross income, when all are measured in terms of consumption goods as *numéraire,* we deduct physical depreciation at the rate δ, and subtract capital losses at the rate $(b-a)$ if $b>a$, or add capital gains at the rate $(a-b)$ if $a>b$, because of the changing price of an efficiency unit of capital in terms of consumption goods. Thus:

$$\frac{dK}{dt} = sY - (\delta + b - a)K. \tag{17a}$$

Alternatively we may assume that net investment is a constant fraction of net income, so that:

$$\frac{dK}{dt} = s[Y - (\delta + b - a)K]. \tag{17b}$$

Since $G_K = 1/K(dK/dt)$, we obtain from (16), (17a), and (17b) two expressions for G_Y:

$$G_Y = a + (1 - \alpha)n + \alpha\left(s\frac{Y}{K} - \delta\right) \tag{18a}$$

$$G_Y = a + (1 - \alpha)n + \alpha\left[s\frac{Y}{K} - s\delta + (1 - s)(b - a)\right]. \tag{18b}$$

In either case, if a, b, δ, s, α, and n are all constant, a constant rate of growth requires that Y/K be constant, so that $G_Y = G_K$. Then (16) gives:

$$\bar{G}_Y = \frac{a + \alpha(b - a) + (1 - \alpha)n}{1 - \alpha} = \frac{(1 - \alpha)a + \alpha b + (1 - \alpha)n}{1 - \alpha}. \tag{19}$$

This rate of growth, as is well known, is independent of s.

The "total" rate of technical progress (equal to the constant rate of growth of output per efficiency unit of labor) may be expressed as $1/(1-\alpha)$ times: *either* (i) $a+\alpha(b-a)$, the rate of unembodied technical progress plus α times the rate of embodied technical progress;[12] *or* (ii) $(1-\alpha)a+\alpha b$, the weighted mean of the rates of technical progress in the consumption sector and the investment sector, the weights being the elasticities of output with respect to labor and capital respectively.

We follow Phelps [8, pp. 561 ff.] in working out the speed of adjustment to a given equilibrium rate of growth after, say, a change in the savings ratio. The first equation of each pair is Phelps's result based on the gross savings assumption (17a); the second is based on the net savings assumption (17b). Multiplying (18a) and (18b) by Y, we obtain

[12] What is here called $(b-a)$ corresponds to λ in Solow [10], but to λ/α in Phelps [8].

the differential equations:

$$\frac{dY}{dt} = [a - \alpha\delta + (1 - \alpha)n]Y + \frac{\alpha s Y^2}{K} \tag{20a}$$

$$\frac{dY}{dt} = [a - \alpha\delta s + (1 - \alpha)n + \alpha(1 - s)(b - a)]Y + \frac{\alpha s Y^2}{K}\cdot \tag{20b}$$

Replacing K by J by means of (15), and solving for J in terms of Y by means of (14), we have:

$$\frac{Y^2}{K} = Y^{2-(1/\alpha)}\, e^{[a+\alpha(b-a)+(1-\alpha)n]t}. \tag{21}$$

The speeds of adjustment which emerge from the solutions to these equations are (*cf.* [8, p. 561]):

$$\rho' = b + (\delta + n)(1 - \alpha) \tag{22a}$$

$$\rho'' = b + (\delta + n)(1 - \alpha) - (1 - \alpha)(1 - s)(\delta + b - a). \tag{22b}$$

By means of (19) we may conveniently express these results in terms of the equilibrium rate of growth $\overline{G}_Y$, as follows:

$$\rho' = (1 - \alpha)[\overline{G}_Y + \delta + b - a] \tag{23a}$$

$$\rho'' = (1 - \alpha)[\overline{G}_Y + s(\delta + b - a)]. \tag{23b}$$

Consider two economies with the same long-run equilibrium rate of growth $\overline{G}_Y$ and the same values of α and δ, but with different values of a and b. Under the gross savings assumption, (23a) shows that if s is increased the speed of adjustment to the new and higher growth path will be more rapid in the economy with the higher rate of capital-embodied progress in our first sense—that is, in the economy in which the rate of technical progress in the investment sector exceeds that in the consumption sector by more (or falls short of it by less). On the net savings assumption, (23b) also shows that the speed of adjustment will be greater, the greater is $(b-a)$. If $(\delta+b-a)$ is positive, however, the speed of adjustment will be less than on the gross savings assumption, and more rapid, the larger is the new value of s.

We conclude with a comment on the "rate of return." "The thought-experiment is to sacrifice one unit of consumption at time t in favor of investment, and then ask what is the largest increment of consumption that can be enjoyed at time $t+1$ without impairing consumption possibilities in any future period" [10, p. 60]. Solow's expression is [10, p. 61]:

$$r = \left(\frac{\partial Y}{\partial J}\right)_t - \frac{\delta + \lambda}{1 + \lambda} \tag{24}$$

where $(\partial Y/\partial J)_g$ is the gross marginal product of J, δ is the rate of physical depreciation, and λ is the rate of embodied technical progress. This can be shown to be equivalent to the rate of profit in terms of consumption goods as *numéraire* (*cf.* [9, p. 40]):

$$r = \frac{1}{p_t}\left(\frac{\partial Y}{\partial J}\right)_n + \frac{1}{p_t}\frac{dp_t}{dt} \tag{25}$$

where p_t is the price of an efficiency unit of capital, $(\partial Y/\partial J)_n$ is the net marginal product of J, and λ is interpreted as the rate of increase per period in the number of efficiency units of capital obtained for a unit of consumption goods sacrificed.[13]

The rate of embodied progress relevant for these calculations is of course the $(b-a)$ of our first interpretation. But the λ calculated by Solow in the empirical part of [10] is the correction of the capital-goods price index of our second interpretation. The calculation which would have yielded an estimate of $(b-a)$ (or rather, since Solow assumes $a=0$, of b) would have resulted, as was noted earlier, from a deflation of investment by a price index of consumption goods. Since the price index of consumption goods rose less rapidly than that of capital goods, deflation by the former would have given a more rapidly rising series of uncorrected investment figures, and a smaller "improvement factor" would have had to be applied to yield a given stock of capital in efficiency units. The difference in the rates of growth of the two price indices was roughly 1 per cent per annum, so that Solow's favored estimate of .05 for λ corresponds to a value of about .04 for $(b-a)$. Of the components of the rate of return [10, p. 89], the only one significantly affected is the deduction for "obsolescence,"[14] $\lambda/(1+\lambda)$, which becomes roughly .04 rather than .05, yielding a rate of return of .27 rather than .26. This is, of course, not the only adjustment which should ideally be made in Solow's calculations—others follow from the desirability of allowing a as well as b to be positive, from the possibility of improvements in men, as well as in methods and models (see the second para-

[13] Solow implicitly assumes that $p_t=1$, so that

$$p_{t+1} = \frac{1}{1+\lambda}; \quad \frac{1}{p_t}\frac{dp_t}{dt} = -\frac{\lambda}{1+\lambda}.$$

Since depreciation of capital goods constructed in period t does not begin until period $t+1$,

$$\frac{1}{p_t}\left(\frac{\partial Y}{\partial J}\right)_n = \frac{1}{p_t}\left[\left(\frac{\partial Y}{\partial J}\right)_g - \delta p_{t+1}\right] = \left(\frac{\partial Y}{\partial J}\right)_g - \frac{\delta}{1+\lambda}.$$

Thus the equivalence of the two expressions for r, (24) and (25), is established.

[14] See also [8, pp. 565–66]. According to normal usage, the "rate of obsolescence" would correspond to the rate of model improvement, b'', in our third interpretation of embodied progress. This, as was pointed out earlier, is equal to $(b-a)$ only on special assumptions.

graph of Section II above), and from the discrepancy between conventional and theoretical measures of income (see footnote 2). The present minor modification is stressed only to illustrate the difference between the first and second of our interpretations of embodied technical progress.

IV. *Summary*

The usual aggregate production function model can be shown to commit its users to the assumption that the relative prices of capital goods and consumption goods are independent of their outputs, and change over time only as a result of a difference between the rates of technical progress in the investment sector and the consumption sector. For the purpose of this argument the output of the investment sector is measured in "efficiency units" of capital, where an efficiency unit of capital or labor is that amount which is a perfect substitute for a standard unit produced or performed in a given base period. Technical progress is assumed to take the form of a Hicks-neutral increase in the output of given numbers of efficiency units of capital and labor so defined.

The rate of "embodied technical progress" used by Solow and Phelps in discussions of the effect on short-run and long-run growth of an increase in the fraction of income invested, and in calculations of the "rate of return," must be interpreted as the excess of the rate of technical progress in the investment sector over that in the consumption sector. There is no reason in principle why this should be positive. Only on special assumptions does this interpretation correspond to the common-sense notion of capital-embodied technical progress as the development of new *models* of capital goods, as contrasted with unembodied progress as the development of new *methods* of producing goods of a given type.

References

1. K. J. Arrow, "The Economic Implications of Learning by Doing," *Rev. Econ. Stud.*, June 1962, *29*, 155–73.
2. E. F. Denison, *The Sources of Economic Growth in the United States and the Alternatives Before Us.* Committee on Economic Development Suppl. Paper No. 13. New York 1962.
3. R. A. Gordon, "Price Changes: Consumers' and Capital Goods," *Am. Econ. Rev.*, Dec. 1961, *51*, 937–57.
4. W. M. Gorman, "Community Preference Fields," *Econometrica*, Jan. 1953, *21*, 63–80.
5. H. A. J. Green, *Aggregation in Economic Analysis: An Introductory Survey.* Princeton 1964.
6. R. C. O. Matthews, "The New View of Investment: Comment," *Quart. Jour. Econ.*, Feb. 1964, *78*, 164–72.

7. R. R. NELSON, "Aggregate Production Functions and Medium-Range Growth Projections," *Am. Econ. Rev.*, Sept. 1964, *54*, 575–606.
8. E. S. PHELPS, "The New View of Investment: A Neo-Classical Analysis," *Quart. Jour. Econ.*, Nov. 1962, *76*, 548–67.
9. P. A. SAMUELSON, "The Evaluation of Social Income: Capital Formation and Wealth," in F. A. Lutz and D. C. Hague, eds., *The Theory of Capital*, London 1961, pp. 32–57.
10. R. M. SOLOW, *Capital Theory and the Rate of Return*. Amsterdam 1963.
11. ———, "Investment and Technical Progress," in K. J. Arrow, S. Karlin, and P. Suppes, eds., *Mathematical Methods in the Social Sciences 1959*, Stanford 1960, pp. 89–104.
12. ———, "Technical Change and the Aggregate Production Function," *Rev. Econ. Stat.*, Aug. 1957, *39*, 312–20.

COMMUNICATIONS

Steel Imports and Vertical Oligopoly Power: Comment

In the *American Economic Review* of September 1964 [1, pp. 626-55], Walter Adams and Joel Dirlam criticize the domestic steel industry on a number of counts—particularly regarding technology, production, pricing practices, vertical integration, and imports. This communication will be limited to an examination of the authors' contentions about steel industry technology. Although several other assertions of the authors justify a re-examination, space limitations are such that this communication will not be able to examine these in the detail necessary.

The authors contend that U.S. steel producers have lagged behind foreign producers in investing in technologically improved facilities and, further, that they have invested in obsolete equipment and that improvements have been adopted only when the industry has been forced to do so by the increasing pressure of foreign competition [1, pp. 626, 646]. To support their charges, the authors cite the examples of the oxygen furnace and the continuous-casting process.

Specifically, the authors state that during the 1950's, the U.S. steel industry bought 40 million tons of the wrong kind of capacity, namely the open-hearth furnace. They state that "Much of the U.S. industry's current investment in plant and equipment is calculated not to add to capacity, but to correct the embarrassing investment errors of the last decade" [1, p. 627].

Increases in open-hearth capacity in the steel industry are shown in Table 1.[1]

Although there was a net increase in open-hearth capacity of nearly 40 million tons, only about 16¼ million tons were accounted for by the construction of *new* facilities during this period. Further, the steel companies built only 5.6 million tons of *new* open hearths—not 40 million tons—during the portion of this period following 1953, when information about the oxygen furnace first became generally available to the industry. The remainder of the increase in capacity represented the results of improved utilization of *existing* facilities—an approach which the steel companies found to be economically more desirable than construction of completely new facilities.

The authors surely do not mean to imply that a fully integrated steel mill has complete control over the fluidity of its capital use. On the contrary, most of the capital becomes sunken and fixed once it is brought into being. The best that can be done in connection with technological changes is to attempt to keep the existing physical plant from becoming uneconomical to operate by making steady improvements and modernizations as these become technologically and financially feasible. It is only in the construction of new facilities

[1] Based on American Iron and Steel Institute data.

TABLE 1—INCREASE IN OPEN-HEARTH STEELMAKING CAPACITY BETWEEN 1950 AND 1960

	Increases	Decreases	Net Tonnage Change in U.S.
Revision of ratings of existing furnaces*	31,569,060	2,561,600	29,007,460
New furnaces	16,250,760	—	16,250,760
	47,819,820	2,561,000	45,258,220
Less: Old furnaces abandoned	—	5,621,080	5,621,080
Net Increase			39,637,140

* Primarily due to the use of oxygen in existing furnaces, introduction of labor-saving equipment, and better refractories.

that a firm has the option of using existing technology or new methods, even if the latter were operationally suitable at the time.[2]

The authors assert that while the U.S. steel industry was emphasizing open hearths, the European and Japanese firms were instead installing oxygen furnaces at a "breakneck pace" [1, p. 627]. The record shows, however, that from 1953 to 1960 the U.S. producers increased their open-hearth capacity by only about 23 per cent in contrast with an increase of about 79 per cent for members of the European Coal and Steel Community and of approximately 169 per cent for Japan.

The authors criticize the U.S. industry for being slow to adopt the oxygen process in spite of its alleged *widespread* use in Europe more than ten years ago [1, p. 647]. The oxygen process was first used commercially in Austria in 1952 on two 35-ton furnaces with an annual capacity of about 400,000 tons. In Europe, another shop was started in 1953, still another in 1955, one more in 1956, two more in 1957, four more in 1958, and two more in 1959. The total capacity of all these operations approximated 4,000,000 net tons. An additional five shops were started up in 1960, bringing the total oxygen-furnace capacity in Europe by 1960 to about 5.3 million net tons. These data obviously cannot be interpreted to mean that oxygen-furnace-produced steel was *widespread* in Europe ten years ago. In fact, as late as 1963, only about 6 per cent of the steel produced in Europe was made by oxygen furnaces; this compares with about 7½ per cent in the United States by 1963. By this measurement, U.S. producers were technologically ahead of—not behind—European producers.

To understand the rate of introduction of the oxygen furnace in the United States, it is necessary to examine both economic and engineering factors. The

[2] Subsequent discussion will indicate why such a choice was not present for much of the period.

early oxygen furnaces used in Europe had insufficient capacity to meet the requirements of the large, integrated steel mills of the United States, where many of the open hearths were producing heats (i.e., "batches") of 300 to 500 tons.

Although it would have been possible for large, integrated U.S. mills to have constructed a number of small oxygen furnaces, there are two apparent reasons why it was not economically desirable for U.S. producers to do this. First, large producers undoubtedly determined that their large open-hearth furnaces were more efficient in producing large tonnages than a large number of small-capacity oxygen furnaces would be. This, of course, would not have precluded the very small U.S. producers from constructing small oxygen furnaces, since these producers did not have the same need for volume; generally, however, these *very small steel producers do not have blast furnaces,* which are a prerequisite to an oxygen steelmaking operation. A second reason why U.S. producers deferred making investments in oxygen furnaces is undoubtedly that they felt they could develop higher-capacity oxygen furnaces in the not-too-distant future and that such furnaces would be much more efficient than small-capacity furnaces, which were the only type that could have been built during this earlier time period. To make the oxygen furnace feasible for widespread use in the United States, a great deal of research and development first had to be undertaken; this was to come at a future date.

At present, some 300-ton oxygen furnaces are already in operation in the United States and others are being built, but the average-size oxygen furnace now in operation in the United States is about 140 tons. Until the large oxygen furnaces were developed, a number of companies adapted their large, modern open-hearth furnaces with oxygen lances and have operated them as efficient production units. In light of these facts, it seems quite apparent that there would have been little or no advantage to the major U.S. companies from oxygen furnaces, rather than continued use and improvement of the existing open hearths, during the years before the large oxygen furnaces were perfected.

The process of the introduction of the oxygen furnace is neither hit-or-miss nor one of massive replacement. Companies tend to replace the oldest and highest-cost open hearths first, and only replace those where capital could not be utilized more efficiently elsewhere. Now that 150- and 200-ton furnaces have been engineered, one company after another is building this type of capacity.

So long as steel produced by conventional methods can compete with that produced by newer technology, it would not be a wise economic decision to scrap the existing facilities and to destroy vast investments in fixed capital. It can be expected that newer technology will become more general as old facilities wear out and are scrapped. This is why it is unlikely that there will be any future purchase of *new* open-hearth capacity, but there will continue to be modifications, adaptations, and modernizations of such facilities so long as they are cost-competitive. Any decision that involves the contemplated scrapping of open hearths and their replacement by oxygen furnaces is carefully weighed against the alternative uses of funds.

Another aspect of technology that the authors discuss involves the use of continuous casting, a process made economically practical as a result of the oxygen furnace. Continuous casting at the present time involves an uninterrupted process of steelmaking and steel-forming of in-process shapes for future conversion. The authors again claim that the U.S. industry adopted the continuous-casting method slowly and only with great reluctance. But, once again, it is necessary to point out that the structure of steelmaking operations in the United States is very different from that in Europe, and that the size of the economical producing unit in the United States is much larger. Even in the United States, the mills that found it economical to adopt continuous-casting techniques at first were the smaller plants.

Although the authors do indicate that continuous casting abroad "began to move into high gear about eight years ago," they do not add that most of the 40 machines installed from 1955 to 1962 had very small capacities. Only 10 of the 40 had annual capacities in excess of 100,000 tons, and the largest was rated at about only 350,000 tons. This contrasts with modern blooming and slabbing mills (which they replace) in the United States that have annual capacities in excess of 3,000,000 tons.

Although it would have been possible for large, integrated U.S. producers to construct small continuous-casting units at an earlier date, such an approach would undoubtedly have resulted in higher unit costs. For the small producer, however, who could not produce the volume of steel necessary to make the operation of large-volume blooming, slabbing, and billet mills feasible, small-capacity cntinuous-casting units were economical. Continuous casting was not installed by any large producers until a large-scale process was developed which would be more efficient than what the producer had at the moment, and until the return on such an investment was anticipated to be higher than any alternative use of funds.

In summary, with reference to technological advances, it is imperative to recognize that the introduction of new methods is not dictated solely by the availability of a new process. The very technological nature of the industry itself affects the wide use of newer methods, since early applications may be restricted to smaller producing units. It also is important to recognize that a basic factor affecting decisions about investments is consideration of alternative uses of money. There are many ways that a firm can spend funds for investment in plant and equipment. The key decision is to spend the monies where they will earn the best returns. With different rates of growth in demand in the United States, Europe, and Japan, the facility decisions necessarily must be different. With rapidly expanding demand, it is financially possible for a firm to install new capacity; without such growing demand, alteration of existing facilities frequently is a more economical move.

Reuben E. Slesinger*

* The author is professor of economics at the University of Pittsburgh.

Reference

1. Walter Adams and Joel B. Dirlam, "Steel Imports and Vertical Oligopoly Power," *Am. Econ. Rev.*, Sept. 1964, *54*, 626-55.

Steel Imports and Vertical Oligopoly Power: Comment

Walter Adams and Joel Dirlam in their recent article set out to examine "the comparative showing of the U.S. steel industry in an increasingly competitive world economy" [1, p. 626]. They feel that the increase in steel imports from 1.2 per cent to 5.6 per cent of total domestic consumption from 1955 to 1962 is symptomatic of a failure on the part of the steel industry. Five substantial causes are then produced to explain the supposedly poor comparative showing.

Three of the causes, "the recovery from wartime destruction and the dismantling of the European and Japanese steel industries," "a substantial increase of world-wide capacity," and "a generally export-orientated price policy by European and Japanese producers" [1, p. 626], were of course beyond the direct control of the U.S. steel industry. But, these three causes along with the relatively low U.S. steel tariffs would yield a significant tendency in themselves for steel imports to increase. A judgment of the U.S. steel industry such as the authors attempt should then start with an assessment of the impact of these exogenous causes. Therefore we are surprised that the authors do not attempt to weigh the effects of the better age structure of foreign capital.[1] Nor do they assess the impact that changes in world overcapacity had on the strength of export market competition during the period considered.[2] Further they cast doubt on the prevalence of "dumping" in stating that the Japanese "could not be convicted of dumping—even in an arithmetic sense—because their U.S. prices were identical to their home market prices" [1, p. 649]. Yet, what was absorption by Japanese exporters of the transport costs between Yokohama and Pittsburgh must be termed either charity or dumping.

Although an evaluation of the impact of these causes, along with low steel tariffs, is absolutely basic to a sensible discussion of other factors which might contribute to increased steel imports, almost the whole article is devoted to an analysis of "vertical oligopoly power." The authors accordingly assign as a fourth cause of higher steel imports "a generally insensitive, utility-like, administered price policy by domestic producers" [1, p. 626].[3] The authors at-

[1] The question of whether older capital in general puts a competitor at a disadvantage is a controversial problem which has received much attention [3] [4]. The great importance of an integrated plant in efficient steel production, it could be argued, is a classic case of Marvin Frankel's "interrelatedness" putting the older industry at a disadvantage [3]. The authors' presentation leaves one unsure as to the importance which they attach to the older age structure of the U.S. industry as a cause, in itself, of increased steel imports. An estimation of the decisiveness of this cause would seem to be logically prior to the rest of their arguments.

[2] There was a chronic shortage of steel capacity outside the United States which ended only in the middle and late 'fifties.

[3] The authors state: "this is not to gainsay the fact that transaction prices occasionally deviate from quoted prices, that there is some off-list selling, and that there are some temporary (though short-lived) dips in the steel index. The trend of the steel price structure, however, is unmistakably and unidirectionally upward" [1, p. 628, n.]. Facing increased competition, one would expect U.S. steel firms to become more willing to employ off-list selling, quantity discounts, and other competitive devices not included in

tempt to prove that it was the vertically integrated structure of the steel industry which produced what they feel was lack of price response to foreign competition. The authors base their proof on the experience in the wire-rod market. They present three explanatory hypotheses of "the lack of price response" to wire-rod imports in ascending order of likelihood:

> 1. . . . the vertically integrated firms were simply inept.
> 2. . . . the squeeze—particulary the delicate but excruciating additional twist of the maintenance of rod prices between 1959 and 1962—was deliberate, and designed to rid the fabricating end of the industry of price competition by independents.
> 3. Given the tapered integration and dual distribution in the wire segment of the industry, the majors had to compete with their nonintegrated customers [in finished steel products made from wire rod]. As long as the independent fabricators continued to buy domestic rod—and most of them used a 50/50 mix of imported and domestic rods in order not to cut themselves off from domestic supply sources—the majors felt that price cutting in the products market would not get out of hand. On the other hand, if the majors had matched the lower prices of imported rods, this would have lowered costs for the nonintegrated fabricators and served as a carte blanche invitation to cut product prices further than they had already been cut [1, pp. 645-46].

These hypotheses are not altogether implausible explanations of the lack of a price response in wire rod, yet even in this limited context doubts arise. For instance, why did independent fabricators continue to buy 50 per cent of their wire-rod imports at a supposedly higher price from domestic producers "in order not to cut themselves off from domestic supply sources" [1, p. 645] when overcapacity in the world wire-rod sector is evidently a problem which will continue far into the future?

The problem with these hypotheses relating vertical integration to increased steel imports is that they are logically inapplicable to imported steel products in general. Wire rod is a wholly atypical product of which only 20 per cent of the output is sold by majors, the rest being retained for in-plant fabrication into finished steel products [1, p. 640]. Perhaps because of its vertically integrated market, its price rose substantially more quickly in the period considered than finished and semifinished steel products in general [1, p. 634]. These hypotheses are relevant only to product markets where the majors are suppliers to and competitors of independent fabricators, i.e., wire rod. In such a vertically integrated market, the majors must consider the impact of a price reduction of a product upon the markets for goods fabricated from that product by both the majors themselves and their competitor-customers. Among the great bulk of steel products which are imported, the majors did not have to worry about price competition in other markets from their customers because

the index over the period. If the increased use of these techniques was substantial, the effective price of most steel products, including wire rod, may well have declined slightly after 1959.

the majors did not retain any significant quantity for further fabrication Therefore, the vertical integration explanation simply lacks generality.

The authors envisage the steel industry as one where "if vertical integration were irrelevant to price behavior," the major firms would respond to the influx of low price imports by "meeting it frontally, in the form of price reductions" [1, p. 653]. Yet there are many reasons why the U.S. industry might not have met competition with price reductions aside from vertical integration. One motivation may have been a belief that it was not worth risking a full-scale price war in order to save a small part of the market. For instance, the industry was confronted with competition from Japan where there was "government pressure upon the industry to export at least enough to cover the cost of their substantial raw material imports . . ." [11, pp. 344-45]. In such a situation, it is likely that a price reduction by the domestic industry would bring a further price reduction by foreign competitors and eventually lead to a rapid downward price spiral.[4]

Another possible reason for not wishing to cut prices was the diversified nature of the U.S. steel industry. Most large U.S. producers make many finished products from steel ingots. One feature of the U.S. industry is that most firms have much more finishing capacity than ingot capacity. As the steel industry was faced with foreign competition in only some of its finished products, it may have been more rational for it to continue to sell as much of the product facing foreign competition as possible near the usual price and concentrate on the more profitable items. These and many other possible reasons for the lack of price response are not discussed by the authors.

The authors' fifth cause is "the installation of ultramodern capacity, reflecting the latest technology outside the United States" [1, p. 626]. By "ultramodern" the authors mean the utilization of such new processes as the oxygen converter and continuous casting. Adams and Dirlam attempt to attribute supposedly disadvantageous changes in relative production costs to the allegedly slower adoption of these technical innovations in the United States. Let us see what impact the relative speed of adoption of these new techniques had on domestic and foreign prices. The authors quote *Business Week* as an authoritative source: "During the 1950's . . . 'the industry bought 40 million tons of the wrong kind of capacity—the open hearth furnace,' while the Europeans and Japanese were installing the cheaper and more productive oxygen converters at a breakneck pace" [1, p. 627]. In reality, the oxygen converter was first put into production in Austria in the early 1950's, but in a form suited for that country's particular requirements and not the requirements of the U.S. [2, p. 167]. The oxygen converter was later modified and did make headway in world steel production but hardly at a breakneck pace. In the European Iron and Steel Community, only $210,000 was spent on oxygen converters in 1955 and this rose to $33 million only in 1960 [6, p. 65]. Even in 1960, the European Community steel industry was spending $50 million on that "wrong kind of capacity," the open hearth, 50 per cent more than on oxygen converters. Japan was installing the oxygen converter at only a slightly

[4] For instance, one major Belgium firm was forced to reduce its wire-rod price from about $112 per ton in 1960 to $75 per ton in September, 1963 [11, p. 354].

faster pace than Europe and then mainly after 1958 [10]. The authors argue that "despite these apparent advantages of the oxygen techniques and despite its widespread use in Europe more than ten years ago, the U.S. steel industry was slow to adopt it" [1, p. 647]. Yet, even by 1960, only 2.6 per cent of the European Iron and Steel Community's installed capacity was in oxygen converters and only by 1963 had it reached 10 per cent [7, p. 36]. Yet, by then the United States capacity in oxygen converters was the same as the Community's at 10 per cent of capacity [7, p. 36] [5. p. 2]. Adams and Dirlam are misled in their view that oxygen conversion was adopted sufficiently more quickly by competing foreign steel firms so as to give them a significant price advantage.

The authors believe that the adoption of "continuous casting" represents another U.S. failure. They are credulous enough to use a continuous-casting-equipment manufacturer as a source of estimates of the "remarkable cost savings for continuous casting" [1, p. 647]. It is true that continous casting has great cost-savings potential, but even an equipment manufacturer would not deny the great technical difficulties involved and the many major modifications since the original design [9].[5] Putting their faith in *Iron Age,* the authors note that continuous casting " 'began moving in high gear abroad about eight years ago' " [1, p. 647]. However, High Authority publications consider continuous-casting investment expenditures negligible until 1962 and then they totalled only $2.3 million [6, p. 23]. Yet, the authors state that in the United States "the first commercial continuous-casting unit went into operation in 1962" [1, p. 647].[6] Japanese expenditure on continuous casting was only slightly higher than the Community's while England's was even lower [8, pp. 43-44].

The reliable technical literature reveals that the United States was not significantly slower than Europe and Japan in adopting this new equipment and in no case was the percentage of capacity in these new techniques high enough in the period under consideration to have an appreciable impact on relative costs.

It may well be that the increase in steel imports to the United States was the fault of the U.S. steel industry and it is possible that vertical integration was a contributory factor. Yet Adams and Dirlam have done little to prove their point.

G. A. Hone and D. S. Schoenbrod*

* The authors were graduate students of Nuffield College, Oxford, when this comment was written. Mr. Schoenbrod is now a student at the Yale Law School. They wish to thank Professor Lance Davis, Professor William Gorman, Sir Roy Harrod, Paul Rock, Robert Skidelsky, and, above all, Philip Andrews for valuable help. Any mistakes this comment may contain, however, are the responsibility of the authors.

[5] One outstanding documentation of the engineering complexities of continuous casting is K. P. Korotker, *et al., The Continuous Casting of Steel in Commercial Use,* Oxford (1960).

[6] There was at least one continuous-casting plant in operation in the U.S. in 1957.

References

1. Walter Adams and Joel Dirlam, "Steel Imports and Vertical Oligopoly Power," *Am. Econ. Rev.,* Sept. 1964, *54,* 626-55.

2. D. J. C. BRANDT, *The Manufacture of Iron and Steel*. London 1960.
3. MARVIN FRANKEL, "Obsolescence and Technical Change," *Am. Econ. Rev.*, June 1955, *45*, 296-319.
4. D. F. GORDON, "Obsolescence and Technical Change: Comment," *Am. Econ. Rev.*, Sept. 1956, *46*, 645-52; reply by Marvin Frankel, pp. 652-56.
5. E.C.S.C., *Generaldirektion Stahl Dokument* 5935/64 d + J. Luxembourg 1964.
6. ———, *Les investissements dans les industries du charbon et de l'acier de la Communaute*. Luxembourg 1964.
7. ———, *Objectifs generaux "Acier 2."* Luxembourg 1964.
8. IRON AND STEEL BOARD, *Development in the Iron and Steel Industry* (Special Report, 1964). London 1964.
9. IRON AND STEEL INSTITUTE, *Bibliography of Continuous Casting*. London 1964.
10. ———, *Special Report 85*. London 1964.
11. POLITICAL AND ECONOMIC PLANNING, *Steel Pricing Policies*, Vol. 30, pub. 484. London 1964.

Steel Imports and Vertical Oligopoly Power: Reply

The riposte from Pittsburgh was predictable. Not so the attack from the birthplace of full-cost pricing. Avoiding repetition, we shall deal with each separately.

I

Professor Slesinger offers four principal contentions to justify the laggard innovation rate of the U.S. steel industry with respect to the oxygen converter and continuous casting.

1. He claims that only 16.25 million tons of "new" open-hearth capacity were constructed between 1950 and 1960. This is based on unpublished (and hence not readily verifiable) data of the American Iron & Steel Institute. In response to our own inquiries, the Institute placed the *net* increase in steel capacity (of all varieties) at 49 million tons during the period,[1] while most of the major steel companies refused to disclose the extent, nature, or cost of their capacity "revisions" with any degree of particularity. A sample comparison of the 1951 and 1960 AISI Directories, however, reveals that during this period U.S. Steel added a *net* of 9 new open-hearth furnaces at Fairless, 2 at Fairfield, and 1 at Geneva; Bethlehem, 7 at Sparrows Point, 5 at Lackawana, and 1 at Steelton; Republic, 6 at Cleveland; Jones & Laughlin, 6 at Pittsburgh and 2 at Cleveland; Inland, 7 at Indiana Harbor; National, 2 at Weirton; Armco, 3 at Middletown; Youngstown, 8 at Indiana Harbor; Colorado Fuel, 1 at Pueblo; and Kaiser, 2 at Fontana. These *net* additions *alone* were accompanied by an increase of 21.96 million tons of capacity. That they occurred during a period when the total number of open-hearth furnaces for the

[1] Letter to authors, June 4, 1964. Since early 1964, the AISI Library in New York bars access to the general public, including professors of economics.

industry actually decreased (from 947 to 906), and in the face of admittedly numerous abandonments, indicates that the *gross* installation of such furnaces far exceeded the AISI estimate reported by Slesinger.[2]

Aside from the fact that capacity "revisions" are not costless,[3] this numbers game is quite irrelevant. Whether the construction of *new* open hearths amounted to 16.25 million tons (Slesinger) rather than 40 million (*Business Week*),[4] 49 million (Forbes),[5] or 48.6 million (Department of Commerce),[6] the central question remains, as we shall show *infra*, why the U.S. steel giants constructed a single ton of new, or "revised" a single ton of old, open-hearth capacity, rather than scrapping their entire investment in an obsolescent, noncompetitive process.

2. Slesinger argues that the failure of the U.S. steel giants to install oxygen converters between 1950 and 1960 reflected a preference for "large" open hearths over "small" converters and a desire to await the upscaling of an embryonic innovation. This unsubstantiated *ex post* rationalization of steel industry behavior is contradicted by persuasive circumstantial evidence.

First, the alleged dichotomy between "large" open hearths and "small" converters is purely mythological. In 1954, the U.S. Steel Corporation had 263 basic open-hearth furnaces, and only 18 of these (6.84 per cent) had an annual capacity greater than that of the "small" (35-ton heat) Austrian converters [3]. In 1957, some 88 per cent of U.S. Steel's basic open-hearth furnaces still had a smaller annual capacity than the "small" 1953-vintage Austrian converters [4]. Also in 1957, when Jones & Laughlin began operating its 85-ton oxygen vessel (with an annual capacity of 475,000 tons), not even the largest open hearths of the largest steel companies could match its capacity. (See Tables 1 and 2.) Indeed, while Slesinger talks about "large" 300- to 500-ton open hearths, the number of such furnaces—as late as 1960—accounted for only 5 to 10 per cent of the industry's total [5].

Second, Slesinger seems to confuse the influence of *firm* size with the technological imperatives flowing from *plant* size. The optimum-sized oxygen con-

[2] The degree of understatement in our sample is illustrated by the case of Detroit Steel, a small producer. Between 1951 and 1960, according to the AISI Directories, the number of open hearths at the company's Portsmouth plant *decreased* from ten to five, while annual open-hearth capacity *increased* from 650,000 to 1,000,000 tons. This capacity increase was achieved by scrapping ten old 100-ton furnaces and replacing them with four 250-ton and one 275-ton furnaces during the period. Thus a 1,000,000 ton *gross* addition of *new* capacity was "concealed" by a *net* capacity increase of only 350,000 tons and an actual *decline* in the *number* of open hearths operated at the Portsmouth plant. (Cases of this sort would not, of course, show up in our sample.)

[3] Thus, one company modernized (i.e., "revised") its open-hearth capacity during the 1950's at a cost of $19 per ton of added annual capacity. The same company during the early 1960's installed *new* oxygen converters at a cost of only $13 per annual ton. Letter to authors, May 19, 1964.

[4] Nov. 16, 1963, pp. 144-46.

[5] Jan. 1, 1963, p. 31.

[6] U.S. Congress, Joint Economic Committee, *Steel Prices, Unit Costs, Profits, and Foreign Competition*, Hearings, 88th Cong., 1st sess., Washington 1963, p. 186. This figure includes increases not only of open-hearth capacity, but other types of ingot capacity as well, and is comparable to the AISI figure cited in note 1.

TABLE 1—CAPACITY OF LARGEST STATIONARY OPEN-HEARTH FURNACES OF THE SIX LARGEST U.S. STEEL COMPANIES, JANUARY 1, 1957

Company	Plant	Rated Capacity per Heat (N.T.)	Number of Furnaces in Category	Annual Capacity per Furnace (N.T.)
U.S. Steel	Fairless	290*	9	244,444
Bethlehem	Sparrows Pt.	380	5	261,600
Republic	Cleveland	375	4	250,000
J & L	Pittsburgh	275	11	209,545
Youngstown	Indiana Harbor	307	8	222,000
National	Weirton	600	1	303,960

* The largest *tilting* open hearths of U.S. Steel (at McKeesport) have a rated capacity of 191.8 N.T. per heat and an annual capacity of 442,000 N.T. per furnace.
Source: [4, pp. 57, 170, 215, 263, 322, 353, 394].

TABLE 2—CAPACITY OF LD OXYGEN CONVERTERS OF OESTERREICHISCHE ALPINE MONTANGESELLSCHAFT (O.A.M.). 1953, AND JONES & LAUGHLIN, 1957

Company	Plant	Rated Capacity per Heat (N.T.)	Number of Furnaces in Category	Annual Capacity per Furnace (N.T.)
O.A.M.	Donawitz	35	2	220,000
J & L	Aliquippa	85	2	475,000

Source: [7, p. 3].

verter, even in a giant firm, is related to the complementary facilities of the plant in which it is installed, and there is a wide range of size feasibility—35 to 300 tons per heat—for efficient converter operation.[7] Thus, the nation's largest steel firm (U.S. Steel) installed a 150-ton converter in 1964, whereas the fifth-largest producer (National) had been operating a 300-ton converter since 1961. Firms of such disparate size as Inland and Wheeling both use ca. 225-ton converters, and U.S. Steel and Pittsburgh both use 150-ton vessels. Certainly, if a "large" firm like Jones & Laughlin found it profitable to use a "small" 85-ton oxygen converter in 1957, can it reasonably be argued that other steel giants—i.e., U.S. Steel and Bethlehem—had to wait an additional seven years before making a similar cost-saving move?

The "scale" argument is simply no defense for innovative lethargy. As early as 1954, 10 "small" 35-ton Austrian converters, if installed at U.S. Steel's giant Fairless Works, could have matched the capacity (2,200,000 tons) of the plant's 9 "large" 290-ton open hearths. Even if this was thought inadequate, however, the converter's adaptation to the "large-scale" needs of the

[7] It is noteworthy that the 30-ton-per-heat Bessemer converters operated by U.S. Steel at its Lorain Works in 1960 each had an annual capacity of 300,000 net tons, i.e., a larger capacity than most of the Corporation's open hearths [5, p. 355]. In 1964, U.S. Steel still found it profitable to operate *bottom-blown* Bessemer converters no larger than 35 tons per heat [6, pp. 269-308]. How, then, can it be argued that the Corporation would have been unwise to install *top-blown* converters of the same "small size ten years earlier,

U.S. industry would hardly have taken 10 years—had the steel executives invested less in their public relations effort to denigrate this technological breakthrough and more in their research, development, and innovative imitation.[8]

3. Both Slesinger and the Oxford Group defend the U.S. steel industry against our charge of technological backwardness, asserting that European and Japanese producers did not adopt the oxygen converter and continuous-casting innovations at a significantly more rapid rate.

While it was somewhat extravagant of us (and the trade press) to characterize the foreign installation rate of the new processes as proceeding at a "breakneck pace," we were quite correct in concluding that "both continuous casting and the oxygen process began to be introduced in U.S. steel mills at a significant rate only after the import threat became serious. They were adopted by major mills in the United States at least five years after European mills had recognized them as major cost-saving techniques" [1, p. 647].

The comparative installation rate of the oxygen converter in the United States and abroad speaks for itself. By September 1963, the United States still had only 10.04 million tons of LD capacity in place—compared to 46.21 million tons for the world as a whole [7, pp. 3, 6]. If more than 2.5 million tons of other types of oxygen capacity (Kaldo process and rotary converter) were added to the world total, the U.S. share would have been even smaller. Moreover, the lag of the United States behind foreign steel producers was all the more remarkable, because the LD process developed by the Austrians was *immediately* applicable to conversion of the low-phosphorus ores found in North America. Germany, England, France, Sweden, Belgium, and Luxembourg, by contrast, had to wait until 1957 before the LD process was modified sufficiently (by the addition of lime powders in the LD-AC, OLP and LD-Pompey processes) to be suitable for processing the high-phosphorus ores used in their steel industries. Once this adaptation was made, these countries began to install the latest technology. So did Japan.

The case of Japan puts the issue in focus. By the end of 1960, the Japanese steel industry—which was *one-fifth* the size of its U.S. counterpart and *smaller* than the U.S. Steel Corporation alone—had installed some 5.45 million net tons of oxygen converter capacity, compared to 4.84 million for the United States and *zero* for its largest steel producer [7, pp. 3, 5]. Moreover, in the

[8] As late as November 1957, U.S. Steel informed the Kefauver Committee that "further development" was necessary before new processes like oxygen steelmaking "conceivably could be substituted for, or displace existing practices." Their "growth potential," the Corporation felt, "cannot be forecast [*sic*]." U.S. Senate, Antitrust and Monopoly Subcommittee, *Administered Prices: Steel, Hearings*, Part 3, 85th Cong., 1st sess., Washington 1958, pp. 1057, 1058. In the same month, however, Jones & Laughlin had already begun operating its "small" 85-ton oxygen converter—with unambiguous success. According to Avery C. Adams, its president and board chairman, "the basic oxygen process represents the only major technological breakthrough at the ingot level in the steel industry since before the turn of the century. With the exception of what we in the industry call trick heats, i.e., one heat made under ideal conditions, the best open hearth practice today results in a production rate of 39 to 40 tons per hour. Our basic oxygen furnaces have produced at the rate of 106 tons per hour to date this month. On a trick heat basis, we have hit 160 tons per hour." Letter to the Stockholders, April 1959.

same year, some 17 per cent of Japanese ingot capacity was in oxygen converters (as compared to roughly 3 per cent in the U.S.) and only 54 per cent in open hearths (as compared to 85 per cent in the United States).[9] One scarcely need editorialize on these data.

With respect to continuous casting, nowhere are our critics able to rebut the *Iron Age* report that "close to 40 machines were installed [abroad] in the period from 1955 through 1962"; that the U.S. steel industry did not take a "second look" at the process until 1961; and that, except for a pilot plant at U.S. Steel's Chicago Works, the first commercial machine went into operation in the United States in 1962—at the plant of diminutive Roanoke Electric Steel Corporation.[10] As late as September 1965, *Fortune* still reported that "continuous casting of steel was pioneered in Europe but long resisted by U.S. steel makers. *Now* the resistance is ending."[11] Similarly, in August 1965, the Cleveland Federal Reserve Bank stated that "the steel industry in this country has begun to invest in the continuous casting process but it still lags behind Europe where the process has had *widespread* use for *several* years."[12] To suggest that the innovation had to be upscaled before it could be used by the U.S. steel giants is, for reasons cited *supra*, another example of the plant-versus-firm-size fallacy.

The most significant question, of course, is not *how* much the U.S. steel giants lagged technologically but *why*, despite their immense superiority in financial and technical resources, they lagged *at all*—either in invention or innovation, and against far smaller rivals both here and abroad.[13]

[9] [5, p. 449], Japan Iron & Steel Federation, *Statistical Yearbook for 1961*, p. 25, and Jean Bienfait, *La Sidérurgie Japonaise*, Lausanne: Centre de Recherches Européenne, 1965, p. 89. Incidentally, the Oxford Group's own reference on Japan [10] reinforces our findings: "One of the most impressive features of the Japanese iron and steel industry is the *rapid application* of the LD process and its development to an exceptionally high standard of operating efficiency" [p. 55]. Oxygen converters, according to the report, accounted for only 3.4 per cent of Japanese steel production (as distinct from capacity) in 1957, but for 11.5 per cent in 1960 and 30.7 per cent in 1962 [p. 56]. Comparing the switch from open hearths to converters in Japan and the United Kingdom, the report concludes that "the *pace* of development in Japan is very much more rapid and has taken place much *earlier*" [p. 56]. (Emphasis added throughout.)

[10] G. J. McManus and R. Shah, "Continuous Casting," *The Iron Age*, Sept. 12, 1963, pp. 159-66.

[11] "The Latest Big Wrinkle in Steel," *Fortune*, Sept. 1965, p. 160. (Emphasis added.)

[12] "Plant and Equipment Expenditures," *Economic Review*, Aug. 1965, p. 7. (Emphasis added.) For a brief history of invention and innovation of continuous casting, see Jewkes, Sawers, and Stillerman, *The Sources of Invention*, London 1958, pp. 276–80. Note also that the Korotkov study cited by the Oxford Group dealt only with the first year's operation (1955-56) of the continuous-casting plant at the Krasnoye Sormovo Works in the U.S.S.R. On the basis of that (favorable) experience Korotkov predicted that by 1960 "this method of steel casting will augment the output of rolled products [in the U.S.S.R.] by one million tons" and that "two milliard roubles should be saved by using this process" (p. ix).

[13] The position of the U.S. industry is reminiscent of the lady, interviewed on TV shortly after the Russians launched their sputnik. Asked for her evaluation of the U.S. role in intercontinental and extraplanetary rocketry, she said that "the United States has always been, and always will be, first in everything." When reminded that the

4. All of Slesinger's contentions ultimately rest on his central argument that "so long as steel produced by conventional methods [could] compete with that produced by newer technology, it would not [have been] a wise economic decision to scrap the existing facilities and to destroy vast investments in fixed capital."

Given direct operating savings of about $5 per ton of oxygen converter over the open hearth; given the investment cost of roughly $15 per ton of converter capacity (as compared to about $35-40 per ton of open-hearth capacity), the wisdom of retaining open-hearth facilities should have been questioned in the early 1950's.[14] The precepts of sound replacement economics[15] should have dictated the use of oxygen converters not only for expansion purposes, but also for the wholesale replacement of existing (and presumably serviceable) open-hearth capacity.

But Big Steel chose to ignore these precepts. During the 1950's, it was apparently prepared to live with obsolescent capacity and indulge in its "rational" oligopolistic dislike of any innovation that introduces uncertainty into the pricing process. It was content to let McLouth and Kaiser innovate a revolutionary technology as long as there was no danger that this might precipitate an outbreak of price competition. Relying on its insulation from domestic and foreign competition, Big Steel decided to wait until its technically obsolescent facilities were fully depreciated. Unlike firms in a competitive industry, it was not compelled to regard investment, once made, as truly sunk cost and to take into account only the operating expenses of existing equipment. Instead, it afforded itself the luxury of protecting "vast investments in fixed capital."

Ironically, this protectionism which is the key to the industry's investment decisions during the 1950's and which Slesinger still defends, is currently being subjected to revision, if not reversal, by the industry itself. Its comprehensive modernization program was launched in a period of substantial "unused" (i.e., economically "unusable") capacity. Thus, in 1964 (a banner year for steel), the industry was installing oxygen converters at a brisk pace while using only 75 per cent of existing facilities. Obviously, as the *Wall Street Journal* observed, the increase in ingot capacity came "because of mill efforts to lower costs and not from any lack of raw steel." Indeed, the industry was reportedly mothballing some 7 million tons of open-hearth capacity, reclassifying it as "standby capacity" with the intention of dismantling much of it "be-

Russian satellite and had just beaten us into space, she exclaimed, "Give us time; give us time." In this connection, see our forthcoming article, "Big Steel, Invention, and Innovation," *Quart. Jour. Econ.*, 1966.

[14] *Steel*, Apr. 27, 1959, p. 61; and *The Iron Age*, Dec. 12, 1957, p. 87, and Sept. 24, 1959 pp. 67-68. As early as 1952, a leading trade journal devoted almost an entire issue to a steel conference at Leoben, Austria, where some 360 engineers and scientists (60 of them from 7 countries outside of Austria) met to discuss the oxygen revolution in steelmaking and to receive first-hand reports on the experience of the Linz-Donawitz pioneers. These reports dealt with the metallurgical characteristics of oxygen steel, the engineering aspects of operating an LD converter, the economic feasibility of the new process, and a rather detailed statement on operating and investment costs. See *Stahl und Eisen*, Aug. 14, 1952, 72, 989-1024, but esp. 997-1004.

[15] Cf. M. A. Copeland, *Our Free Enterprise Economy*, pp. 181-209.

fore long."[16] According to the *Iron and Steel Engineer,* much of the projected new oxygen capacity will "be used to replace existing workable capacity. Companies are being forced to the process in order to compete. . . . *The low capital cost and the savings in operating costs more than overbalance any considerations to continue operating existing equipment.*"[17] In brief, even the major steel companies now seem to reject the rationalizations of the 1950's as a guide to sound investment decisions in the 1960's.

II

Since there is no main thrust to the diverse criticisms by the Oxford Group, we shall answer them in the order in which they are raised.

1. We are criticized for not assessing the "impact of exogenous causes" on the level of steel imports. But these "exogenous causes" were all reflected in the very existence of an import problem, and it was our purpose to explain the U.S. steel industry's insensitive price behavior in responding to that problem.

2. We are chastised for our lack of concern with the comparative age structure of U.S. and foreign steel plants. Again, this criticism is hardly relevant. Our article dealt with the apparent indifference of the U.S. steel industry to a massive erosion of its home market, and the prevalence of an import-induced two-price system. Indeed, to the extent that the age structure of U.S. steel plants was a significant variable, it should have militated in favor of a prompter counterattack against the import beachheads. Instead, the industry's addiction to target-rate-of-return pricing and cost-plus protectionism of existing investment contributed to its lack of response to steel imports from newer, more efficient foreign plants [1, pp. 652-53, esp. note 19, and our comments on sunk costs, *supra.*].

3. We are challenged on our definition of dumping. Under the Anti-Dumping Act of 1921—the context in which we used the term—an "arithmetic" violation occurs whenever the f.o.b. price in the exporter's home market is higher than his export price (minus transportation costs, customs duties, etc.). The Japanese did not run afoul of the statute, because they met this "arithmetic" requirement. They did not absorb freight between Yokohama and Pittsburgh. They practiced neither dumping nor charity—in a legal or economic sense—but they *were* guilty of waging competition against an exploitative oligopoly.

4. Our critics fail to understand why independent fabricators continued to buy 50 per cent of their wire rod from domestic producers at prices higher than those charged by importers. The reason (given by the fabricators) is simple, viz. "in order not to cut themselves off from domestic supply sources" [1, p. 645]. And this is not as irrational as the Nuffield College economists imply. Maintaining good will is important in an industry that suffers from periodic shortages of domestic supply and where such political vagaries as tariff manipulation make a total reliance on foreign sources risky, if not foolhardy.

5. The Oxford Group questions the value of our analysis because it rests on

[16] Jan. 4, 1965, p. 4.

[17] Jan. 1963, p. 171. (Emphasis supplied.)

"a wholly atypical product" like wire rod. This statement borders on the preposterous. Between 1957 and 1962, wire rod was one of the industry's largest tonnage imports, and increased faster than the imports of any other steel product. It was the keystone for an entire segment (wire and wire products) of the steel industry. It captured a sizable share of the noncaptive U.S. market —17.1 per cent as early as 1958, 39.2 per cent in 1962, and more than 40 per cent since 1963 [1, p. 631]. Finally, the PEP pamphlet on *Steel Pricing Policies*, which the Oxford Group cites with approval, itself singles out wire rod to illustrate typical price fluctuations in world markets.[18]

6. Our vertical integration hypothesis is said to lack generality because "among the great bulk of steel products which are imported, the majors did not have to worry about price competition in other markets from their customers." Aside from the lack of substantiation for this claim, our critics simply reiterate and support—while seeming to refute—our explanation for price behavior in the "wire rod-wire-wire products" segment of the steel industry. Yet they insist, with inexplicable fervor, that our vertical integration hypothesis does not apply to those segments of the industry where there is no vertical integration or dual distribution. They seem unwilling to take cognizance of the subject matter dealt with in our article, and rather accurately described in its title, "Steel Imports and *Vertical* Oligopoly Power." (Emphasis added.)

7. Our critics suggest that "there are many reasons why the U.S. industry might not have met [import] competition with price reductions aside from vertical integration." But here they seem to ignore the differences between rational behavior in a horizontal and vertical context. If the steel industry were organized in the form of successive horizontal oligopolies, the import phenomenon would clearly have had a different impact. At each stage, the horizontal oligopolists would have greeted substantial imports in the same manner as they would the appearance of a nonconformist price cutter [1, p. 653]. They would have been compelled to regard the market for each product as a separate (i.e., the only relevant) market in order to make rational pricing decisions.

8. Our critics tell us that the U.S. steel industry is a multi-product industry, and hence enjoys production flexibility—i.e., the possibility of shifting from "unprofitable" items facing import competition to "profitable" items, presumably insulated from such competitive threats. But they offer no evidence whatsoever that the steel industry during this period availed itself of this production flexibility, and that it in fact compensated for the loss of import markets by shifting to more profitable items. Throughout this period, there was substantial and persistent overcapacity in all segments of the industry, and especially in basic steel.

9. We are said to be misled in our "view that oxygen conversion was adopted sufficiently more quickly by competing foreign steel firms to give them a significant cost advantage." Nowhere did we make such a statement, nor was our vertical integration hypothesis dependent on such a cost-anchored comparison. Nevertheless, the technological backwardness of the U.S. steel in-

[18] P. 345. See also Council of Economic Advisers, *Report to the President on Steel Prices*, Washington, April 1965, pp. 14-15.

dustry could not be ignored in view of the industry's addiction to target-rate-of-return pricing, its refusal to regard capital costs as sunk costs, and its insistence on recouping investment—come what may, and *coute que coute.* In this context, technological backwardness meant that the industry was imprisoned by its higher book costs associated with past investment, and hence unwilling to meet the import threat with aggressive price competition. It thus reinforced the insensitive price behavior that was an outgrowth of vertical integration.[19]

III

After careful evaluation of the criticisms from Pittsburgh and Oxford, we are still not persuaded that the U.S. steel industry is technologically progressive, or that vertical integration *cum* dual distribution is an unimportant determinant of steel pricing policy.

WALTER ADAMS AND JOEL B. DIRLAM*

[19] The cost implications of technological backwardness are also noteworthy, of course, in view of the traditional assumption that *superior* technology enabled U.S. steel firms—despite the handicap of higher wages—to compete successfully in world markets. Perhaps, as one cynic remarked, the failure of the industry to be *au courant* of the latest technology is attributable to the consistently moderate demands of the United Steel Workers of America.

* The authors are professors of economics at Michigan State University and the University of Rhode Island, respectively.

REFERENCES

1. W. ADAMS AND J. B. DIRLAM, "Steel Imports and Vertical Oligopoly Power," *Am. Econ. Rev.*, Sept. 1964, *54*, 626-55.
2. AMERICAN IRON AND STEEL INSTITUTE, *Directory of Iron and Steel Works of the United States and Canada,* 1951.
3. ———, ———, 1954.
4. ———, ———, 1957.
5. ———, ———, 1960.
6. ———, ———, 1964.
7. KAISER ENGINEERS, L-D Process Newsletter No. 21, Sept. 27, 1963.

Manufacturing Investment, Excess Capacity, and the Rate of Growth of Output: Comment

In a recent article in this *Review* [1, p. 607] Professor Alice Bourneuf states, "This paper tests the accelerator in a period during most of which there was little or no pressure on capacity." And also that "accelerator-type forces may be more important in periods of pressure on capacity." The purpose of this comment is to attempt to clarify the extent to which these propositions have in fact been tested.

Consider the relationships between capacity (C_t), desired output ($\overline{Y}_t$), and desired capital stock ($\overline{K}_t$), where capacity is defined in an accounting

sense and is the maximum output which could be produced given the capital stock. Typically desired output is less than capacity:

$$\overline{Y}_t = rC_t \quad \text{where} \quad 0 < r < 1.$$

Then we may define (1) the capital-output ratio or conventional accelerator as $\sigma = \overline{K}_t/\overline{Y}_t$, and (2) a capital-capacity ratio as $\sigma_c = K_t/C_t$. It follows that $r\sigma = \sigma_c$ so that in general the conventional accelerator is larger than the capacity accelerator.

Now, given the form of Miss Bourneuf's investment equation [1, p. 609],

$$I_t = a(C_{t-1} - Y_{t-1}) + bC_{tt} + c\Delta Y_t + k \tag{1}$$

and considering only the first term, if we let $a = -\beta\sigma_c$, then substituting we get

$$\begin{aligned} a(C_{t-1} - Y_{t-1}) &= -\beta\sigma_c(C_{t-1} - Y_{t-1}) \\ &= -\beta K_{t-1} + \beta\sigma_c Y_{t-1} \\ &= \beta(\sigma_c Y_{t-1} - K_{t-1}), \end{aligned} \tag{2}$$

which is a more recognizable form of the acceleration principle with adjustments for capacity considered where β is the reaction coefficient. However, the accelerator is not the conventional capital-output ratio but rather the smaller σ_c.

Miss Bourneuf's coefficient a embodies both β and σ. Indeed, this is clear since after a is estimated in the study it is divided through by an assumed capital-output ratio (σ), so that the reaction coefficient β may be determined. Similarly, one might have estimated the accelerator by dividing a by an assumed β. The estimates of β, then, are reasonable only to the extent that the assumed accelerator is reasonable. The difficulty here is obvious. Separate estimation of β and σ is desirable, and their joint determination, as in this case, makes it difficult to infer the magnitude of either. In addition, dividing through by σ rather than σ_c will result in underestimating β. Finally, the need to divide through by a capital-output ratio removes much of the apparent empirical advantage of using capacity figures rather than capital stock estimates.

When measuring the accelerator using capacity data the distinction between capacity in the accounting sense (maximum output) and capacity in the economic sense (minimum cost) must be kept in mind. A logically consistent accelerator formulation using accounting capacity as a variable is either

$$I_t = \beta(\sigma r C_{t-1} - K_{t-1})$$

or

$$I_t = \beta\sigma(rC_{t-1} - Y_{t-2}) + (1 - \beta)I_{t-1}.$$

These are equivalent [2, p. 187], and the second form which is based on a distributed lag assumption would enable simultaneous estimation of β and σ.

The second term of equation (1) is straightforward. If ρ is the rate of depreciation, then $b = \rho\sigma_c$ and the true depreciation rate is determined by

dividing b by σ_c. Since b was actually divided by σ, ρ may be underestimated.

The coefficient c in the third term is more difficult to interpret. Alone it would be a simple accelerator coefficient. However, since the accelerator is part of coefficient a in the first term, this cannot be the case. It is not, then, surprising that the value of c is so small. (It is not significant at the .05 level in six of the 13 equations, and in two of these it has the wrong sign.) One might suspect that ΔY_t was added rather as an afterthought to soak up residual variance. In any case, a reasonable theoretical interpretation is not obvious. Empirically, one can say that, other variables having been considered, a $1 increase in Y leads to $.15 more I. However, when the coefficient is divided by σ, the very limited interpretation becomes quite apparent, e.g., if the capital stock is increased by $1, investment rises by $.09. Obviously the coefficient does not mean what it says. If c were an accelerator coefficient, then dividing it by σ would yield a value of approximately 1. In fact it is rather a kind of residual accelerator picking up the variation after the first term $(C_{t-1}-Y_{t-1})$ has been considered. The statement in the text, "manufacturers plan to expand capacity up to only 9 per cent of the addition to output" [1, p. 621], is certainly misleading if not incorrect. Miss Bourneuf goes on to state:

> Direct effects of output changes are small, but it must be remembered that additions to output decrease excess capacity, and therefore increase investment, and that higher investment increases total capacity, and therefore in turn leads to more replacement investment, but also more excess capacity [1, p. 621].

This quotation highlights the difficulty of interpreting the coefficients. The accelerator, while only part of coefficient a, is more than coefficient c. The presence of c may actually bias a downwards since at least some of the accelerator impact is contained in c. If this is the case, then the estimates of β, the reaction coefficient, may similarly be biased downwards. Thus it does not seem that there has been a test of the acceleration theory of investment or that there is evidence presented on the varying importance of the accelerator depending on capacity. To show that excess capacity is an important determinant of investment is not necessarily to test the acceleration principle.

David B. Houston*

* The author is associate professor of economics at Pennsylvania State University. He is grateful to the referee for his perceptive and helpful comments.

References

1. Alice Bourneuf, "Manufacturing Investment, Excess Capacity, and the Rate of Growth of Output," *Am. Econ. Rev.*, Sept. 1964, *54*, 607–25.
2. David Smyth, "Empirical Evidence of the Acceleration Principle," *Rev. Econ. Stud.*, June 1964, *31*, 185–202.

Manufacturing Investment, Excess Capacity, and the Rate of Growth of Output: Reply

Professor Houston raises a basic question on the reasonableness of my equations and also some relatively minor technical points.

I agree that my equations test a capital-capacity accelerator rather than a capital-output accelerator; since induced investment is for the purpose of adjusting capacity, this seems to me to be a reasonable accelerator concept.

Houston also suggests that in strict logic the adjustment of the coefficients of the capacity terms in my equation to a capital stock basis should have been by a capital-capacity ratio rather than a capital-output ratio, since "desired output" is smaller than capacity. He defines capacity "in an accounting sense . . . as the maximum output which could be produced given the capital stock." The capacity indexes I used probably reflect maximum output under normal operating standards, usually referred to as an engineering concept of capacity. Houston's "desired output" is determined by cost factors; this concept may be roughly reflected in the preferred operating rates reported at times to McGraw-Hill, which have ranged from 90 to 98 per cent of capacity. The capital-output ratios I used were based on data for years of very high operating rates in many industries, and the use of capital-capacity estimates would have raised the coefficients, as adjusted, at most by roughly 10 per cent. This might have been a preferable procedure. Using estimates of capital-output ratios to adjust the coefficients of both the output and capacity terms was a very rough procedure, perhaps not too bad in view of the uncertainties on the absolute levels of the capital stock and capacity estimates.

Houston's basic criticism is that my equations do not fit conventional forms of the acceleration principle; in particular a capacity form, which is equivalent to a distributed-lag form, assuming expected output to be some constant times output in *t-n*, or a weighted average of past periods' outputs, a lag of some sort in the investment process of *n* periods, and geometrically declining weight for past changes in output in the distributed lag form. I agree that my equations are not consistent with these precise models, but I believe they are reasonable.

My equations assume that this year's change in output affects the expected future level of output, and, therefore, investment, and that the lagged excess capacity term exerts an independent dampening influence on investment.[1] Past changes in output affect investment through their effects on the excess capacity term; they clearly affect last year's total output, and they also affect last year's total capacity via their effects on investment in previous years. They also affect investment through the capacity term designed to catch replacement.

My investment and capacity equations together can be solved to make investment a function of all past changes in output, weighted in some complex

[1] The current period change in output term seems to me to be much more important in an annual model than it would be in a quarterly model. I also suspect that certain simple distributed-lag schemes are more suitable for explaining quarterly investment because they can be interpreted as reflecting gestation-period effects.

fashion. It is this fact that I was trying to express in the sentence Houston quotes as misleading or mistaken. The iteration examples I worked out actually trace this process over time. Together my equations can be made equivalent to some sort of distributed-lag principle. But the behavior of investment is not believed to be explained by a series of lagged reactions to changes in expected output based on past changes in output.

Since the excess capacity term in my equation does not by itself represent the difference between desired and actual capacity, Houston's suggestion that the speed of reaction coefficient be estimated from the data on the basis of a simple distributed-lag function, with weights of past output changes declining geometrically, cannot be accepted.

My equations imply that the effects of the excess capacity and current change in output terms are additive. The c coefficient of the change in output term probably is related functionally in some fashion to the a coefficient of the excess capacity term; the results for various industries suggest this is the case. The precise nature of this relationship is not clear, however, and separate estimation of the coefficients seems to be a legitimate procedure.

Houston does not think I have tested the acceleration principle. Perhaps this is a matter of definition. I think of the acceleration principle as stating that investment is positively related to output and negatively related to capital stock or capacity. My equations are a test of the acceleration principle in this sense.

ALICE BOURNEUF*

* The author is professor of economics at Boston College.

A Note on Progression and Leisure: Comment

In a recent communication, Robin Barlow and Gordon Sparks [1] attempt to overthrow one of the oldest and most respectable conclusions in public finance theory, namely that an individual will choose to work fewer hours if a proportional income tax is replaced by a progressive income tax of equal yield.[1] On the basis of a brief reappraisal of the familiar geometrical analysis made by Kenneth Boulding [2, pp. 773-75] and set out in rather more detail in Richard Musgrave's treatise [4, pp. 241-43], the authors suggest that there is nothing in the conventional diminishing marginal utility assumptions which logically excludes the possibility that a mildly progressive tax will induce a greater supply of effort than an equal-revenue proportional tax. The conditions under which this "neglected outcome" is possible are then deduced by means of a mathematical analysis of the orthodox model. As a result of this

[1] The authors concede that if "equal yield" is defined in the way suggested by Otto von Mering [5, pp. 454-55], the traditional conclusion holds without modification. Under this definition a progressive tax is said to be of equal yield if, *with the individual working the same number of hours as under the proportional tax*, the revenue raised would be the same. Barlow and Sparks refer to this as an *ex ante* definition of equal yield. Why the government should be so naïve or eccentric in its conception of equal yield is, however, unclear. It seems better to recognize that the comparison suggested by von Mering is for most purposes arbitrary and irrelevant.

analysis they conclude that the traditional result requires qualification to the extent that *"when the marginal utilities decline slowly, a mildly progressive tax with rates substantially lower than that of the proportional tax may produce an equal yield as a result of inducing more work effort"* [1, p. 377]. Although they do not mention it, their analysis would also overthrow the closely related and equally well-known welfare conclusion that the "excess burden" imposed on an individual by a progressive income tax is necessarily greater than that imposed by an equal-revenue proportional tax.[2] Regarding the practical importance of their dramatic modification of the traditional view the authors are, however, extremely cautious, and suggest that "The circumstances which give rise to this neglected result are of doubtful realism. . . . For all practical purposes the theorem remains valid . . ." [1, p. 377]. Few of their readers can have felt so sanguine. Is it really so unlikely that "marginal utilities decline slowly"?

It is the purpose of this note to show, by means of a careful re-examination of the conventional geometrical analysis, that the Barlow and Sparks result, though technically correct, is of negligible practical interest for reasons completely unrelated to the rate of decline of the marginal utilities. Thus we shall find that their proportional-progressive tax comparisons are in general irrelevant. When appropriate alternatives are compared, the familiar result holds without qualification. The single-individual case considered in the conventional analysis (and by Barlow and Sparks) is, however, extremely unrealistic; for practical purposes we are interested in the group. Again comparing relevant alternatives, we find that the familiar result for the single-individual case need not hold for the group.

I. *The Single-Individual Case*

The conventional analysis considers the case of a *single individual* free to choose between leisure and income, both assumed to be subject to diminishing marginal utility. In Figure 1, pre-tax opportunities are represented by A_0B_0 with the individual in equilibrium at X_0, where indifference curve i_0 is tangent to A_0B_0. The effect of a lump-sum tax of amount A_0A_1 is to establish a new equilibrium at X_1 on the new opportunity line A_1B_1 parallel to A_0B_0.

If the government attempts to raise the same revenue A_0A_1 by means of a proportional income tax, the net wage rate to the individual is reduced. In the familiar geometrical exposition it seems to have been assumed that the

[2] Indeed their analysis suggests the need for a similar revision of accepted views regarding the positive and welfare effects of more and less progressive income taxes, not only on the supply of effort, but also on the supply of savings and risk-taking. The results of our re-examination of their findings with respect to effort can be applied *mutatis mutandis* to the other cases also.

In these simple cases it is of course assumed that supply is perfectly inelastic for all factors except the one under consideration. Where more than one factor is elastically supplied, the analysis becomes more complicated, and the results we shall obtain for the single-individual case need no longer hold. The reasons for our agnosticism in the case of the group are also correspondingly reinforced.

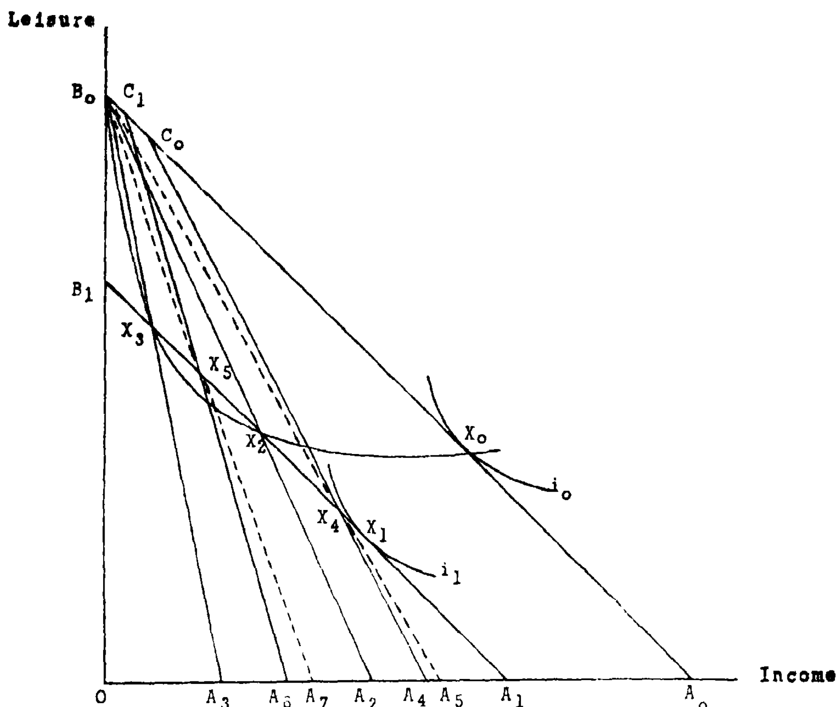


FIGURE 1

rate of proportional tax, and hence the position of the proportional tax opportunity line, is uniquely determined by the requirement that the post-tax line should be tangential to an indifference curve on the equal-revenue locus A_1B_1. In general, however, this is obviously not the case. The diminishing marginal utility assumptions in no way exclude the possibility that there may be several different proportional tax rates (and opportunity lines) all producing the same revenue A_0A_1.[3] Thus, when very conventional Hicksian assumptions regarding the shape of the "price-consumption" curve $X_0X_2X_3B_0$ generated by continuous variation of the proportional tax rate are made, it is apparent that the required revenue A_0A_1 can be raised by either of the two proportional taxes represented by opportunity lines A_2B_0 and A_3B_0.[4]

The price-consumption line $X_0X_2X_3B_0$ divides the equal-revenue locus into three segments, A_1X_2, X_2X_3, and X_3B_1. At points between A_1 and X_2 it follows from the diminishing marginal utility assumptions and the shape of the price-consumption line that indifference-curve slopes are everywhere

[3] Essentially the same point, however, has been quite widely recognized in the analysis of excise taxes. In fact, of course, the "income" tax, as conventionally interpreted and applied, is precisely an excise tax on effort. If income is interpreted more broadly to include an imputed value for leisure, the price effects we are considering disappear.

[4] It is interesting to notice that Musgrave's diagram [4, p. 242] includes a price-consumption line of precisely this sort. However, he overlooks or ignores the proportional tax alternative corresponding to our A_3B_0.

less than the slopes of the corresponding proportional tax lines. Let us now consider the possibility suggested or at least implied by Barlow and Sparks that a progressive tax opportunity line with slope less than that of A_2B_0 may produce equilibrium at some point X_4 below X_2 (though presumably above X_1!) on A_1B_1; in other words, that there may be a mildly progressive tax more stimulating to effort than the proportional tax represented by A_2B_0, and yielding the same revenue A_0A_1. Following the simple mathematical model of Barlow and Sparks, we shall assume that the progressive tax takes the form of a proportional rate applied to income in excess of an exemption; this in no way restricts the generality of the argument. Let us suppose then that the progressive tax opportunity line is $A_4X_4C_0B_0$, where the slope of A_4C_0 is less than that of A_2B_0. Is progressive tax equilibrium possible at X_4? To see that it is *not*, consider the proportional tax line A_5B_0 passing through X_4. From our particular assumptions regarding the indifference map (and reflected in the shape of the price-consumption curve $X_0X_2X_3B_0$), proportional tax equilibrium at X_4 is impossible because the slope of the proportional tax line A_5B_0 exceeds that of the corresponding indifference curve at X_4. However, the slope of the progressive tax line $A_4C_0B_0$ is necessarily greater than that of A_5B_0, and equilibrium in the progressive tax case is therefore a fortiori *impossible*.

Consider next the possibility of progressive tax equilibrium in the range X_2X_3. At points between X_2 and X_3, indifference-curve slopes are everywhere greater than the slopes of the corresponding proportional tax lines. Is it possible that a progressive tax opportunity line such as $A_6C_1B_0$ with slope less than that of A_3B_0 may produce equilibrium at some point X_5 below X_3 (but above X_2) on A_1B_1; in other words, that there may be a mildly progressive tax more stimulating to effort than the proportional tax represented by A_3B_0? Consider the proportional tax line A_7B_0 passing through X_5. From the price-consumption curve $X_0X_2X_3B_0$ and the diminishing marginal utility assumptions, it is clear that at X_5 the slope of the indifference curve is greater than that of A_7B_0. The slope of the progressive tax line $A_6C_1B_0$ necessarily exceeds that of A_7B_0, and progressive tax equilibrium is therefore possible at X_5.[5] For taxes with the right degree of progression, it is clear that progressive tax equilibria can be established at all points between X_2 and X_3. In general there will therefore be an infinite number of progressive tax schedules more stimulating to effort than the proportional tax represented by A_3B_0.

[5] It is interesting to notice that Musgrave's progressive tax opportunity line is drawn to produce equilibrium in a range corresponding to our X_2X_3. He does not, however, demonstrate that such an equilibrium is in fact possible, nor does he remark upon its significance for the theorem he is attempting to prove. Even more important, he fails to show that progressive tax equilibrium could not equally well be established in a range corresponding to A_1X_2. His "proof" [4, p. 241, n. 1] serves only to establish the possibility of progressive tax equilibrium somewhere in the range X_2B_1. Boulding [2], and Alan Williams [8, pp. 65-68] who follows his treatment very closely, offer no formal proof whatever. In a note published shortly after the rigorous analysis by Barlow and Sparks, Malcolm Levitt [3] provides a clear statement but no formal proof of the Barlow-Sparks result, and similarly implies, but does not prove, the impossibility of progressive tax equilibrium in our range A_1X_2. He apparently fails to see the significance of the latter result.

Indeed, considering the segment X_3B_1, it is easy to see that there is no progressive tax as depressing to effort as the proportional tax represented by A_3B_0.

In the above argument we have made only the simplest Hicksian assumption regarding the shape of the price-consumption curve. In general there is no reason why the number of intersections with the equal-revenue locus, and hence the number of proportional tax alternatives, necessarily need be restricted to two.[6] Thus, for certain levels of revenue, the price-consumption curve may divide the equal-revenue locus into a number of additional segments having similar characteristics to A_1X_2 and X_2X_3. Progressive tax equilibrium will then be impossible in segments corresponding to A_1X_2 where indifference-curve slopes are less than proportional-tax slopes, and possible in segments corresponding to X_2X_3 where indifference-curve slopes are greater than proportional-tax slopes.

As a result of this analysis we can conclude that, in the conventional framework, there will in general be many progressive income tax schedules more stimulating to effort (and imposing a smaller excess burden) than at least one proportional tax of equal yield. It is certainly true, as Barlow and Sparks suggest, that this result has been widely overlooked or ignored in the public finance literature; and, contrary to Barlow and Sparks, there seems no reason whatever to suppose that the circumstances which give rise to it are in any sense unrealistic.

More important, however, is our finding that in the case of the single individual there is no progressive tax schedule capable of producing as much work effort (or imposing as small an excess burden[7]) as the "best" (or least burdensome) proportional tax yielding the same revenue. In their analysis Barlow and Sparks completely overlook the crucial possibility that the revenue produced by their proportional tax CB/OB might alternatively have been produced by a lower-rate proportional tax more stimulating to effort. In fact, however, it is only where such a lower-rate alternative actually exists that their argument [1, Sec. III] regarding the existence of a more stimulating progressive tax can possibly be valid.[8] Similarly in their mathematical model it can readily be shown that $ds/de < 0$ (for $e = 0$) only where $ds/dt > 0$, viz., where $\eta_y < t$ (s being leisure, e the exemption level, t the rate of proportional tax, and $-\eta_y$ the elasticity of the marginal utility of

[6] In particular cases there may of course be only one, or even none, depending upon the amount of revenue to be raised. In the former case the price-consumption curve will be tangential to A_1B_1 at a single point, and the revenue raised will be a maximum for the family of proportional taxes. Here there will be no progressive tax capable of raising the given revenue A_0A_1. It is also conceivable that there may be several proportional taxes capable of raising the maximum revenue for the proportional tax family. For this level of yield there will again be no equal-revenue progressive tax for purposes of comparison. See Pigou [7, Pt. II, Ch. 5].

[7] Even in the case of a single individual, the conclusion regarding excess burden is generally valid only if there are no other taxes already in existence.

[8] In this case, however, their argument [1, Sec. II] regarding the possibility of a *less* stimulating progressive tax is necessarily invalid if we assume a conventional Hicksian price-consumption curve.

income schedule); in other words, that an equal-revenue progressive tax can stimulate more effort than a given proportional tax only where there are other equal-revenue proportional taxes (at lower rates) more stimulating to effort than the one under consideration. For the best proportional tax, $ds/dt > 0$ is ruled out, and $ds/de < 0$ is therefore impossible. It could reasonably be argued, however, that for relevant comparisons, only the best proportional tax should be considered.[9] Why should the government consider imposing a high and extremely burdensome rate of proportional tax if the same (or even a greater) revenue can be raised less painfully by imposing a lower rate? If this argument is conceded, it is clear that the traditional result, properly formulated, holds without qualification.

Regarding the more general question as to the relative effects of more and less progressive (or regressive) income taxes, of which the problem we have been considering is merely a special (and very simple) case, we can show, using a similar geometrical approach, that if the degree of progression of a tax family remains constant over all ranges of income, the best member of a more progressive family necessarily reduces effort more and imposes a greater excess burden than the best member of a less progressive family. If the degree of progression varies over different income ranges, only the conclusion for the special case we have been considering necessarily still holds.[10]

II. *The Group Case*

For practical purposes, however, we are interested in effects on the community rather than the single individual. As it stands, the conventional analysis of the single-individual case tells us nothing about this more important question. It might, however, be suggested that if the substitution of the best progressive income tax for the best proportional tax of given revenue necessarily reduces effort in the case of a single individual, the effect of making this same substitution for each and every individual must surely be to reduce work effort (and welfare) of all individuals and hence total work effort (and welfare) for the community.

This generalization to the group which follows naturally from the form of the traditional analysis, though formally unobjectionable, is, however, notoriously misleading. In general, the only way in which such a substitution could be made in the income tax field would be by applying different progressive rate schedules to all individuals, these schedules being so designed as to impose the same total liability on each individual as under the given proportional tax. Such a procedure is obviously administratively impossible and politically unacceptable. Moreover, the main reason for considering the progressive

[9] At the other end of the spectrum of relevance, the fact that there is no progressive tax so burdensome and depressing to effort as the "worst" proportional tax is clearly of no conceivable practical interest if, as seems reasonable in this context, we can abstract from "merit wants" or sumptuary considerations.

[10] For purposes of this more general analysis the degree of progression can be defined in a number of ways, all yielding the same results. Pigou [7, Pt. II, Ch. 2] distinguished two possible measures, the rate of change in the average tax rate and the rate of change in the marginal tax rate. These and other possible definitions are discussed by Musgrave and Tun Thin [6].

tax alternative in practice is of course that it provides the obvious means of imposing relatively higher average rates of tax on the higher-income groups. The progressive tax substitution envisaged above could not, however, perform this function, but simply represents a diabolically complex method of imposing the constant average rate of the proportional tax.[11] In general, and with relatively minor modifications, it therefore seems necessary, for comparisons of even the slightest practical relevance, to consider substantially the same rate schedule applied to all individuals.

However, if the same rate schedule must be applied to all individuals, it is only if leisure-income tastes and opportunities are identical that the familiar results will still hold.[12] In the more relevant and realistic case where tastes and opportunities may vary widely, no sweeping generalization is possible. The substitution of the best progressive tax for the best proportional tax will raise marginal and average rates of tax to high-income individuals, and lower them to low-income individuals, whilst some middle-income recipients will pay higher marginal and lower average rates. Given only the conventional diminishing marginal utility assumptions, the net effect on the supply of effort, both for the higher and lower income groups and for the community as a whole, is obviously impossible to predict.[13] A similar agnostic conclusion holds also for welfare effects on the community.[14]

[11] If the reason for considering the progressive tax alternative is (say) built-in flexibility, this particular objection does not hold. The equal-revenue requirement must then of course be interpreted as applying "over the cycle."

[12] As emphasized in the pioneering contributions of Earl Rolph and George Break [5, pp. 110-22], and Ian Little [5, pp. 123-31], the simple geometrical analysis of the effects of taxation on a single individual, which we have applied to the leisure-income choice in Section I above, cannot be used to derive results applicable to the group. For the latter purpose the opportunity lines of the single-individual analysis must be replaced by the corresponding transformation curves, and the "given revenue" must be interpreted in terms of a given resource transfer to the public sector. Rolph and Break went on to show that the familiar Hicks-Joseph proposition regarding the "excess burden" of an excise as compared with an income tax, previously "demonstrated" with the aid of the single-individual diagram, necessarily holds for the group only if perfect pre-tax allocation is assumed. Little then demonstrated that, even if perfect pre-tax allocation is assumed, the proposition is still not generally valid unless factor supplies are perfectly inelastic.

Applying this general equilibrium methodology to our present comparison of the effects of proportional and progressive income taxes on leisure-income choice (with fixed quantities of other factors), the simple diagram of Figure 1 can readily be modified along Rolph-Break lines to incorporate a leisure-commodity transformation schedule of the familiar convex variety. Then, assuming perfect pre-tax allocation and a "world of equals," the proposition that the best proportional tax produces more effort and imposes a smaller excess burden than the best progressive tax securing the same resource transfer to the public sector can still be illustrated geometrically even in the case of a group. However, in this world of equals the main practical reason for considering not only the progressive tax alternative but any form of income taxation would have disappeared. A nondistorting lump-sum tax such as the poll tax becomes distributionally unobjectionable!

[13] This point has been clearly recognized and developed in some detail by Musgrave [4, pp. 243-46].

[14] The reason is, however, rather different, since the differential income effects of the progressive tax, which may be of great importance in the positive analysis, are irrelevant from the point of view of efficiency.

III. *Conclusions*

On the basis of the above discussion, it appears that the Barlow and Sparks analysis requires modification or clarification in a number of important respects. (1) In the conventional case of a single individual, there will in general be many progressive income taxes more stimulating to work effort and imposing a smaller excess burden than at least one proportional tax yielding the same revenue to the government. The circumstances giving rise to this result are in no way unusual or unrealistic. (2) As a rule, however, there seems no reason to suppose that the government will include these inferior (and higher-rate) proportional taxes in its comparisons. Concentrating on relevant alternatives, we find for a single individual that the best proportional income tax necessarily produces more work effort and imposes a smaller excess burden than the best progressive income tax. This is in fact precisely the formulation of the traditional result to be found in Pigou's classic study [7, Pt. II, Ch. 5]. (3) The single-individual case considered in the conventional analysis is, however, of negligible interest in itself, and extremely misleading in its apparent implications for the group. In the general case of a community of individuals with varying tastes and leisure-income opportunities, the effects, both on the supply of effort and on excess burden, of substituting the best single progressive rate schedule for the best single proportional rate schedule are impossible to predict. Thus for reasons very different from those implied by the Barlow and Sparks analysis, the simple theory we have been examining suggests no weighty objections to progressive income taxation from the point of view of announcement effects on the supply of effort. Here again it is salutary to notice that the classic formulation of Pigou remains unexceptionable [7, Pt. II, Ch. 6].

J. G. Head*

* The author is senior lecturer in economics at the Australian National University, Canberra, Australia. This paper was written while he was visiting lecturer in economics at Princeton University, spring semester, 1964-65. He is indebted to Richard Musgrave and Carl Shoup for stimulating and helpful comments.

References

1. R. Barlow and G. R. Sparks, "A Note on Progression and Leisure," *Am. Econ. Rev.*, June 1964, *54*, 372-77.
2. K. E. Boulding, *Economic Analysis*, 2nd. ed. New York 1948.
3. M. Levitt, "Comparison of the Equilibrium Labour Supply under Proportional and Progressive Taxation," *Jour. Pol. Econ.*, Oct. 1964, *72*, 496-97.
4. R. A. Musgrave, *The Theory of Public Finance*. New York 1959.
5. ——— and C. S. Shoup, ed., *Readings in the Economics of Taxation*. Homewood 1959.
6. ——— and Tun Thin, "Income Tax Progression, 1929-48," *Jour. Pol. Econ.*, December 1948, *56*, 498-514.
7. A. C. Pigou, *A Study in Public Finance*, 1st ed. London 1928.
8. A. Williams, *Public Finance and Budgetary Policy*. London 1963.

A Note on Progression and Leisure: Reply

As far as the technical aspects of Head's contention are concerned, we are in agreement that our condition for an increase in work effort as a result of replacing a proportional tax by an equal-yield progressive tax can hold only if there exists a lower rate proportional tax with the same yield. This is clearly demonstrated by his geometrical exposition, although his interpretation of our mathematical model is not clear. The question at issue is whether, given an initial proportional tax rate which satisfies the equilibrium conditions stated in our equations (2), (3), and (4) for a given yield and a zero exemption level,[1] there exist other values of t which also satisfy these equations. This question cannot be answered simply from the sign of the derivative ds/dt. A rigorous condition can be derived by observing from Head's diagram that a lower rate equal-yield proportional tax will exist if, and only if, the falling price-consumption curve cuts the equal-yield line from above at the initial equilibrium point; that is, if, and only if, the slope of the price-consumption curve is greater (in absolute value) than the slope of the equal-yield line at the initial equilibrium point. This condition can be shown to hold precisely under the circumstances specified by our inequality (12).[2]

As for the practical importance of our result, we feel that Head overstates his case. His contention that a comparison between a progressive tax and the "best" proportional tax is "appropriate," "valid," and "relevant," whereas other comparisons are "irrelevant," is essentially arbitrary if one is concerned with predicting the effect of tax changes on the supply of effort rather than the welfare implications of different taxes. In a real situation, it is not self-evident that the tax which is to be replaced will be the "best" of its family.

Head is correct in saying that the case of the group, as analyzed by Musgrave, is of more practical interest than the case of the individual. Nevertheless our concentration on the individual was deliberate, since after Musgrave's treatment of the group, the case of the individual remained as the only theoretical case which allegedly "proved" that progression necessarily reduces work effort.

ROBIN BARLOW AND GORDON R. SPARKS*

[1] See Robin Barlow and Gordon R. Sparks, "A Note on Progression and Leisure," *American Economic Review*, June 1964, *54*, p. 376. We would like to take this opportunity to correct two minor typographical errors in our original communication. Equation (2) should be

$$y = (1 - t)x + e$$

and Equation (5) should be

$$\frac{dy}{de} + (1 - t)w\frac{ds}{de} + x\frac{dt}{de} = t.$$

[2] *Loc. cit.*, p. 377.

* The authors are assistant professors of economics at the University of Michigan and Massachusetts Institute of Technology, respectively.

Diminishing Returns and Linear Homogeneity: Further Comment

It is with considerable interest that we read the different communications on "Diminishing Returns and Linear Homogeneity" that appeared in this *Review* (September 1964). Those of Dr. D. Schneider and Professor G. Warren Nutter retained our special attention. Unfortunately both the communications, otherwise ably written, contain serious mistakes when they treat the cases of the so-called Law of Diminishing Returns in the history of economic analysis. Of the two, Dr. Schneider's note is much more explicit on this point.

I

Dr. Schneider speaks of the "traditional law of diminishing returns" without, however, explaining clearly what he means thereby. If he means thereby the law "promulgated" by Serra and Turgot to which Knut Wicksell later subscribed, as he seems to imply (and in which Nutter seems to concur), he is guilty of half-truths. In the first place, Serra [4, pp. 23 ff.] is much more explicit on the so-called increasing returns from industries than on diminishing returns from agriculture, on which a clear and unambiguous statement from him is not easy to come by. Turgot's statement on the law of returns is, on the other hand, a marvel of rigour and precision.

As a matter of fact, if we are to speak at all of the "traditional law of diminishing returns" we must begin with its first explicit formulations by Turgot and West. Their formulations are, however, quite different. As we shall try to show presently, in modern economic analysis F. Y. Edgeworth and Wicksell are respective continuators of Turgot and West.

II

West [7, p. 12], unlike Turgot, constantly confuses between diminishing average returns and diminishing marginal returns and, unlike Turgot again, supposes diminishing returns to operate from the very beginning of the incremental application of the variable input. If we compare this with Wicksell's position [8] we find a close similarity between the two. In one place Wicksell speaks of the "decline in the average yield of labour when the number of labourers is increased" and he qualifies this as the "law of diminishing returns" [8, p. 122]. Elsewhere he speaks of the "law of variation of the total product of a given area, when the number of labourers and the intensity of agricultural work increases or decreases," and the numerical example he gives shows the case of diminishing marginal returns [8, p. 114]. Moreover, he maintains that "the law presumably operates from the beginning" [8, p. 123]. In all this Turgot's position [6] is quite different. Indeed his performance in this respect is of such high order that it remained for Edgeworth [1] only to give finishing touches to it and thereby render it technically more perfect. In fact, contrary to what Schneider and Nutter think, it is not Wicksell but Edgeworth who should be more justly bracketed with Turgot in this respect.

First, in the two input-one output case Turgot [6, p. 645] states what in effect is the law of variability of *marginal* and not of *average* returns to the

successive increments of the variable input, the other input remaining constant. This is exactly what Edgeworth [1, p. 67] calls the "primary concept" of diminishing returns.[1] Secondly, with Turgot [6, p. 644] again, as it is later with Edgeworth [1, p. 70], the diminishing returns are a criterion of a maximum. Finally, Turgot [6, p. 645] holds that there is first a phase of *increasing* marginal returns before *diminishing* marginal returns set in. Edgeworth's position [1, p. 68] again is basically similar. In other words, in the Turgot-Edgeworth case the graph of the (supposedly) differentiable output function (with respect to the variable input) is at first convex and then concave from below, whereas in the West-Wicksell case it is concave from the beginning. Moreover, Schneider's assertion that "there is a positive output even when no variable input is used" may be true for Wicksell but it is hardly true for Turgot [6, p. 645], who explicitly holds that "seed thrown on a soil naturally fertile but *without any preparation* would be an advance *almost entirely lost*";[2] which means that land is what Karl Menger [3, p. 37] calls in his now classic article a "dependent input" (*Unselbständiges Gut*), i.e., with l as land-input and $l > 0$, we have $f(l, 0) = 0$. Thus, unlike Schneider, we do not see any contradiction between the Turgot-Edgeworth case and the Knightian case [2, p. 99].

PARESH CHATTOPADHYAY*

* The author is lecturer in economics at the Institut d'Etude du Développement Economique et Social of the University of Paris. He expresses his gratefulness to Professor E. Malinvaud, who kindly went through an earlier draft and suggested a number of improvements.

[1] Thus G. Stigler [5, p. 112] is misleading when he asserts that the average and marginal returns "had been used as synonymous since the very discovery of the law of diminishing returns."

[2] Our italics.

REFERENCES

1. F. EDGEWORTH, "The Laws of Increasing and Diminishing Returns," *Papers Relating to Political Economy*, Vol. 1, London 1925.
2. F. H. KNIGHT, *Risk, Uncertainty and Profit*. New York 1964.
3. K. MENGER, "Bemerkungen Zu den Ertragsgesetzen," in *Zeitschrift für Nationalökonomie*, Vol. 7, 1936, pp. 25-56.
4. A. SERRA, "Breve trattato delle cause che possono far abbondare li regni d'oro e d'argento dove sono miniere," *Scritti classici italiani di economia politica*, parte antica, tomo 1, Milan 1803.
5. G. STIGLER, *Production and Distribution Theories*. New York 1941.
6. A. R. J. TURGOT, "Sur le mémoire de Saint Péravy," in G. Schelle, ed., *Oeuvres de Turgot et documents le concernant*, Vol. 2, Paris 1913-1923.
7. E. WEST, *Essay on the Application of Capital to Land*, ed. J. H. Hollander, Baltimore 1903.
8. K. WICKSELL, *Lectures on Political Economy*, English translation, Vol. 1, London 1934.

Diminishing Returns and Linear Homogeneity: Further Comment

The interesting exchange between Liebhafsky, Sato, Rowe, Schneider, de Fontenay, and Nutter in the September 1964 issue of this journal leaves one loose end which proves both embarrassing and challenging. If they are correct, as undoubtedly they are, in their assertions as to the compatibility of both short-run inflection points and linear homogeneity in certain production functions, then the Stonier and Hague [3, p. 226] demonstration of their absolute incompatibility leaves something to be desired. As one who in microeconomics classes agreed with the Stonier-Hague position, citing their geometrical proof in evidence, the embarrassment generates the challenge of detecting where it is these gentlemen went wrong.

Consider their argument: In their Figure 89 [3, p. 226], presented here[1] as Figure 1, linear homogeneity makes $RP=PQ$, and the isoquants vertical displacements of one another. Since the iso-cost curves are parallel, $LP=PM$. Also, since $EP<LP=PM$, $PF>PM>EP$. Q.E.D. "This proof

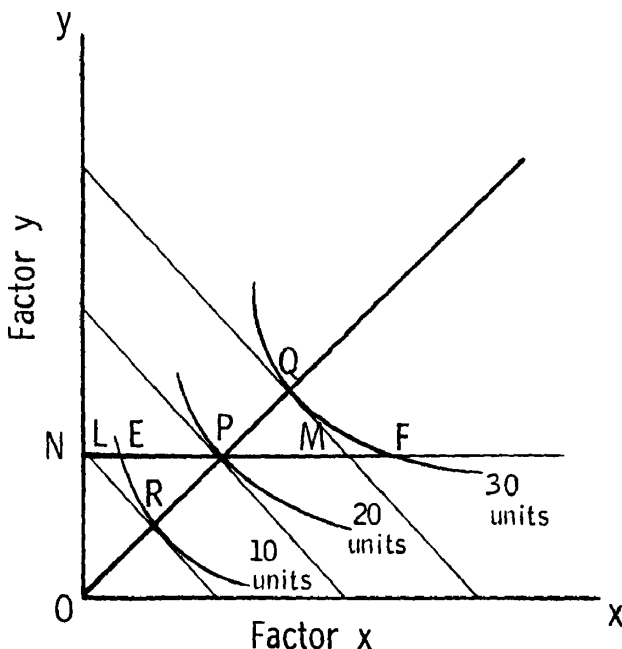


FIGURE 1

Source; [3, Fig. 89, p. 226].

holds with any equal product map representing a homogeneous production (function) of the first degree. On any such equal product map the marginal physical productivity of a variable factor always falls as more of it is added to a fixed factor" [3, pp. 226–27].

[1] Our Figures 1, 2a, and 2b are taken from Figures 89, 90A, and 90B, page 226, A. W. Stonier and D. C. Hague, *A Textbook of Economic Theory*, 3rd ed. (New York, John Wiley & Sons, 1964), with the permission of the publishers.

They go on to present another case (their Figures 90A and 90B, presented here as Figures 2a and 2b), in which the marginal physical product on

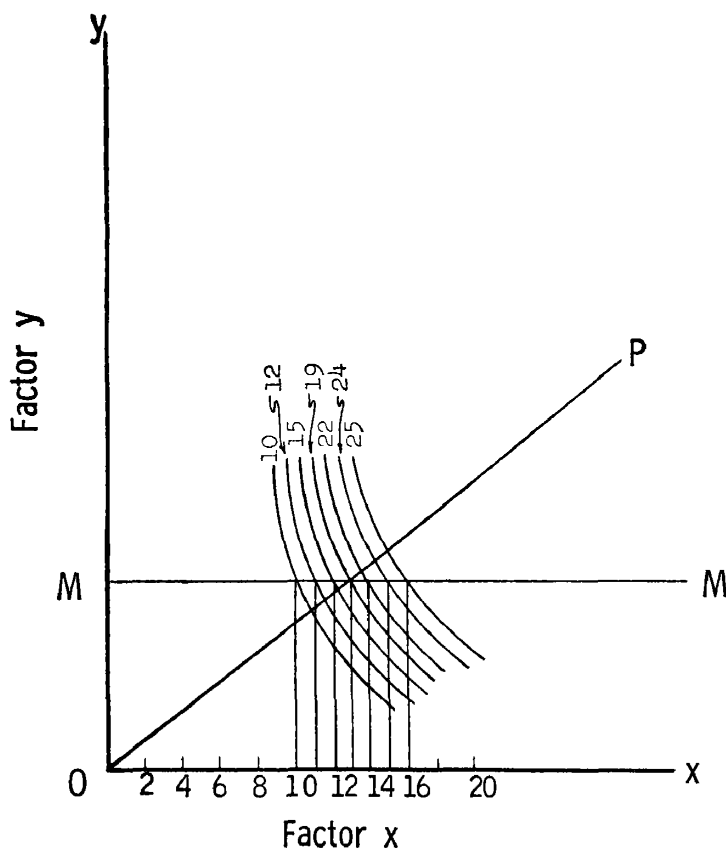


FIGURE 2a

Source: [3, Fig. 90A, p. 226].

one factor may rise before falling, but they make it abundantly clear that this could not represent the short run in a linearly homogeneous production function.

Therefore, the moral of the debate previously cited is, in effect, that Stonier and Hague have committed a Type 2 error in Figure 1, i.e., they accepted the false hypothesis that all linearly homogeneous production functions exhibit universal decreasing marginal products, and made a Type 1 error in Figure 2, i.e., they reject the true hypothesis that decreasing marginal products can be generated by a linearly homogeneous production function. How did this happen?

First, Nutter's production function [1, p. 743]

$$O = 22y^{1/4}x^{3/4} - 20y^{1/3}x^{2/3} \tag{1}$$

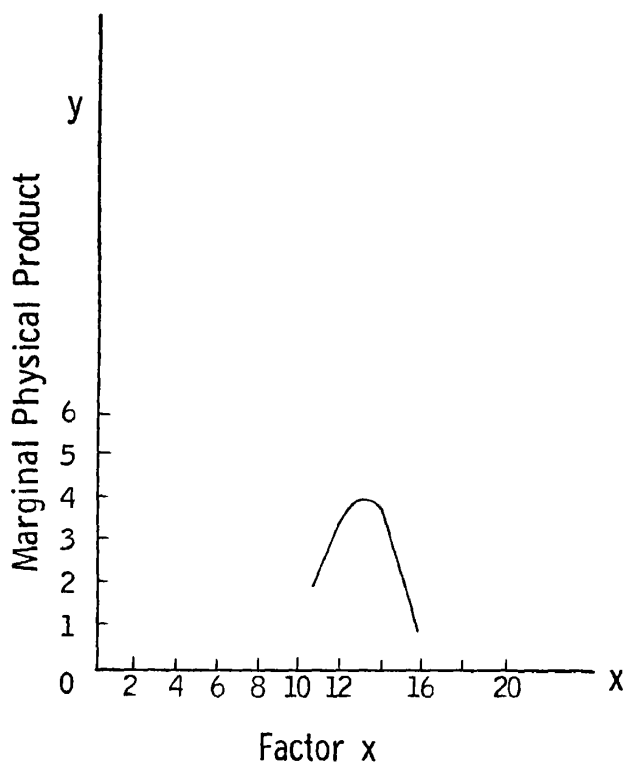


FIGURE 2b

Source: [3, Fig. 90B, p. 226].

shows, for y fixed at 1, first rising marginal product of x in the range $0.01 < x < 2.50$ and then falling marginal product in the range $2.50 < x < 100$. Similarly, Rowe's production function [2, p. 746]

$$O = \frac{y^2 x^2}{15y^3 + .5x^3} \tag{2}$$

shows a range of increasing marginal product for x (y fixed at 1) when $0 < x < .5265$.

This indicates that in Figure 1, had more isoquants been drawn, say, for 40 units and 50 units of output, the demonstration of monotonically declining marginal product for x could prove difficult indeed. The persuasiveness of Stonier and Hague's proof seemingly lies in the fortuitous choice of the fixed value for input y. For at ON, point P is located such that it is simultaneously a non-end-point of line segment LF *and* on the scale line OQ. This means that the crucial theorem of the whole equalling the sum of its parts can be used, i.e., when EP is unambiguously $< LP$ and PM is unambiguously $< PF$, the points are "placed correctly." But, imagine the line segment PF extended to cut a 40-unit isoquant. This would be equivalent to

having originally fixed y at height R. The interior of the line segment R, . . . (the intersection with the 20-unit isoquant) is now "badly placed," for there are distances *between* the iso-cost curves and isoquants whose lengths are, in general, geometrically unknowable. This makes the parallel-line theorem used by Stonier and Hague useless for it now cannot be paired with the whole-parts theorem.

An amusing production function nevertheless can be inferred from Figure 1 if we assume that Stonier and Hague's linearly homogeneous function exhibits first diminishing returns then increasing returns to input x. Take the function

$$O(x, y) = y[a + (x/y) + b)^3], \quad (a > 0, b < 0, a + b^3 > 0), \tag{3}$$

and fix y at y^* to get

$$O(x, y^*) = y^*[a + (x/y^* + b)^3]. \tag{4}$$

The marginal product of x will always be positive, i.e.,

$$O'(x) = 3(x/y^* + b)^2 > 0 \tag{5}$$

and the marginal product of x will first fall and then rise, for

$$O''(x) = \frac{6}{y^*}(x/y^* + b)\begin{cases} <0 & \text{if } x < -by^* \\ =0 & \text{if } x = -by^* \\ >0 & \text{if } x > -by^*. \end{cases} \tag{6}$$

Thus, if line segment LF were extended far enough, we would see increasing marginal product of input x, a result opposite to that portrayed in microeconomics texts (including Figure 2), and the reverse of the kinds of functions which were the subject of the Nutter-Liebhafsky controversy.

Algebraic specification of production functions, then, seems to be the necessary starting point for any study of "laws" of returns, for Figure 2 could well represent a picture of the Nutter or Rowe functions, a result which the inherent shortcomings of plane geometrical proof convinced Stonier and Hague was "impossible."

ROBERT PIRON*

REFERENCES

1. H. H. LIEBHAFSKY, "Diminishing Returns and Linear Homogeneity: Comment," *Am. Econ. Rev.*, Sept. 1964, 739–44.
2. J. W. ROWE, JR., "Diminishing Returns and Linear Homogeneity: Comment," *Am. Econ. Rev.*, Sept. 1964, 745–46.
3. A. W. STONIER AND D. C. HAGUE, *A Textbook of Economic Theory*, 3rd ed. New York 1964.

* The author, an instructor in economics at Oberlin College, is a research fellow at Harvard University for the academic year 1965–66. He wishes to thank Robert Weinstock, Physics Department, and Hirschel Kasper, Economics Department, Oberlin College, for their valuable suggestions.

Notes on Marxian Economics in the United States: Comment

Martin Bronfenbrenner, in the December 1964 issue of this *Review*, deplored the absence of professors of economics adhering to and teaching according to the viewpoint of Marxism on American campuses. In particular, he expressed the opinion that, given a choice between an off-campus conservative economist like Henry Hazlitt and an off-campus Marxist economist like myself, two-thirds of the present campus economists would choose Perlo as a visiting professor for a year. Bronfenbrenner included a provision that this would be at no cost to the college or university. However, I presume that this provision would not affect the choice of the professors.

To test Bronfenbrenner's observations, in January and February of 1965 I wrote the heads of economics departments of 18 U.S. universities, most of them major institutions, referring to the *AER* communication and indicating my availability as a visiting professor for the 1965-66 academic year. I noted, however, that I would require a normal salary. I included a listing of courses I thought I could teach most effectively and a vita.

Of the 18, 14 replied. Three stated that they or their departments were definitely interested and would investigate to find out what could be done. By now it is evident that none of these three will offer me a visiting professorship for the 1965-66 academic year. However, from further correspondence and conversations I conclude that the subject is still alive in all three, either for a semester during the academic year, for the succeeding summer session, or for the following academic year.

From correspondence and conversations with two of the departments concerned, it appears that administrative authorities (Board of Trustees, Dean of Faculty) may well prove decisive obstacles, although they have not yet been tested.

Ten heads of departments stated that there were no appropriate openings for the 1965-66 academic year, either because of lack of funds or because all suitable positions were already filled, or both. Of these, four stated that they liked the idea and would attempt to carry it out for a later academic year. A fifth stated agreement with Bronfenbrenner but made no mention of the future. He expressed hope that some other university would be able to arrange a visiting professorship for me, as did several others. The remaining five in this general category made no mention of later years.

The final respondent was "intrigued" by my suggestion and said he would give it thought. While preferring me to Hazlitt, he was wary of "doctrinaire Marxists."

One respondent in writing, and a number of professors in conversation, noted that state universities "have more problems in employing an avowed Marxist" than privately endowed institutions because Trustees must consider possible legislative repercussions as well as their own inclinations.

Most responses were friendly in tone, although some were merely courteous, and some were enthusiastic.

Concerning the four heads of departments who did not reply, I do not assume that this designates hostility or indifference. I identified heads of departments from the *1964 Handbook of the AEA*. In a number of cases the head of department had changed by this year, as I discovered when my letters were

answered by different persons, sometimes after several months delay. It is possible that similar accidents accounted for some or all of the four unanswered letters.

* * *

On another subject: this *Review* has accepted and published the two contributions I offered, and it has a reasonably good record of reviewing my books and, I believe, books of other Marxists. But no Marxist has appeared on an annual meeting program of the American Economic Association since Paul Baran was a discussant in December 1959. I, for one, have never been invited to participate in an AEA program, although I have been in those of some other professional associations.

It seems to me that Marxist views should be solicited for all annual meetings of the American Economic Association. At least five of the major subjects of the 1964 annual meeting really required presentation of a Marxist viewpoint to give a rounded picture of economic thought and research results:

1. Economic Growth: International Comparisons;
2. Comparative Economic Systems: Nationalized Industry;
3. Economic History: Its Contribution to Economic Education, Research, and Policy;
4. The New National Econometric Model: Its Application;
5. The Economics of Poverty.

This is not to decry the many good papers that were presented, nor to assert that it would be easy to find in the United States Marxist economists available to present papers on all of these subjects. I assume that program invitations are decentralized, with a given professor in charge of each subject. Thus a certain amount of advance coordinating may be necessary to insure the invitation of Marxists to participate in the programs. But I daresay such difficulties could be overcome and the Association could markedly improve over its past record in this respect.

VICTOR PERLO*

* The author is an economic consultant, Croton-on-Hudson, New York.

The Burden of the Debt: A Mathematical Proof

A familiar proposition is that we owe the national debt to ourselves. However, this proposition has still not been rigorously proved. My intention is to provide such a proof.

The proposition that the national debt as a liability must just equal the total of the associated assets can be stated as follows:

$$\sum IOU\text{'s} = \sum UOMe\text{'s}. \tag{1}$$

If we divide both sides of the equation by OU, we have:

$$\sum I\text{'s} = \sum Me\text{'s}. \tag{2}$$

Q.E.D.

JOHN M. KUHLMAN*

* The author is professor of economics at the University of Missouri.

BOOK REVIEWS

General Economics; Methodology

On Economic Knowledge—Toward a Science of Political Economics. By ADOLPH LOWE. New York and Evanston: Harper & Row, 1965. Pp. xxi, 329. $6.95.

This volume presents Adolph Lowe's distillation of "forty years of reflections about the subject matter and the method of a science of Economics." He explains that "the reconstruction of economic science is the ultimate aim of this inquiry" (p. 159). Lowe's reconstruction includes not only (1) a broadening of the scope of economics, but also (2) the construction of a theory of controlled economic systems, and (3) the application of new methodological procedures.

With regard to the nature and scope of economics it is Lowe's position that, while the body of universal or general economic theory developed by economists from Smith to Keynes is very useful and cannot be dispensed with, nevertheless this general theory is incomplete since it has little connection with the sociopolitical environment in which the market system is lodged. Lowe asserts that economics should be extended beyond the body of "traditional" economics to include an analysis of the factors and forces in the institutional environment that have a large impact on the operations of the market system. He combines the study of "man-to-matter" relationships (nonsocial) in the area of pure economics with the study of "man-to-man" relationships (social) in the sociopolitical environment of the private market system in order to create a Political Economics. This economics "tries to relate the economic core process [the market system] to the dominant factors of the sociopolitical environment" which have much to do with determining the structure and functioning of the market system (p. 249). Political Economics then becomes the study of "controlled economic systems."

The controlled economic system that is Lowe's primary concern is the "organized capitalism" of Western Europe and the United States. In organized capitalism, market behavior has been greatly altered as the result of the increasing immobility of industrial capital, the growing uncertainty of business expectations, and the very long-term financial commitments required of corporate enterprises. In this new era of capitalist development the public authorities endeavor to provide a better working of the private market system with the aid of manipulative and command controls. The public control of market activity is designed to reduce economic uncertainty and to provide an economic climate favorable to the good functioning of private enterprise. Lowe sees Western economic systems moving toward some kind of capitalist planning, such as is found today in France, in which decentralized private economic de-

cision-making is combined with manipulative controls and a minimum of "command" control.

Having broadened the scope of economics to include the analysis of the institutional environment of the market system, Lowe then raises the question as to whether or not his Political Economics, like pure economics, is a science that ascertains "empirically relevant theorems" or "repeatable patterns" of behavioral interdependence. He explains that the studies of the sociopolitical process made by Karl Marx and other investigators of capitalism have not revealed any "regularities of motion sufficiently strict to be formulated in laws of coexistence and succession." Lowe proposes to remedy this deficiency of broad-gauged economic analysis by using a new methodological approach in which observation and analysis are combined with "participation" or "actual intervention in the course of events."

Lowe's new methodological procedure, described as "instrumental inference," reverses the traditional procedure in which the economist moves from a postulated initial state in which known behavior and motivations lead to a terminal state of market equilibrium. Lowe explains that in organized capitalism the government sets up certain macro-goals relating to such matters as economic growth and consumption and investment levels. Once these goals have been established, it is then possible to infer or demonstrate what economic arrangements and business behavior are necessary to assure goal attainment. The postulated terminal state at the end of the control or plan period indicates what economic policies must be adopted in the initial state (at the beginning of the control or plan period) in order to achieve the projected national economic goals. According to Lowe's interpretation, public control bestows on the economic processes of the real world that degree of orderliness that is a precondition for theoretical generalization. It is then possible to ascertain empirically relevant theorems and to predict the course of economic behavior towards goal attainment. The motion of the whole economic system becomes determinate and one can then speak of a science of Political Economics.

Lowe's views on the nature, scope, and methodology of economics are not likely to find much acceptance among a large section of the economics profession. Many economists will not accept his interpretation of the evolution of the capitalist system towards a controlled economic system. Others are not likely to follow his suggestion that they should move beyond the limited confines of general or pure economic theory to take account of the impact of factors in the sociopolitical environment on the operations of the market system. Lowe's reflections on economics should have a special appeal to economists like Gerhard Colm, Clarence Ayres, Gunnar Myrdal, and others who have already been working along the lines suggested by him. These heterodox economists will in general find themselves in agreement with Lowe's views on the nature, scope, and methodology of economics. More than this, Lowe's reflections on economics should provide a much-needed scientific underpinning for the work of economists who do not fit into either the "free enterprise" or the Keynesian mold.

University of Maryland ALLAN G. GRUCHY

Patterns of Market Behavior. Edited by MICHAEL J. BRENNAN. Providence: Brown University Press, 1965. Pp. vii, 258. $8.50.

The collection of thirteen excellent essays in *Patterns of Market Behavior* is published as a tribute to Philip Taft on the occasion of his sixty-third birthday. The essayists are present and former colleagues, and former students. The general high quality of the essays in the volume allows one to conclude that Professor Taft's associates have delivered their tribute to him in commendable fashion.

Although Taft's primary interests and contributions are in the field of labor economics, each contributor to the volume has developed an essay in a subject area of his own interest and speciality. In keeping with the present state of methodology in economics, seven of the essays are cast in highly mathematical terms. To these the reader must bring some degree of sophistication in mathematics to gain a full understanding of the author's thesis and objectives.

The editor has arranged the essays in the volume under four major market categories: Commodity Markets: "On the Determination of Prices in Futures Markets," "Optimal Programs for Sequential Investments," "Some Economic Aspects of Outdoor Recreations"; Resource Markets: "A More General Theory of Resource Migration," "Improved Allocation of Labor as a Source of Higher European Growth Rates," "Time, Work, and Welfare," "Changes in Occupational Structure"; Money Markets: "A Commentary on Some Current Issues in the Theory of Monetary Policy," "The Bills Only Doctrine in Retrospect," "The Integration of Simple Growth and Cycle Models"; International Markets: "The Effects of Devaluation in a Growing Economy," "Differential Growth Rates Among Open Economies: Theory and Fact," "Economic Development and Comparative Advantage." A complete bibliography of Taft's writings is also included.

Some flavor of the substance of the essays can be gained from a brief review of several of them. James N. Morgan's article, "Time, Work, and Welfare," is especially timely in view of the current emphasis on the poverty problem. He uses original empirical data to examine the factors conditioning the annual hours of work an individual is willing to offer; other data shed light on the factors coincident with wives in the labor force; he develops an excellent exposition on the socioeconomic factors which accompany poverty in the United States.

Mark B. Schupack, in his essay, proposes a workable means of measuring changes in the age-structure of different occupations. Hyman P. Minsky in "The Integration of Simple Growth and Cycle Models" develops a framework within which to analyze growth and cyclical forces in an intermediate time-horizon of from ten to fifteen years. Minsky incorporates both aggregate supply and demand factors into his model, suggesting that the traditional short-run and long-run models have been concerned with only one or the other of these variables. His thesis should evoke some interesting discussions. Finally, by way of illustrating the scope of coverage, Jerome L. Stein takes Myrdal's thesis that the free play of market forces aggravates interregional inequalities in income per capita and demonstrates that the thesis is not tenable in a free capital movement area such as the United States. This article is wor-

thy of study by theorists of regional economic development. Because of the diversity of subject matter covered, and in view of the good quality of the material, we recommend that the profession become acquainted with this excellent volume.

GERHARD N. ROSTVOLD

Pomona College

The Economics of Competitive Coexistence—Convergence Through Growth. By CYRIL A. ZEBOT. New York: Frederick A. Praeger, 1964. Pp. ix, 262. $6.50.

This at times lucid and broad-gauged essay attempts an ambitious analysis of growth prospects of the three great divisions of the sociopolitical world: the Western, the Eastern, and the Developing Peoples. Communist strategy centers on overtaking the West by a resolute growth effort (pp. 3f.). The richer Western peoples also need further growth to satisfy their developing needs for a more comfortable and fuller life (p. 56). These needs are not checked by a Galbraithian tendency toward diminishing marginal utility of income growth nor are they produced by an emulative drive or by advertising effort (pp. 51, 57).

The developing societies can only realize their growth aspirations by educational programs which foster cultural skills and aptitudes required for industrialization. This human development allegedly overshadows in importance the coercive role of the state (pp. 18f.) which can, however, play a constructive role in providing education and in arranging for international or regional economic cooperation (pp. 21f.) including formal aid programs of advanced Western countries (p. 22).

The root problem of all three peoples—Western, Eastern, and Developing—is inflation. In the West the drive to inflation springs from the pressure of unmet wants for higher living standards noted above, by an institutionalized form of unionism which aggressively pushes up money wage levels (pp. 86f.), and by business power to administer prices according to standards which avoids downward price adjustments when unit costs fall and which otherwise strives for stable profit margins and maximization of sales rather than profits (p. 94). The drive to raise wage rates faster than productivity stimulates automation which thus represents "a very intensive substitution of mechanization for labor" (p. 88). This generates unemployment, on the one hand, and excess credit expansion on the other to finance the larger volume of output produced with augmented payroll charges and sold at somewhat higher price levels.

Communist and underdeveloped economies also experience the drive to inflation but in other forms. In underdeveloped countries it arises out of a tendency to introduce an advanced industrial technology ahead of the corresponding growth of technical skills, thus producing unemployment, high production costs, and inflation (p. 123). In the Soviet Union inflation arises out of the totalitarian character of the communist political economy and its ruling class. This political economy centers on manipulation of physical controls

with a corresponding neglect of the market-clearing and allocative functions of prices which fail to correspond to costs in any uniform way (pp. 125-27). Shortages and planning deficiencies are borne by the consumer who suffers from a chronic shortage of goods which undermines efforts at economic management or service of the people in their daily lives. In the Soviet economy inflationary pressures are thus directly related to the "gap that separates the totalitarian dictatorship of the Soviet government from the supposed socialist character of the Soviet economy" (p. 129).

The remedies for inflation, though taking different forms in Western, Eastern, or developing societies, works out to a "grand convergence through growth" (the subtitle of the book and the theme of the final chapter, "Conflict or Convergence"). In the Western world these remedies must involve regulation through fiscal policy and tax reductions of aggregate demand, "geared to a realistic level of full employment," and supported by credit policy which plays an adaptive, not an initiatory, role (pp. 96-107). Organized labor must agree to limit wage increases to productivity increases supplemented by premium payments in areas of labor shortage. Industrial management must permit final product prices to fall as well as rise. Procedurally, wage negotiations between management and labor need to be facilitated only by institution of a public chairman who will preside over the negotiations (p. 110).

The Eastern world is advised to meet the inflation problem by leavening the communist economy with socialist principles of politics. These would widen the role of the consumer interest and give increased emphasis to the price system, thus leading to a wide-ranging decentralization of "authority and institutions" (pp. 139-42). The underdeveloped societies will doubtless develop a political economy with a huge public sector, but which in most essential respects would fit into the decentralized, market-oriented political economy of the West.

This is merely the bare frame of argument which is expounded in appendices half again as large as the book. Inevitably so wide-ranging an argument is vulnerable. Only in this decade could the basic problems of so many peoples be reduced to that of inflation, which thus takes on the role of a universal sin. Did not Keynes but yesterday expound on the value of inflation as stimulating enterprise and loosening the hold of the dead hand of the past? Did not Schumpeter ascribe all development to innovation financed in a burst of inflation? Did not Wesley Clair Mitchell show us clearly why cyclical expansions would generate price rises?

And even if inflation is totally evil and sinful, still it hardly ranks as the predominant problem of our times. The Communist world desperately needs constitutionalism or limited government under law which is more difficult to achieve than a price system toward which clever Eastern technocrats aspire or devices of inflation control which Communist bosses can devise. I find myself resenting the notion that all underdeveloped peoples have the same root problems. Some may need family planning devices. Others may need to overthrow parasitic landed aristocracies. Other societies may need to disestablish a standing army and conquer militarism or to develop an orderly form of democratic government.

In the Western world too, problems are more varied than Cyril Zebot indicates. At least we in the United States are plagued with evils more deadly than creeping inflation: our inability to revamp the inherited chaos of municipal government which is turning our great metropolitan communities into shambles; the emergence of the handout as the supreme political principle of a welfare state and the ordering device for the distribution of a rising share of our national income; the domination of our public life by the public relations manipulators and bureaucracies of the mass media. And I don't like to see credit policy assigned merely an adaptive role.

MANUEL GOTTLIEB

University of Wisconsin-Milwaukee

Our Depleted Society. By SEYMOUR MELMAN. New York: Holt, Rinehart and Winston, Inc., 1965. Pp. x, 366. $4.95.

This book is an analysis of the economic consequences of the large-scale diversion of resources to military purposes. The author, a professor of industrial engineering at Columbia University, attempts to demonstrate that the nation's capacity for generating economic growth has seriously declined as a result of the high level of defense preparation.

In the 15 chapters of the book, the reasoning underlying the author's conclusion appears to consist of three propositions:

1. U.S. national defense policy has relied largely upon a concept of deterrence—the prospective losses with which the U.S. military forces confront an opponent have been structured so as to far exceed whatever gains the opponent may foresee; hence, the present amount of deterrence is either excessive or self-defeating.

2. National defense has absorbed scarce resources from their peacetime employment.

3. Defense preparations use a very large quantity of a comparatively small and fixed supply of technically skilled personnel.

To what extent can the methods of economic analysis be used to substantiate the three propositions? Taking the first proposition, the author advances two types of proof. The first is the over-kill line of reasoning: we possess sufficient redundancy of nuclear weaponry and their delivery systems to accomplish many times over any conceivable set of missions. The second type of substantiation, which is meant to explain why over-kill exists, consists of invoking the old merchants-of-death argument— the conspiracy of the military-industrial complex. A proof of a poorly performing military-industrial complex requires more than an invocation of former President Eisenhower's good name, which is the only evidence offered. One has a right to expect some discussion of the distinctive attributes of the defense industry's structure which might produce such an outcome. Unfortunately, the administrative pricing system which conditions industrial contractors' behavior has not been analyzed. Similarly, the intra-military reward-disbursement-behavior relationship is another empty analytical box. But it is simply not legitimate to assume the

worst possible case without any qualification or caution offered to the reader.

The second proposition requires the reader to believe that, if defense expenditures were substantially reduced, a large number of unsatisfied demands could be met, such as education, housing, and health care. No mention is made that these demands might remain unfulfilled even if there were no defense expenditures. The concepts of effective demand or the older distinction between needs and wants would have been appropriate in the context. Either one would have served to qualify what can only be called very excessive oversimplification.

The third proposition is in a sense a restriction on the previous one. It is argued that certain kinds of skills, especially those of engineers and scientists, are used in the production of defense preparedness in excessively large amounts. Thus, the author imputes the supposed technological retardation of the railroad industry to short supply of system engineering design talent. But the technological status of the railroad industry can be explained readily in terms of the economics of automobile and truck transportation and a distinctive administrative pricing system imposed upon railroads that insists upon a physical maximization of tons per train mile. In order to maintain that an absolute shortage of engineers and scientists exists, it is necessary to argue that the available supply of suitably endowed individuals is fixed. But the distribution of capabilities and capacity for learning in the population is usually shown to be quite widespread. Determination of the specific industries and occupations that suffer the greatest diminution of labor supply as a result of increased defense efforts must be assessed after the entire redistribution of labor occurs. It is very doubtful that the availability of skilled individuals for peacetime occupations has declined by the number who have shifted to defense employment.

P. M. Carrick

General Electric-TEMPO, Santa Barbara, California

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Probability and Profit: A Study of Economic Behavior Along Bayesian Lines. By William Fellner. Homewood, Ill.: Richard D. Irwin, Inc., 1965. Pp. x, 239. $7.50.

"A good many reasonable decision-makers—though by no means all—seem to act differently depending on whether they act under the influence of shaky degrees of belief, i.e., of probabilities the numerical values of which are highly unstable in their minds, or act under the guidance of firm and stable degrees of belief." Thus on page 4 William Fellner asserts the empirical relevance of the theory that forms the cornerstone of this monograph.

Fellner's primary objective is to broaden the scope of the Bayesian approach to decision-making under uncertainty. First, he rejects the limitations imposed by the strict frequentist approach (represented by Richard von

Mises), according to which probability reasoning applies only to the limited class of decisions involving events with objective probabilities. Second, he rejects the pure subjective probability approach (represented by L. J. Savage), according to which strict probability reasoning can handle a very wide range of decisions where probabilities are represented by degrees of belief. In place of these extreme positions, Fellner offers his own "semiprobabilistic" approach, according to which decision-makers *may* slant (usually downward) probabilities pertaining to events not encompassed by the frequentist approach. These probabilities may require special treatment either because they are interpersonally controversial or are unstable in the mind of the decision-maker, or both.

Thus, for example, we might observe that a person will bet on either heads or tails on the flip of a fair coin at a 2.1 to 1 pay-off ratio, but will not bet on *either* the proposition that the United States will put a man on the moon by the end of 1970 *or* the proposition that the United States will not put a man on the moon by the end of 1970, at the same payoff ratio. According to the subjectivists, the propositions about the coin toss and those about the moon landing all fall into the same general class. This implies that a rational person who is willing to bet on the coin toss at a 2.1 to 1 pay-off ratio must also be willing to bet on at least *one* of the propositions about the moon landing at the same ratio, since at least one of the propositions must have a subjective probability of .5 or more. According to the frequentists, the propositions about a moon landing are not comparable to the propositions about the coin toss and cannot be explained on the same or similar bases.

Fellner would contend that degrees of belief about the moon landing are likely to be shaky, and may therefore be slanted or discounted. For propositions about which degrees of belief are shaky, the decision-maker's decision weights, which are slanted probabilities, need not sum to one for all possible mutually exclusive events. Thus, a rational investor may refuse to accept either bet about the moon landing even though he will bet on the coin toss. In most other respects, however, the treatment of slanted probabilities parallels the procedures applicable to strict probability reasoning. Consistency requirements for semiprobabilistic behavior are set forth by the author, and the relation of these to the requirements of strict probability theory is discussed.

Fellner outlines a theory of profit that encompasses the semiprobabilistic behavior he envisages. He first describes a purely probabilistic model, in which the investor carries his commitment up to the point where the expected marginal utility of potential gains is equal to the expected marginal utility of potential losses—that is, where expected total utility is maximized. Fellner then incorporates semiprobabilistic behavior into the profit model. The revised model can explain behavior that cannot be interpreted by a pure probability approach. Fellner argues, furthermore, that his version is superior to such rule-of-thumb approaches as maximin of gain, maximax of gain, and minimax of regret.

Of greatest interest to those who wish to assess the usefulness of Fellner's contribution for economic analysis is Chapter 7. Here the author presents illustrations of his own approach, applying it to three problems in economic

theory. In the first illustration, it is shown that profit maximization leads the semiprobabilistic (slanting) investor to undertake a lower level of investment and output than purely probabilistic considerations would dictate. Fellner indicates that his approach may be used to rationalize observable tendencies toward average cost pricing.

The second example is related to "Cournotesque" behavior. Fellner shows how the introduction of uncertainty may explain why oligopolists pursue strategies that, compared with the ideal strategy of joint maximization, work to their mutual disadvantage. However it appears that Fellner's conclusion depends only on the introduction of uncertainty into the model and has no necessary connection with semiprobabilistic behavior.

The third illustration concerns investor preferences for liquid assets. James Tobin's model, which explains liquidity preference as behavior towards risk, is presented in slightly modified form, so as to take semiprobabilistic behavior into account. Downward-slanting individuals will play it safer (hold more cash, less securities) than pure probability reasoning would suggest. However —and this Fellner considers more important—the Tobin model assumes that the time of liquidation of noncash assets is known with certainty. But, asks Fellner,

> . . . what if . . . we take account of the fact that the individual assigns various probabilities to the prospect that he may have to sell at various dates, and to the prospect that he may be able to wait for a convenient date of liquidation without a sacrifice of other important objectives? In such a modified model a downward-slanting tendency expresses itself *also* in an inclination to overweight the probability that the security *will have to be sold at a time when its price is low*. I believe that, in the real world, this may well be the most important manifestation of the downward-slanting tendency.

The observable conservatism of financial managers *can* be explained, however, on strict probability grounds. Reduction of cash holdings increases the probability of having to liquidate on unfavorable terms; as a result, the marginal contribution of cash to the expected value of a portfolio increases as less cash is held.

Thus it would seem that, among the three illustrations provided by Fellner, the first argues most strongly for injecting semiprobabilistic behavior into the model. Nonetheless, the "slanting" notion has great intuitive appeal, especially as it applies to decisions of managers who must account to others, such as boards of directors, for decisions based on shaky judgments.

In addition to Fellner's own theory, the book contains discussions of the historical development of the theory of decision-making under uncertainty, and a valuable bibliography of 52 items with extensive commentaries. An appendix presents some not-very-compelling results of betting experiments with Yale students. ***Probability and Profit*** should interest all economists concerned with decision theory and should be a useful addition to reading lists of courses dealing with this subject.

Samuel B. Chase, Jr.

The Brookings Institution

Microstatics. By WILLIAM S. VICKREY. New York and Burlingame: Harcourt, Brace and World, Inc., 1964. Pp. x, 406. $7.95.

Metastatics and Macroeconomics. By WILLIAM S. VICKREY. New York and Burlingame: Harcourt, Brace and World, Inc., 1964. Pp. x, 314. $7.50.

We have here, apparently, Professor Vickrey's course in economic theory, as conveyed to us in a highly evolved set of lecture notes. All in all, it is an excellent course. It has its ups and downs, however, and more so than most courses. Both great triumphs and virtual disasters of pedagogy may be found in these two volumes. The proportion of triumphs is a good deal higher in the micro volume, where the author's heart presumably lies.

The unevenness in quality is related to a rather remarkable variability in the level and analytical style of the discussion. From page to page, Vickrey alternates among the roles of rigorous theorist, casual empiricist, philosopher of science, summarizer of uncited empirical investigations, critic of economic policy, worker of complicated numerical examples, popularizer of mathematical economics, and others as well. At one point, the reader will be led by the hand at a pace appropriate to an intermediate theory course, then there will be a section that will provide a good review for a doctoral candidate studying for his comprehensives, then a particularly novel perspective or insight likely to be helpful to the author's academic colleagues, then an example of extraordinary precision with theoretical fine points, and finally, there are sections which seem to be hasty summaries of analyses that Vickrey has done on the back of an envelope somewhere. The general effect is fairly lively and interesting. One great virtue is that the real world does not get hidden behind the analytical apparatus for very long periods at a time. On the other hand, the transitions in level and mode can be disconcertingly abrupt. The reader may get confused as to whether the world under discussion at a given point is the real world or some abstract theoretical world, and if the latter, whether the assumptions characterizing this world have been fully spelled out. Also, the instructor who uses these volumes as textbooks will have to do some careful planning if he is to provide the variations where Vickrey supplies only the themes, and the themes where Vickrey supplies only the variations. The latter situation occurs more frequently than most instructors would probably desire.

The first chapter of the micro book is, inevitably, the "What Is Economics?—What Is Economic Theory?" chapter, and it is a model of its kind, both sophisticated and concise. The next three chapters provide the analytical core of microeconomics, under the headings consumption, competitive exchange, and production. On the whole, these chapters are excellent. They are the best in the two books from the point of view of careful and insightful presentation of basic theoretical material; given the decision to eschew calculus or more powerful techniques, it would be difficult to surpass the precision and clarity of Vickrey's development of most of the central propositions.

Still, one can quibble here and there. For example, considering the general level of precision in these chapters, one might expect something better by way of introduction to the utility concept than the following: "Whether the economic unit is a single individual or a more or less cohesive group, our econom-

ic theory will assume for the time being at least that there is some valid and worthwhile objective which the behavior of the unit is intended to secure and in terms of which this behavior can be rationalized. . . . We will call the degree to which a unit achieves its objectives the satisfaction or 'utility' of this unit" (pp. 20-21). The ensuing discussion provides only a hint of the line of development in which the utility function appears as an implication of more fundamental assumptions about rational behavior, and it completely obscures the distinction between the use of rationality assumptions as an approach to positive economics and the combination of rationality assumptions and individualistic value judgments that lies behind welfare economics.

Also, both the consumption and production chapters suffer from the absence of any explicit use of the concept of a function. This concept is enormously easier to understand than some of the more arcane and arachnoid diagrams on which this "elementary" exposition of microeconomics is based. And in Chapter 6, Vickrey has an extensive discussion of a "social-welfare function of the individual rank orders"; as functions go, this is not an easy one to understand. Nevertheless, the treatments of many goods and many factors of production never refer to demand functions and cost functions, and thus they leave the uninitiated reader with very little information about the n-variable generalizations of the diagrammatic analyses. One does not need to use calculus to make the point that the general outcome of an optimization process can be summarized by the functional dependence of certain quantities on certain other quantities.

Chapter 5, entitled "The General Competitive Equilibrium," deals in a perfunctory and inexact manner with the optimality properties of competitive equilibrium. There is a sketchy argument that would not convince a skeptical reader that competitive equilibrium is Pareto optimal, followed by a section entitled "The Assumptions of the Theorem that Competition Is Optimal." In this discussion there is a complete muddling of conditions that are defining of competitive equilibrium, conditions that are necessary for its existence, conditions that are necessary for its Pareto optimality, and conditions that are necessary if every Pareto-optimal position is to be achievable as a competitive equilibrium. In direct violation of the intentions expressed in the preface and the viewpoint put forward in the introduction, the distinction between theoretical validity on given assumptions and empirical relevance is abandoned. Actually, almost three-fourths of this "general equilibrium" chapter is given over to variations on the theme of determining optimal resource allocation by maximizing the net social "surplus," measured by the appropriate areas on a demand and supply curve diagram. Some of this is very instructive and some of it is very tedious; the necessary caveats for the whole business are somewhat underemphasized.

The next chapter deals briefly with compensation criteria and their problems, but is devoted mainly to a very nice summary exposition of the Arrow impossibility theorem, single peakedness, and so forth. As suggested above, the implicit assumption about the reader's ability to pick up abstract concepts is drastically different here from what it is in the earlier chapters. The same

comment applies to the exposition of game theory in Chapter 9. The seventh and eighth chapters contain a workmanlike development of familiar results on monopoly, oligopoly, and monopolistic competition, distinguished by a good deal of attention to product differentiation and spatial competition.

The term "metastatics" in the title of the second volume refers to intertemporal analysis in the absence of uncertainty. Under this general heading Vickrey presents the elements of capital theory and Fisherian analysis, and discusses the classification of inventions, anticipated seasonal fluctuations when storage is possible, acceleration effects, and von Neumann-Morgenstern utility theory. Some of these topics are treated with less thoroughness and less emphasis on analytical methods than their importance would seem to justify, but the discussion is free of theoretical error and confusion, and has only minor faults of exposition.

Chapter 5 is a good introduction to money, the demand for money, and the behavior of banks. The following chapter, in "Macroeconomic Variables and Concepts," is not at all satisfactory as an *introduction* to this subject. The emphasis on the conceptual difficulties involved and the arbitrary nature of many of the definitions in any practicable system of national income accounts is overwhelming, and the reader does not learn how these problems have been handled in the Department of Commerce or any other system of accounts.

Chapter 7 covers the traditional topics of macroeconomic statics. The discussion begins with classical views on the mechanisms that should produce full employment, proceeds from a critique of these views to the development of a Keynesian model, and finally to an integrated model that may be rendered as $S(i, y) = I(i, y)$ and $L(i) \cdot y = M$. The latter is presented diagrammatically and briefly put through its paces with schedule shifts and monetary and fiscal policy. The particular form chosen for the demand for money relation gives the integrated model a more classical flavor than when transactions and speculative demands are treated as additive, but the substantive difference is small. There is the barest mention of real balance effects. In contrast to most of the material in these volumes, this chapter is neither particularly good nor particularly bad.

The last three chapters of the second volume are grouped together under the heading "The Dynamics of Changing Expectations." Topics covered include cobweb phenomena, Hicksian trade cycles, the elasticity of expectations, Harrod-Domar growth theory, and the problems of accurate public forecasting and open public policy. Particularly noteworthy are the attention given to a fairly elaborate Hicksian cycle model (with specific quantitative relations supplied and worked out for 131 periods), the interesting discussion of the problem of open public policy, and the utter absence of any reference to neoclassical growth theory *à la* Solow, Swan, Tobin, *et al.*

Five chapters of the micro volume have mathematical appendices. These do not attempt a complete development of the main elements of price and allocation theory, and are probably somewhat too concise to be of much value to the individual encountering this sort of material for the first time. In the appendix to the third chapter the utility maximization problem of the consumer is incorrectly formulated as a maximization problem both in quantities consumed *and*

in the multiplier associated with the budget constraint. This will not do, especially since the budget constraint is treated as an inequality and the possibility of satiation admitted. In the correct formulation, the Lagrangean expression is *minimized* with respect to the multiplier.

Several of the chapters in both volumes have exercises appended for the reader. These appear likely to yield large gross returns, but the return over cost will probably appear small to the students. It is interesting that no exercises are included that involve manipulation of a statical macroeconomic model, but exercises *are* provided in which the student is expected to carry through the numerical calculations for the elaborate Hicksian cycle model! Both volumes have indexes in which errors of omission have been effectively minimized. (Sample selection from the macro volume: "Brick, as money base," "Brouwer's fixed-point theorem," "Rabbits in Australia," "Shoplifting in the national income.") There is a lengthy list of readings and references, arranged by topics within chapters, at the end of each volume. The arrangement by topics is not an adequate compensation for the absence of footnotes in the text proper; there are a great many points at which the reader deserves more direct guidance to alternative or supplementary discussions. Apart from sections that treat numerical examples or explain complicated diagrams, the writing is fluid and enjoyable to read.

It is a rare course in economic theory that cannot be improved by incorporating portions of these books on the reading list. They are highly recommended for highly selective use.

SIDNEY G. WINTER, JR.

University of California, Berkeley

The Sources of Increased Efficiency—A Study of DuPont Rayon Plants. By SAMUEL HOLLANDER. Cambridge: M. I. T. Press, 1965. Pp. xii, 228. $10.00.

This book is concerned with technical change: what it is, how it comes about, what its characteristics are, and how important it is. In the first chapter, Professor Hollander reviews the literature that treats technical change as a shift in the aggregate production function, and notes some of the shortcomings of this approach. He agrees with Domar that this concept of technical change is a catchall, or residual. The remainder of the book consists of a detailed examination of this residual in a single industry: rayon manufacture.

Technical change is defined as "the introduction or the spread of cost-reducing changes in technique" (p. 4). The author then asks, "To what extent can changes in the productivity of the firm's inputs be ascribed to 'technical change' in the narrow sense?" (p. 21). As the problem is defined, changes in input productivity must be due to technical change, economies of scale, or changes in the quality of inputs. As the author gives little attention to input quality, much of the book is concerned with apportioning increases in productivity between technical change and economies of scale.

Hollander does not use the usual reversibility criterion for distinguishing between the two. Rather, he confines economies of scale to a plant-expansion effect: the spreading of overhead costs over a larger level of output. The use

of more efficient methods of production at larger levels of output is regarded as technical change, although most economists would term this economies of scale. On this basis, Hollander finds that technical change explained between 35 and 100 per cent of the reductions in unit cost in the plants in his sample.

Technical change is subdivided into indirect (innovations intended to increase volume) and direct (innovations designed to lower the cost of producing a given volume). His results show indirect technical change to be more important than direct. This probably reflects the fact that output was expanding rapidly in the period under consideration. It is difficult to see what other significance attaches to this finding.

A further distinction is made between major and minor changes in technology. A major change is one considered "difficult" prior to its introduction. On this basis, minor changes were found to predominate. But the reader is not told why major technical changes that have little effect on unit costs are introduced, despite their difficulty. Why isn't R and D effort channeled into the development of further minor changes in technology, some of which have an appreciable effect on cost? The author provides a partial answer by explaining that the rate of introduction of major innovations limits the rate of minor technical change. But in this case, of what interest is the distinction?

Hollander examines also the relative importance of embodied and disembodied technical change, and finds that 80-90 per cent of technical change was embodied. He argues also that replacement investment was more important than expansionary investment in leading to improvements in technology. This might suggest that plant managers are more likely to introduce new techniques when they are replacing obsolescent equipment than when they are adding to their capital stock. Alternatively, however, the result could simply mean that the timing of replacement investment is related to the availability of new technologies. In the latter case, the author's distinction has little meaning. Moreover, even if the former interpretation is correct, then the observed importance of replacement investment is no doubt simply a reflection of the importance of output-increasing technical change.

Finally, the author considers the sources of innovation, and finds that minor innovations tended to be developed internally, whereas major innovations tended to be developed in the research laboratory. One wonders whether this last point might in part reflect circular reasoning, considering the way in which major and minor innovations are defined. Is it not more likely that an innovation arising out of the work of a formal research unit would be regarded as "difficult" while an innovation developed on the spot would be regarded as "easy"?

This book is important as a case study of technical change at the micro level. However, it is a bit heavy on detail. Two-thirds of the book consists of a blow-by-blow account of changes in technology over time in each of five plants; one wishes that this material were presented in summary form. At the same time, the study could usefully have included a great deal more economic analysis, as well as further discussion of the policy implications of the author's findings.

Institute for Development Studies B. F. MASSELL

A History of Marginal Utility Theory. By E. KAUDER. Princeton: Princeton University Press, 1965. Pp. xxii, 247. $6.50.

This is a strange book. For one thing, it merely sketches the early history of utility theory and gives a somewhat disjointed account of recent developments, concentrating almost exclusively on the Austrian marginalists in the conviction that "the members of the Viennese group have probed deeper into the value problem than other economists." For another thing, while dealing solely with the logic and structure of marginal utility theory, it is full of expressions of alarm about the decline of marginal utility doctrine in our own times, insisting that "the neglect of marginal utility would deprive the theorist of an essential tool needed for economic analysis." And yet the entire argument of the book is conducted without any reference to the actual economic content of marginal utility theory: "the price theory based on marginalism has been intentionally neglected." The author claims that "the essential achievements" of the theory are "a thorough analysis of consumer behavior and some valuable insights into the demand-structure." But this claim is hardly substantiated by his account of the interminable Austrian discussions over the fine points of household planning. In the final paragraph of his book, the author concedes in a magnificent understatement that "during the whole history of marginal utility, the theorists have not always checked their findings against observed cases and statistical evidence. In natural science every discovery is tested. Why have we done so little?" If this rhetorical question had appeared at the beginning of the study, instead of at the end, a different and, I cannot help feeling, a more useful history would have emerged.

Marginal utility theory up to the introduction of the problem of choice under uncertainty by von Neumann and Morgenstern was fundamentally concerned with the rationale for downward-sloping demand curves, and it cannot be understood except in relation to the demand behavior that it attempted to explain. All along there were theorists, such as Cournot and Cassel, who employed demand functions without worrying about a utility substructure. Most economists rejected this positivist approach that seemed tantamount to throwing away information derivable from introspection. Since demand curves cannot be simply observed, it was hoped that the specification of behavioral assumption—and that is all that utility theory is—would add information on the nature of demand functions. And yet the long and tortuous history of utility theory presents a disheartening picture. Few of the sponsors of utility theory were interested in testing the behavioral implications of the theory and, indeed, on the whole utility theory was not a fruitful source of hypotheses about demand. The work before us confirms Stigler's conclusion in his brilliant paper "The Development of Utility Theory" (*Journal of Political Economy*, August and October 1950) that the practical upshot of fifty years of utility theorizing was nothing more than the proposition that "if consumers do not buy less of a commodity when their incomes rise, they will surely buy less when the price of the commodity rises."

The book opens with a chapter on the delayed acceptance of marginal utility theory in the eighteenth and nineteenth century, developing the thesis that Catholic-Thomistic culture favored, while Protestant-Puritan cultures resisted,

its development. The argument does not go beyond the author's earlier statement (*Quarterly Journal of Economics*, November 1953) and J.P. Henderson's cogent criticism of the thesis (*ibid.*, November 1955) is not answered or even mentioned. Bernoulli, Bentham, and Gossen receive generous treatment as pioneers of marginal utility theory, but of the three founders of modern utility theory, Menger alone is examined in detail. Indeed, the chief contribution of this book to the history of economic thought is the light which it throws on Menger's intellectual development: the author examined Menger's personal library deposited at Hitotsubashi University and discovered a large number of valuable annotations and notes in Menger's own hand.

The last half of the book is devoted to the development of marginal utility theory between 1880 and 1947, organized around certain central topics that appeared again and again in debates, such as the definition of rationality, the relation between costs and utility, the validity of a law of diminishing marginal utility of income, the measurability of utility, and the like. The author's exposition of these issues lacks analytical clarity and at times is downright confusing. For example, the discussion over the imputation problem is presented without a single reference to the fact that the Austrian writers dealt exclusively with the case of fixed input-coefficients and, hence, could not resort to marginal productivity considerations. And to put game theory in the same chapter as Wieser's theory of imputation, on the grounds that both theories are concerned with the division of a fixed sum among a number of participating agents, is hardly calculated to illuminate the theoretical issues involved.

M. Blaug

University of London

Economic History; Economic Development; National Economies

The Conquest of the Material World—Essays on the Coming of Industrialism. By John U. Nef. Chicago and London: Chicago University Press, 1964. Pp. xii, 408. $8.95.

It is a pleasure to be able to reread consecutively the series of beautiful essays Professor Nef has published over the last thirty years. They contain solid and purposeful research, original ideas, and stimulating polemics. Since the individual essays first appeared as self-contained pieces, the volume is in some measure repetitious; but this should not bother the reader, as the restatements help to emphasize the unifying thesis which runs through the book: the significance of the industrial upsurge in England during the hundred years between 1540 and 1640, which the author describes, somewhat waveringly, either as the "major rehearsal" for the "elder Toynbee's industrial revolution" which began in the last decades of the 18th century or as the "real" start, of which the later development was but a natural and not more impressive continuation.

The collection is introduced by an essay on "Mining and Metallurgy in Medieval Society" which is an illuminating and far-ranging piece, even though in

his eagerness to stress the rising dominance of the Continental Renaissance governments over the extraction of metals Nef tends to play down—unjustifiably and unnecessarily—the immense contribution made by South-German capital, not just to the financing, but to the technological progress in mining and metal-making. In the concluding part of the volume, the essay on "Genesis of Industrialism and Modern Science" makes some interesting, though perhaps somewhat superficially treated, points on the different position of science in England and in France, and particularly on the differences in orientation of French and English mathematicians in the 16th and 17th centuries; however, not much more emerges than the appearance of mere simultaneity, rather than of any intimate connection, between progress in industry and science of the period. Not everybody will be able to appreciate the alternatives of the epilogue with its assertion that religious faith is more important than the fruits of scholarship for "the things that are closest to our hearts" and that the danger of pseudoscientific beliefs is greater than that of dogmatic perversions of true religion. These pages will be read with respect as an expression of deeply felt personal views and of the urgent need for simple absolute truths in a world of relativism where any one of the very uncivilized "civilizations" and very uncultured "cultures" is readily regarded as a civilization or a culture. But the relation of those things to the essays is tenuous, save perhaps for Nef's sense of loss of spiritual and aesthetic values that occurred in the process of "conquering the material world."

The heart of the book lies in the comparison between English and French industrial growth from 1540 to 1640. The main fact of a rapid spurt in England, centering upon mining and "heavy industries," is very convincingly established. The attempt to assess the quantitative differences in the rates of growth and volumes of per capita industrial output is somewhat less successful. Although a good deal of factual material is adduced, it is on the whole impressionistic (especially for France), so that very precise questions receive rather tentative and imprecise answers. The clearest case for England's advantage over France is that of coal output, an area that no one is better qualified to treat than Nef, the author of a classic monograph on the subject. With regard to other branches of English industrial endeavor, modern research has arrived at less impressive results. On the whole, it appears very likely indeed that, during the period, England's industry grew at a faster clip than that of France. Whether, as Nef believes, English industrial output by 1640 came to equal that of France is much less certain.

In interpreting his results, Nef is rather skeptical about attributing much importance to the spiritual effect of Protestant doctrines. He sees the main effect of the Reformation upon growth in the dissolution of monasteries, which transferred lands, rich in coal and iron ore, into private hands that were eager to exploit the resources, as well as in a basic change in the nature of demand. The demise of the Roman Catholic Church reduced the demand for "things of beauty" and created a "vacuum" filled by the new demand for utilitarian goods. This change in general economic orientation from "beauty to utility," from "quality to quantity" Nef regards as crucial for the emergence of modern industrialism and considers it more revealing than any measurable dimen-

sions of industrial progress. Incidentally, while Nef the scholar continues pounding on the significance of the change, Nef the individual keeps deploring it, and at one point even raises the remarkably inappropriate question as to whether the beauty of a modern automobile can stand comparison with a medieval cathedral, a painting of Botticelli's, or a play of Shakespeare's.

Nef's well-known attack on the Hamilton-Keynes thesis, "Prices and Industrial Capitalism in France and England," has been included in the present volume, and rightly so since it, too, deals with the origins of "industrialism" and has an important bearing upon the comparisons of industrial growth in the two countries. The thesis claimed that the influx of precious metals and the resulting price revolution with its lag of wages (the "profit inflation") were largely responsible for the economic upswing of the period. Nef questions the validity of this monetary explanation, just as earlier in the book he was much too reluctant to attribute the depression of the 14th century to the exhaustion of easily accessible silver mines on the Continent. Nef certainly succeeds in showing that the picture was much more complex than Keynes presented it in his *Treatise on Money*. As Nef argues, the indices used by Keynes tended to exaggerate the fall in real wages. Some portion of the laborer's wage was paid in kind; some of the laborer's foodstuffs came from his own plots of land; the soaring timber prices may have raised the cost of production above the general level of prices; bread prices did not rise as much as grain prices; and, above all, France, which showed roughly the same degree of profit inflation, did not experience an upswing comparable to that of England. It would seem that Nef has been indeed able to cast some doubt on the thesis, but has hardly succeeded in refuting it. Some of his points are telling, others are obviously weak. If prices of bread—and beer—rose somewhat less because (as Nef claims) of cost-reducing technological improvements, this would mean that the wage earners' levels of consumption fell less than Keynes's figures indicated, but it would also mean that it had become possible to sustain a larger number of industrial workers on a given quantity of staple foodstuffs. And it still remains to explore to what extent the new technology in milling and brewing, as indeed in industry at large, was not the result of investments first induced and then increasingly rendered possible by the "profit inflation."

Moreover, only the *extent* of the fall of real wages is in dispute. The fact itself is generally accepted. The available information shows that prices of cereals rose more than those of converted products, a natural enough effect in conditions of falling real wages. Nef comments on this circumstance by saying, "As the price of bread apparently increased more than that of other foods, it is possible that the poor replaced bread, cakes, and porridge to some extent by other kinds of nourishments, such as herrings, beef, mutton, eggs, cheese, and small beer" (p.253). This statement, which gratuitously disturbs the ghost of Marie Antoinette, almost certainly reverses the causal nexus. During the period, inferior goods showed higher price rises not only within the whole group of foodstuffs, but also within the group of bread grains (rye versus wheat, which incidentally may explain the plantings of rye on less suitable soils under the late Tudors); and elements of this pattern can be observed all over Europe from the shores of the Atlantic to the plains of Russia. In fact, it

s extremely rare that an economic historian can travel so far and so safely on o simple a hypothesis.

As far as the comparison with France goes, Nef does tend to shift the argunent when he claims that the lower rates of growth in France despite a similar legree of "profit inflation" prove that the price revolution did not *inevitably* ead to a commensurate spurt of industrial development. Inevitability is not in operational concept in historical research and the thesis was not argued in hese terms. There are, however, excellent reasons why England could utilize he opportunities afforded by the price revolution more effectively than France. There is the absence of the religious wars that continued in France intil the last decade of the 16th century. More important, however, is the basic fact of England's relative economic backwardness at the start of the period. As Nef himself points out, England's "concentration of capital in mining and metallurgy from 1540 to 1640 was stimulated by the application of technological processes introduced with the help of skilled foreign artisans" (p.132); the blast furnace probably came to England from France (p.177). And the area of productive borrowings from abroad was not confined to mining and metal making. France, much more advanced, had no comparable possibilities at her disposal. Similarly, the English stress on "utilitarian" goods, of which Nef rightly makes so much, is in general a characteristic of modern industrializations in conditions of backwardness. At any rate, such differences in the rate of growth as existed between England and France are unlikely in themselves to detract from the Hamilton-Keynes thesis. As far as the latter is concerned, we now have some studies dealing with the problems involved, and a great deal more research is necessary which will have to utilize very thoroughly for a larger number of countries the mass of price information which has been collected since the publication of Nef's paper nearly 30 years ago. But whatever the final judgments, there is no doubt that all such research will greatly benefit from the probing questions raised by our author.

ALEXANDER GERSCHENKRON

Harvard University

Postwar Economic Growth: Four Lectures. By SIMON KUZNETS. Cambridge, Mass.: The Belknap Press of the Harvard University Press, 1964. Pp. 148. $4.25.

This little book contains Simon Kuznets' Haynes Foundation Lectures of 1964. It is a book about postwar economic growth viewed in the very broadest perspective. It is concerned, of course, with the pace of postwar growth and its more narrowly economic causes. It is still more concerned, however, with the wider conditions and concomitants of the growth process. It therefore reaches into the political and institutional arrangements under which contemporary growth is taking place and considers the consequences which probably flow from the arrangements which have been either necessarily or fortuitously adopted. In developing his very wide themes, Kuznets brings to bear something like the full range of experience relevant to modern growth, that is, the

historical record of the developed countries going back into the nineteenth century and the international diversity present today in the developed noncommunist countries and the developed communist countries as well as in the numerous underdeveloped countries still trying to launch themselves into the growth process under a variety of institutional forms.

The variety of issues on which Kuznets at least touches matches the range of experience he considers, and it would be quite impracticable—as well as uninteresting—to try to catalogue them. A recurrent objective, however, runs through the book. This is to trace the consequences of heightened interdependence among people and nations in a world rendered increasingly diverse by economic growth and by the forms of organization adopted to achieve it. The potentialities of both cooperative interchange and violent conflict have been dramatically enlarged by progress in the means of transportation and communication. And while the desire for economic growth and the efforts to foster it have become more intense and worldwide in range, the incidence of growth has been very uneven, and the distribution of people with respect to levels of living and productive power has tended to widen rather than to narrow. To this important element of diversity, one must add another. The systems of social and political organization through which nations seek growth have also become more diverse. The question Kuznets appears to pose, therefore, is this: are there in the growth process, with its concomitant tendencies to interdependence and diversity, the potent seeds of domestic and international strain and conflict? Kuznets' answer is yes. His view of the matter, all too briefly and categorically stated, runs somewhat as follows.

The desire for modern growth and for the material rewards it promises has now spread to all peoples. Modern growth, however, involves a familiar, but nonetheless radical, transformation in the economic and social structure of countries, a transformation which goes on as long as growth takes place. To carry through the required transformation on a sustained basis over many years demands a government sympathetic to the expansion of economic activity and strong enough to sanction, protect, and cushion the many readjustments of social relations implied. Such governments have hitherto been national in character and nation-states have proven, so far, to be the only practical and effective units capable of organizing the necessary apparatus of power and decision. The spread of the desire for economic growth has, therefore, been matched by the proliferation of independent nation-states.

In principle, it might be that nation-states and their national governments could work together in harmonious cooperation. Concomitant influences, however, some of them intimately involved in the growth process itself, decree otherwise. Modern growth started in one country, was taken up by a few others and then spread in widening circles throughout the world. In short, the time of entrance of different countries into the process was uneven. Great disparities have, therefore, resulted in the pace of economic advance of different countries which, sustained over several decades, successively revolutionized the balance of economic and military power. The countries that were advancing understandably wished to exploit the advantage offered them by a new balance, while others, just as understandably, attempted to resist the change. Wars

were, and are, a natural consequence, and the occasions for war are only multiplied by the readjustments of political spheres implicit in the break-up of colonial empires, a break-up itself required by the need to establish national governments as a basis for economic growth.

Not only war, but world war of unprecedented destructive power, appears to have been a concomitant of modern growth. The great progress in transportation and communication which intensified the economic ties among countries also tended to create large blocs of interdependent countries. Capital flows required to promote growth among laggard countries made for relations of dependence. The same progress in transportation and communication, combined with much higher productive power, meant that when war came it could involve large numbers of men and great volumes of equipment sustained in combat at distant bases for long periods.

Finally, Kuznets notes that while national governments have hitherto proved to be a prerequisite for growth, governments organized on a variety of systems and enforcing a variety of forms of political and economic organization appear to be compatible with growth. We, therefore, find national governments presiding not only over democratic capitalist systems but also over communist dictatorships or party oligarchies, as well as other varieties of organization. The conflict of rival systems then enters to complicate and aggravate the conflicts inherent in adjusting political influence to a changing balance of economic power.

Kuznets does not present his views as baldly and schematically as I have done, and my statement may do him an injustice. In any event, it is clear that he does not see economic growth as the sole source of domestic and international strain and conflict. There are other sources of divisive nationalism besides economic growth and other sources of war between national states besides shifts in the balance of economic power.

As already said, the recurrent theme sketched above is only the leading motive in a score rich in other material. To mention only two: there are suggestive discussions of the costs and consequences of World War II in their bearing on economic growth, and there is a brief, but balanced, presentation of alternative interpretations of the generally rapid postwar growth rates and of the contrasting postwar growth records of different countries.

These lectures afford economists a chance to become better acquainted with Kuznets in a less familiar mood, not in his empirical, but in his interpretive aspect. Few economists dare to leave the borders of economics and the security of statistical measures. Few who do can bring to their excursions Kuznets' historical perspective and imaginative reach, and these lectures suggest that the man who has given us so many solid statistical monographs is equally at home in the speculative essay.

MOSES ABRAMOVITZ

Stanford University

The Economics of the Developing Countries. By HLA MYINT. New York: Praeger, 1965. Pp. 192. $3.00; paper, $1.95.

The book-length literature in economic development has tended to divide

into two very different classes: into compendia of economic analysis, often speciously sophisticated, whose purpose is as much to plead the theoretical respectability of the field as to tackle the real problems of development; and into popularizations, usually empty of analysis, which move monolithically to the conclusion that the problems either are practically intractable or really require no more than some simple reorientation. Refreshingly, Professor Myint's work avoids both these extremes, and while brief, it is comprehensive, beautifully written, and analytically sound.

Its brevity, however, somewhat limits the audience for the book. Its very conciseness makes the problems appear too simple and the conclusions too obvious to permit its use as a primary text, though parts should prove useful supplements to both graduate and undergraduate reading lists. On the other hand, little will be learned by the development economist who has long since assimilated Myint's seminal articles of the 1950's and has kept up with recent controversy. The most appreciative reader undoubtedly will be the general economist who would like to know what this field called "economic development" is all about and where current thinking in it is leading.

The book consists of two quite distinct parts. In the first half, Myint reviews the process by which the African and Asian countries were absorbed into the world economy and the reasons why they failed to achieve steady growth. These chapters are basically a synopsis of Myint's three famous "backwardness" articles (*Oxford Economic Papers*, 1954; *Review of Economic Studies*, 1954-55; and *Economic Journal*, 1958) and will solve for many a teacher the dilemma as to which article to assign his students. Now as before, Myint deals chiefly with the "semi-empty," as opposed to the initially overpopulated, countries and focuses primarily on the growth of export crops and of foreign-dominated mines and plantations. Two welcome extensions of the earlier writings are the greater concern for the monetary aspects of the process and the clearer picture of the time-dimensions. Not so welcome is the greater emphasis on the "cheap labor policy" of foreign entrepreneurs on which Myint puts most of the blame for the failure of the "opening-up" to lead to sustained growth. Certainly he goes too far in suggesting that the vastly higher rates of growth in North America and Australasia were caused by their "conventional standard of high wages" (p. 57).

The next four chapters of the book are largely a critical analysis of what Myint feels are the false shibboleths of development policies. One by one, he attacks the relevance of the disguised unemployment concept, savings-oriented growth theories, the adequacy for planning of the aggregate capital-output ratio, the critical-minimum-effort thesis, the need for a big-push, balanced growth arguments, the "self-destroying" nature of deficit finance, the declining world demand for primary products, and the desirability of across-the-board protection for domestic manufacturing industries. Needless to say, with so many targets and only 80 pages for all, but a few lashes can be devoted to each. It is inevitable that some of the criticisms are now well known, the more so since Myint's views are closely attuned to recent trends in development literature. Nevertheless several of the strokes are remarkably well directed. Readers with stomach left for the balanced vs. unbalanced growth controversy

will find Myint's classifications of the different strands of the theory particularly lucid, and his discussion of trade policies is excellent.

Although the book is marvelously coherent and instructive throughout, the reader will probably feel disappointed at the end on two counts. One, the early chapters on the failure of the "opening-up" to bring about sustained development in the African and Asian economies are not well integrated into the subsequent discussions of development policies. While knowledge of the causes of backwardness in no way insures knowledge of its cures, one would have hoped that Myint's unique insights into the historical process would have led him to specific policy recommendations, perhaps even have guided his entire thinking about development policy. And two, the generally critical tone of the later chapters makes it difficult to recognize Myint's positive contributions. While the final chapter is intended to "bring out the implications of my analysis" (p. 166), it in fact goes on to new material (i.e., the role of markets, educational policy, and foreign aid).

While *The Economics of the Developing Countries* offers little that is new, it is always useful to know where a man of Myint's stature stands. More important, the book is so concise, so clear, and so in tune with general thinking that it will provide a valuable benchmark of the state of economic development theory at the mid-1960's.

RICHARD C. PORTER

University of Michigan

Principles of Development Economics. By HENRY J. BRUTON. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. viii, 376. $7.50.

Textbooks on economic development tend to be full of anecdotes, colorful examples, peculiar special cases, and tables listing economic features of remote lands. Professor Henry J. Bruton has written a text without a single footnote, without a single table giving data on underdeveloped countries, and absolutely devoid of references to his travels abroad. What is he trying to accomplish?

Making some small modifications to the established body of neoclassical assumptions and analysis, Bruton tries to isolate key economic principles which are especially relevant to the "typical" underdeveloped country. Naturally, his approach will not please those economists who do not consider neoclassical economics and/or economic calculus relevant to underdeveloped countries, especially those who abhor the use of the production function, widely employed by Bruton throughout his book.

After a terse definition of the basic problem faced by developing economies (raising per capita income), the book proceeds to catalogue the main sources and mechanics of growth for any economy. This is done with the aid of the production function concept. Bruton's presentation of the neoclassical theory of production takes into account some of the criticisms levied against it. For example, he explicitly assumes that capital, once embodied in a machine adapted to a given technique, cannot be used in any other technique, and that only a few techniques of production are available to any single industry. His discussion of technological change also considers the possibility that techno-

logical breakthroughs achieved in labor-poor, capital-rich countries may force underdeveloped countries to choose between maximizing output or employment.

The first 80 pages of the book in effect provide the building blocks needed for growth models, although Bruton wisely refrains from presenting the properties of any specific model. Attention is given to the implications for growth of the existence of interdependent sectors, using the input-output table as a framework for the discussion. While a chapter is also included in this first part of the book discussing demand and pricing problems, the emphasis is on supply conditions throughout the volume.

After examining in general the sources and mechanics of economic growth, the author presents in one chapter the main features of a "typical" underdeveloped economy which accounts for its slow growth. This heroic attempt to summarize in about 30 pages the great variety which exists among backward economies is, of course, consistent with the emphasis of his book on economic principles instead of facts and figures regarding underdeveloped countries. Nevertheless, one wishes Bruton had included some sort of typology of underdevelopment, to make the student aware of key differences which exist among underdeveloped countries and the influence which such variety may have on the analysis.

Part II of the book is devoted to capital formation. This section starts with an explanation of "the optimal saving problem," and later on analyzes ways in which capital formation may be increased. These include the mobilization of unemployed labor, more effective use of existing capital, capital inflows and, of course, increasing saving. In this part of the book Bruton introduces some of his favorite themes and policies: the importance of an effective taxing machinery; the need for flexibility and "capacity to transform" in the economy; and the high potential pay-off awaiting a better allocation of existing resources. The sections dealing with the possibility that an economy may lack the transformation capacity to exploit fully its saving propensity are especially clear and well written.

The economics of the accumulation of technical knowledge and labor skills receive as much attention as the economics of the accumulation of physical capital. Part III of the book discusses these other possible uses of investable resources, treating the accumulation and application of knowledge as well as the accumulation of labor skills as just another economic process to which production function reasoning can be applied. The author admits that in these fields such reasoning becomes harder to quantify and apply, but argues that an attempt to use economic rationale is imperative lest decisions in these fields be left in the hands of politicians. In spite of one's sympathy for such a position, it must be granted that the "marginal return should be equated with marginal costs" advice flowing from the analysis tends to become formalistic and empty. In spite of this criticism, the more than 60 pages devoted to the two chapters dealing with technical knowledge and the training of labor contain much that is stimulating and instructive. The presentation in these chapters is generally straightforward; however, the discussion of the criteria for replacement of machines under conditions of technological change (pp.

197-200) is rather confused. After pointing out that sunk costs do not affect the replacement decision, the author goes on to use fixed costs as an element in the determination of when machines should be replaced when there are prospects of new and more productive machines being available in the future.

Part III of the book also includes chapters on the social environment of economic development and on population. It is argued that no single environmental feature is either essential or is an insuperable obstacle to the achievement of development, and emphasis is again placed on the need for flexibility if an economy is to exploit opportunities which are open to it. Bruton's views on population are strong, perhaps due to his experience in Asia: he claims that a conscious policy aimed at reducing the birthrate should be part of the development program. A typology of underdevelopment would have been useful in showing that for countries with relatively high resource/labor ratios (such as some Latin American and African countries), such a strong statement may be misleading.

Part IV of the book devotes two chapters to examining the criteria for the optimal allocation of investable resources. The emphasis is on showing that no simple rule of thumb will yield optimal results, and that one should fall back on maximizing the social marginal productivity of investments, modified to take into account all possible side and indirect effects, which for this purpose should be quantified as accurately as possible. The author explicitly asserts that no generalization is possible regarding the optimal sequence of investment by sectors of the economy (such as first agriculture, then light industry, etc.).

Bruton's approach to the economics of development becomes even clearer in the last part of his book, which deals with money and finance and strategies of development. Popular catchphrases and issues, such as the take-off, industrialization vs. agriculture, big push, etc., are criticized as highly simplified views of the problem of growth and as harmful to the understanding of the development process. The usual economic calculus, the author feels, is enough to tackle development problems without the need of grand generalizations. The discussion of the relation between inflation and economic development is lucid and well balanced, and avoids getting involved in the sterile debate between "monetarists" and "structuralists."

Bruton's approach will train the student to deal in a logical and careful way with the many false issues and misleading simplifications found in the field of development. In this respect, this text is to be welcomed most enthusiastically. However, rigor has a price, and in this case the price is absence of liveliness and occasional lapses into rather empty formalism. The book does not read easily and lacks the wit and charm of, say, Kindleberger's book on the same subject. At times Bruton seems perversely intent on making the book unnecessarily forbidding. Thus, in discussing input-output, instead of labelling his sectors as "Agriculture," "Industry," etc., he uses general notation which is likely to terrify an undergraduate. On page 12 he starts a discussion of the meaning and measurement of capital which may satisfy Mrs. Robinson, but which will leave the undergraduate a bit baffled. Perhaps a few footnotes with quaint anecdotes would not have been so bad after all.

Even an author so fully committed to presenting bare theoretical bones as Bruton is, cannot, when writing a text on development, wholly put aside his favorite policy biases. At several points in the text, statements in support of some policies are made without much elaboration as to their rationale. Thus, in the middle of a chapter on demand and pricing problems, it is stated that: "The achievement and maintenance of export markets may depend to a very great degree upon the ability of the developing country to provide the world with new products" (p. 68). In the chapter on "the nongrowing economy" it is flatly asserted that: "To parcel out titles to plots of land to individual peasants who have no access to credit, seeds, fertilizer, or water except from their landlord will surely create nothing but chaos" (p. 118). Two more examples of *obiter dicta:* "But control and red tape do not seem to have the economizing effect that simply a high price has. Also, controls and red tape are frequently best tolerated by persons least concerned with productive investment" (p. 138). "(Indeed one may go further and assert that improving the tax system is the single most important task confronting the government of the underdeveloped country)" (p. 155).

Foreign trade is regarded throughout most of the text as just one more line of economic activity of the underdeveloped economy, with no specific peculiarities. Bruton deems that exhorting the underdeveloped economy to increase the transformation capacity of its economy and to set the price of foreign exchange equal to its social opportunity cost is all that needs to be said to a country complaining about foreign exchange shortage. The optimal pattern of foreign trade can be determined in the same way as allocation within the economy, naturally using taxes and subsidies to eliminate divergences between private and social costs and benefits. This handling of foreign trade highlights the virtues as well as the limitations of this text. It provides an excellent general framework for the analysis of development problems, but is likely to leave the student a bit cold and uninformed about several policy questions and historical patterns of trade and growth. Besides this skimpy treatment of foreign trade, the text discusses only briefly issues related to foreign aid.

A substantive criticism may be made of one of the themes of the book. In several places Bruton points out that in underdeveloped countries it is common to find overvalued exchange rates and interest rates which are below the equilibrium rate. He asserts that this leads to misallocation of resources by encouraging the use of "too many imports" (p. 106) and of capital intensive techniques. Yet, the fact that lucky holders of exchange permits and beneficiaries of the subsidized loans of state banks can get part of their foreign exchange and credit needs at rates below those which rule in competitive markets does not necessarily lead these entrepreneurs to use them as if they had unlimited access to the subsidized resources. Indeed, they are likely to be fully aware of the true scarcity value of these rationed resources. Thus, while controls may create windfalls for the groups favored by the authorities, it is doubtful that they will induce the favored entrepreneurs into using "too many imports" or "too much capital." In fact, of course, overvalued exchange rates lead to "too few" imports in the long run via their impact on exports; this is certainly a misallocation of resources, but not of the type described by Bru-

ton. The policy of rationing foreign exchange and credit by using controls rather than equilibrium prices could even be partially defended on the grounds advanced by Bruton in his discussion of the policies required to induce the most "suitable" form of technological change in underdeveloped countries. As he points out (p. 193), it is well known that if the supply of all factors is perfectly elastic to the individual firm, there will not be an incentive for these firms to give any bias to their technological change. Rationing of foreign exchange and of credit would thus help to give a bias to technological change in favor of capital and foreign exchange saving, presumably the "right" type of bias to have in the underdeveloped economy.

The over-all judgment on this book depends very much on the views of each teacher on the best way to introduce the student to the economics of development and on what development economics should be all about. In spite of the minor criticisms made above, the reviewer's biases on these issues are very similar to Bruton's, and thus I would consider his book one of the best available texts in the field. The teacher using it will have the additional windfall of holding a monopoly on anecdotes and colorful examples.

CARLOS F. DIAZ ALEJANDRO

University of Minnesota

The Morphology of Greek Industry—A Study in Industrial Development. By GEORGE COUTSOUMARIS. Research Monograph Series No. 6. Athens, Greece: Center of Economic Research, 1963, Pp. viii, 430.

The Center of Economic Research in Greece was established to provide: "(1) basic research on the structure and behavior of the Greek economy, (2) scientific programming of resource allocation for economic development, and (3) technical-economic training of personnel for key positions in government and industry." *The Morphology of Greek Industry* is a policy-oriented, empirical investigation of the structure of Greek industry, stimulated to a considerable extent, it appears, by the association of Greece with the European Common Market and the policy problems involved in development within that framework.

The book is divided into three principal parts, with an appendix of 54 pages of statistical tables. None of the main parts is entirely morphological; some attention to the performance and functioning of industry as well as some description of government policies toward industry are interwoven throughout the book. Part I, however, is largely statistically descriptive, with chapters dealing with the number of producing units by industry, output and employment size distributions, characteristics of industrial demand, locational patterns of industry, investment and capital-labor factor mixes, cost structures, and forms of business and financial organization. Part II deals with economic performance as measured by rates and patterns of growth, factor productivity and factor returns, and indicia of scale economies. Part III views the development outlook including an assessment of the probable effects of the Common Market in Greece, and some policy conclusions.

The detailed statistical findings cannot be indicated here. Greece, after

rapid industrial expansion between 1923 and 1939, has a manufacturing sector which accounts for less than 20 per cent of its gross domestic product. The industrial structure has in general changed little since 1930 and is characterized in most industries by small manufacturing establishments and, especially in textile, clothing, and footwear production, by large numbers of home-establishments. Only three-tenths of one per cent of the manufacturing establishments had 100 or more employees in 1958, and multiple establishment firms are rare in most industries. It is concluded that manufacturing suffers from "considerable diseconomies of scale" and that horizontal mergers would lead to greater efficiency.

Household demand for manufactured goods, measured with cross-section data, is found to be generally income-inelastic, though the elasticity between manufacturing output and net national income, based on time series, is income-elastic. The author argues that because of these elasticities, the expansion of industry will depend on developments in producer inputs and capital goods which might reduce costs and prices for "wage goods" and, in turn, release demand for the expansion other sectors.

Industries which are "raw-material oriented" (e.g., food processing) are geographically dispersed while those which are "market oriented" concentrate in a few urban centers—Athens in particular. George Coutsoumaris is critical of government policies favoring decentralization because of their failure to take account of optimal location patterns and to coordinate the over-all development planning.

Capital-output coefficients are typically higher than those for U.S. industries, a phenomenon which is said to reflect underutilization of capacity and high inventory requirements of an inefficient marketing system. Capital-labor ratios and ratios of the wage bill to capital amortization, interest, and profits are computed also, but few conclusions are drawn from them. Following a description of the asset composition of firms and sources of investment funds, Coutsourmaris provides an interesting analysis of capital markets and the extent of corporate enterprise, indicating several impediments to the growth of larger-scale enterprise and to development generally. That larger enterprises, the development of external economies (e.g., power, transportation), and improved tariff policies are essential is supported by studies of cost structures and interindustry relationships.

Part II, "Economic Performance," places these findings in historic and international perspective. It is concluded, perhaps too optimistically, that Greece is "on her way toward a second stage of industrial development in which manufacturing output approaches a balance between consumer and producer goods." But changes have been slow because of narrow markets, political and economic uncertainties, and inappropriate government policies. Productivity, while rising, is low in absolute terms due to scale inefficiencies, the use of inferior technologies, an inefficient industrial organization, inadequate managerial resources, government intervention, and market imperfections.

In the concluding part, the outlook is presented in terms of projections based on recent experience with national product growth rates and the relevant income-elasticities. It is then noted that entry into the Common Market will require growth in areas in which comparative advantage exists, and these

are held to be material-oriented industries, labor-intensive, transport-extensive industries, regionalized industries, and intermediate-product industries. The size of the market for some Greek industries should thus grow; others will experience increased foreign competition and probable contraction. Greek labor, which has already flowed to European markets, could be joined by Greek capital seeking higher returns, and both could have retrogressive effects. Association with the Common Market, that is, makes the demands for economic reorganization even more pressing. The short section on policy conclusions indicate steps to speed such reorganization, the nature of which is anticipated by the previous discussion of the sources of inefficiency.

Coutsoumaris has presented an admirable statistical tableau of the economic structure of his country, descriptions of its weaknesses and prescriptions for their cure. Yet reading it—and the recent news of political events—hardly leads to sanguine expectations.

ALMARIN PHILLIPS

University of Pennsylvania

Chile: una economía difícil. By ANÍBAL PINTO. Mexico City: Fondo de Cultura Economica, 1964. Pp. 184. 16 pesos.

Aníbal Pinto is a prolific Chilean who has published many books and articles on economic matters: e.g., *Finanzas públicas, Hacia neustra independencia económica, Chile: un caso de desarrollo frustrado,* and *Cuestiones principales de la economía.* He has been a professor of fiscal policy in the University of Chile; founder and president of the Chilean Economic Association; founder, director, and editor of the Chilean economic review, *Panorama económica;* director of the journal of the Faculty of Economics of the University of Chile; and one of the project directors of the Chilean Economic Institute. Along with all this, he has taken an active part in the work of the Economic Commission for Latin America of the United Nations.

In the United States, a person with all these accomplishments would, in all likelihood, be an economist. In Latin America, things are not that simple. Pinto was a law student when he fell ill and, like Ricardo at Bath, read a bit of economics for diversion. The subject interested him; he continued to read it; and finally obtained a year's scholarship at the London School of Economics. In this way he joined the sizable group of Latin Americans who are considered economists "autodidacta." Since Smith, Ricardo, and Harrod were also largely self-taught, the propriety of considering such a person to be an economist must rest on the quality of his work.

In assessing Pinto, the present work may be taken as typical. It began as a set of notes for some lectures on the Chilean economy from 1950 to 1963. The lectures were never given, but the notes became the basis for a broader study of the current problems in economic development and Chile's prospects for the future. Pinto uses the word "difficult" in order to get away from the semantic and ideological overtones of "rich" and "poor," "developed" and "underdeveloped." He insists (p. 84) that it is more sensible to agree that the Chilean economy is difficult than to argue endlessly about its richness or poverty. While accepting that the same can be said of many other economies, he points

out that the Chilean economy is small and has reached the end of the "easy stage" of import substitution, and that the Chilean social and political structure is responsible for all sorts of complications in the process of economic development, many of them unknown. The solution suggested is the discovery of a "more complicated model" of economic development which would combine "the complete transformation of the economy, a new type of 'opening' to the outside world, the achievement of greater autonomy in its workings, and—last but not least—a distribution of resources and income which would take into account and satisfy the aspirations of that sizable part of the population which has, up to now, remained at the margin of the development process in the last decades" (p. 10).

The problem posed is respectable enough, but Pinto's analysis is not quite adequate. The argument proceeds on a level of rigorous superficiality. Whenever things begin to get interesting, he throws up his hands and moves on to another point, either because the data for a proper discussion are lacking or because there "is not time" for a longer exposition:

> As the Institute study frankly states, one of the most serious omissions in the data is that referring to the changes in income distribution. The only reason for that is the refusal of the responsible institution, the Development Corporation, to release the official figures covering the years after 1954 (p. 40).
>
> Most of the data concerning industrial production do not lend themselves to a detailed examination of the problem (p. 46).
>
> With respect to the problem of relative prices, the global figures on hand do not help analyze the problem or shed much light on it, either (p. 48).
>
> It is not possible on this occasion to inquire in detail into the causes of this phenomenon [weakness of investment], the importance of which in the development process must be emphasized (p. 56).
>
> We cannot analyze the problem [sources of funds] in detail (p. 115).
>
> The problem [of stability] has been debated *urbi et orbi*, and we cannot consider all of the arguments about it (p. 116).
>
> It hardly requires excuses to confess that we cannot make any further progress in the analysis of these questions [foreign trade] (p. 128).

Then, when statistics are actually produced, they are presented in tables full of careless errors: units are cited when thousands of units are meant (p. 108); some columns have no headings at all and are quite meaningless (p. 48); while, in other tables, several columns have the same heading but contain different figures (p. 51). The text itself appears to have been dictated or written at high speed. It abounds in clichés and repetitions. But, once the reader understands that the author is not a trained economist or a prose stylist, the book can be read with profit. It presents the same sort of interesting, but hardly penetrating, description of a country's economic problems that magazines like *Vision* or *Newsweek* occasionally publish. They do nicely until a proper study can be completed.

I. H. OTTO

Monterrey Institute of Technology, Mexico

Economic Systems; Planning and Reform; Cooperation

Techniques of Economic Forecasting. An Account of the Methods of Short-Term Economic Forecasting Used by the Governments of Canada, France, the Netherlands, Sweden, the United Kingdom, and the United States (with an introduction by C. W. McMahon). Paris: Organization for Economic Co-operation and Development, 1965. Pp. 173. Paper, $3.75.

Quantitative Planning of Economic Policy. A Conference of the Social Science Research Council Committee on Economic Stability. Edited by BERT HICKMAN. Washington: The Brookings Institution, 1965. Pp. xiii, 279. $7.95.

Economic planning is a field which is almost as broad as all of economics. Since a plan is an expression of the wishes of the policy-makers there arises at the very onset the problem of formulation of a preference function. The preferences must be translated into policy objectives which require the construction of a (explicit or implicit) model of the economy. To find out how best to achieve the objectives it is necessary to make conditional forecasts of economic development; to make the forecasts, a knowledge of quantitative relations within the economy is needed. If plans are to be implemented, the economists must be aware of the political and administrative processes within the economy. If successive plans are to improve, an appraisal of past performance and an analysis of the causes of failure are essential.

The complexity of tasks and the multiplicity of talents which are required make collective publications particularly welcome. Much can be learned through a comparison of experiences of different economists dealing with similar problems, as well as from the comparative treatment of various aspects of the planning task. The two volumes under review rely, respectively, on the two approaches. The OECD concentrates on short-term national forecasting, which is an indispensable building block in the formulation of national short-term plans and policies. The Brookings Institution collection looks at a whole gamut of planning problems, from the formulation of policy-makers' preference functions, through econometric model-building, to a reconciliation of the approach of the mathematical economist-planner with that of a pragmatic policy-maker.

Both volumes draw on international experience. The OECD gives an account of short-term forecasting methods of the governments of Canada, France, the Netherlands, Sweden, the United Kingdom, and the United States. Of the eleven chapters of the Brookings volume, five discuss national planning experience. Of these two are devoted to the Netherlands, two to Japan, and one to France.

The two collections give the reader a vast menu to select from, and they contain some excellent morsels. Unfortunately these morsels do not amount to much of a meal. As in most reprints of conference papers, there is a lack of common purpose and of a uniform standard which would make the separate pieces add up to an integrated whole. The editors of the two volumes fought valiantly to forge unity out of confusion and to draw some general conclu-

sions (the palm goes to C. W. McMahon who performed a tour de force in writing a clear and concise summary of the achievements and problems of short-term forecasting incorporating the information given in the various national essays) but, short rewriting the component essays, they had to satisfy themselves with a limited success.

Take the OECD volume. The purpose of putting together the experience of six countries is, presumably, to permit a comparison of the success of the various national forecasting techniques. The degree of success of applying method X to country A cannot be readily compared with the degree of success of applying method B to country Y. Ideally one might want to see the results of application of diverse techniques to a single country, or of a single technique to diverse countries in order to learn something about forecasting methods as well as about the problems of forecasting under diverse institutional systems. Since the papers relate national experiences, one could hardly hope that any of them would undertake such a formidable research task. However, it would not be too much to ask for a uniform exposition of the methodology and at least some discussion of the success of forecasts using, if possible, comparable methods of measurement of forecast fit. Even this more modest requirement is far from being satisfied.

Of all the papers the one on Sweden comes closest to telling the reader how the forecasts are made and how good is the fit. The exposition is lucid and informative, though it would gain from a comparison between the results of the final forecast and of the econometric forecast which is run as a sort of a check on the more informal procedure used by the government. The Dutch who have spent years on model-building and on rigorous testing on forecasts have contributed a very skinny paper. The reader would learn much more by turning to C. A. Van den Beld's admirable essay in the Brookings volume.

The Canadian essay shows how and why things are done in that country, but some crucial questions are left unanswered. For instance we are told (pp. 47-50) that the forecasts utilize the results of a periodic capital investment intentions survey. A chart on page 49 shows that the survey has a systematic bias, but it is never revealed whether the forecasters try to correct for the bias or whether they accept its presence with fatalism. No attempt is made to discuss the fit of the forecasts.

The other essays are much less informative, with the prize going to the five-page piece on French forecasting. It may be noted in passing that Mr. Cazes's piece in the Brookings volume which deals with long-term features of the French plans is equally unrewarding. What the French planners say in fact is: "we are giving to our patient an excellent patent medicine called 'active planning.' It contains input-output tables, target figures, horizontal and vertical commissions and several secret ingredients. As the chief doctor, M. Massé, has said in a recent *Econometrica* article, the question to what extent are French plans implemented is *'ambiguous.'* Moreover, we should not ask whether the medicine does good or whether it harms the patient, because the unshaken belief in its efficacy is an essential condition for the medicine's success."

The high point of the Brookings volume is reached almost at the start. In

Chapter 2, Professor Theil gives a lucid, accessible, yet profound discussion of the problem of "Linear Decision Rules for Macrocynamic Policy Problems." The essay manages to show in twenty pages the advantages as well as the weaknesses of a quadratic preference function for policy-making purposes, and demonstrates how the method applies to short-term planning.

Two theoretical chapters by Karl A. Fox and Erik Thorbecke and by Jati K. Sengupta show that the authors have looked at many short-term forecasting and policy models and that they are well acquainted with the arcana of latest econometric thought. The few readers who are expert econometricians will, doubtless, disagree with this or that point, but will find most of the treatment sound if not very novel. The many whose econometric training is deficient will realize how much learning it requires just to understand what the authors say, let alone compete with them. For the authors it was doubtless useful to write down a concise summary of their knowledge. The readers will primarily benefit from the copious footnotes.

A bridge of sorts is provided between theoretical model-building and practical planning policies by E. S. Kirchen and Lucian Morrisens who have provided a chapter on "The Objectives and Instruments of Economic Policy" based largely on a collective work entitled *Economic Policy in Our Time.* The chapter catalogues all the familiar policy goals (full employment, balance-of-payments stability, etc.) and all the familiar policy instruments, and does very little else. The closing chapter by Charles C. Holt entitled "Quantitative Decision Analysis and National Policy: How Can We Bridge the Gap?" is much more rewarding. It shows what are the possibilities as well as the limitations of quantitative planning in the context of policy-making by the U.S. government.

If the reader learns anything from the Brookings Institution volume it is that the Dutch are at the forefront of the theoretical as well as practical work on short-term planning. Theil's discussion of the formulation of a policy-makers' preference function should be read in conjunction with William Hessel's "Qualtitative Planning and Economic Policy in the Netherlands" (Chapter 7 of the collection) in which it is shown how conditional forecasts are used in arriving at a policy consensus. Van den Beld's chapter on "The Short-Term Planning Experience in the Netherlands" tells in detail how the conditional forecasts are made, and evaluates the results of policies. This sort of unity (which should have permeated the whole book) is marred by some minor discrepancies in treatment. For instance in Van den Beld's essay the 1956 economic situation and the 1957 forecast are presented (on p. 155) in 1956 prices, and in Hessel's essay (on p. 171) in 1955 prices. Van den Beld discusses the tragets of the Central Planning Bureau (which on p. 155 are mistakenly labeled as "Effects of the Program on 1957 Outcome"), whereas Hessel deals with the targets of the Social and Economic Council which were changed before being incorporated in the plan. The relations between the two sets of targets are never made clear. The resulting confusion could have been eliminated through more careful editing.

If any reader needs to be convinced that in some countries there is a vast

gap between the theory and practice of economic planning, he should turn to the two chapters by Shishido and Watanabe, which show how the Japanese economy has been steadily running away from the planners. The Japanese plans have been overcautious and poorly "balanced," and they have had very little impact on the country's development. Whether the Japanese economy suffered or benefited from the lack of planning is something of a moot point.

Both the OECD work and the Brookings volume have useful references, and the Brookings volume has an index.

STANISLAW H. WELLISZ

Columbia University

Economic Policy in Our Time. By E. S. KIRSCHEN AND ASSOCIATES. Vol. I: *General Theory;* Vol. II: *Country Studies: United States* [O. Eckstein], *United Kingdom* [F. Blackaby], *and Norway* [J. Faaland]; Vol. III: *Country Studies: Belgium* [L. Morissens], *Germany* [H. Besters], *the Netherlands* [F. Hartog], *France* [J. Benard], *and Italy* [E. Tosco]. Chicago: Rand McNally & Co.; Amsterdam: North-Holland Pub. Co., 1964. Pp. 474; 224; 482. $12.50; $6.00; $12.50.

Volume I attempts to generalize the practice of economic policy in eight Western countries between 1949 and 1961. It is accompanied by two volumes reporting actual measures taken. Readers will be disappointed if they expect an expansion or new explorations along the frontier of the theory of economic policy scouted so admirably by Jan Tinbergen nearly a decade ago.[1] However, nothing in economic literature approaches the present authors' threefold aim: (i) to classify the instruments and objectives for a large and representative part of Western economic history since World War II; (ii) "to compare and contrast the economic policies actually followed in our countries since 1949"; and (iii) "discuss the ways in which policies are in fact formed and difficulties of their execution" (Vol. I, p.v). These are admirably accomplished, but alas they aren't enough. The names of Professor Kirschen's associates create expectations which remain unfulfilled. By restricting their investigation to a taxonomical description of economic policies we are denied, as they acknowledge, learning anything new about the efficacy of policies and the economic mechanisms by which measures achieve aims. Their excuse for having stopped short of venturing into what economists generally consider their proper realm is they "had enough to do without it" and in doing so "would have had to reach agreed views on a number of the more disputed points in present-day economics. This would not have been practical" (I, p.vii). In sum, we are told only about the decision-making process and decisions actually taken, but virtually nothing about the time lapse before the actions achieved or failed to achieve the desired objectives, nor about the conflicts, interactions, and feedbacks within and between instruments and objectives. The book should be judged, of course, by how well it accomplishes its aims, but my disappointment is no less.

Volume I, subtitled *General Theory,* is the joint product of all nine authors. In Part I they use the terminology and framework of analysis developed by

[1] Jan Tinbergen, *Economic Policy: Principles and Designs,* Amsterdam 1956.

Jan Tinbergen and the postwar experience of the six Common Market countries, together with Norway, the United Kingdom, and the United States, to prepare a standardized classification of instruments and objectives "which will be suitable for any developed country with a capitalist or mixed economy" (I, p.v). Twelve objectives were chosen to describe the aims of countries between 1949 and 1961 (I, pp.5-6):

Mainly short-term (conjunctural)

1. Full employment
2. Price stability
3. Improvement in the balance of payments

Mainly long-term (structural)

Major

4. Expansion of production
5. Improvement in the allocation of factors of production
6. Satisfaction of collective needs
7. Improvement in the distribution of income and wealth
8. Protection and priorities to particular regions and industries

Minor

9. Improvement in the pattern of private consumption
10. Security of supply
11. Improvement in the size or structure of the population
12. Reduction in working hours.

Certainly every economist could question both the list's limitations (why are increasing productivity and rising real wages omitted?) and the distinction between essentially conjunctural and long-term, e.g., full employment and improvement in the balance of payments for the United States appear to require considerable structural change; but each reader would probably agree with the authors' desire to "keep the number of objectives as small as possible" (I, p.6).

The authors discuss five families of instruments: public finance, money and credit, exchange rate, direct controls, and changes in the institutional framework. All together, 63 instruments are enumerated as having been used between 1949 and 1961, serving the authors claim, 303 objectives.

"Considering the whole field of economic policy," they state, "the public finance family of instruments was the most important one" (I, p.148). This conclusion is based, like most of the others, not on the results of empirical investigations, but rather on the authors' agreed collective opinions. This is a relatively new technique of nonparametric analysis which, I suppose, will be used more frequently owing to the increasing costs of statistical studies. The method has the obvious advantages of being economical, but the disadvantage of being subject to the particular biases of its practitioners.

In Part II the authors develop their theory of the decision-making process by which objectives and instruments are chosen. The principal policy-makers are parliament, political parties, government (politically appointed officials), administration (permanent civil service and associated agencies), interest groups, foreign and international influence, and courts. The authors state: "The role of Parliaments . . . is a declining role in the decision making process . . ." (I, p.159), which they explain by the expansion of public policies (I, p.164) and "through the development of parties as disciplined and autonomous bodies. . ." (I, p.193), but this is not supported by any analysis and rests, like most of their other statements, on their collective opinions. The authors propose a law about the distribution of influence between the important policy-makers: "the influence of the Administration is an inverse function of that of the interest groups and the political parties" (I, p.236). Again, I regret that they did not attempt to analyze and illustrate the relationship, nor to explain whether the casual relationship is only from interest groups and political parties *towards* the administration or whether all three are dependent upon other factors, too.

The authors also conclude that short-term objectives take priority over long-term ones (due to the priority of the urgent), but this is later weakened by their statement that the long-term objective of economic expansion provided the chief focus of policy in France, Italy, and Norway throughout the entire period, was dominant in Germany during the first half, and rose to the first rank by the end of the period in Belgium (I, p.348). Further, they believe that there is less disagreement over objectives than over the instruments to achieve them, because instruments affect different economic and social groups. While global multipurpose instruments are preferred to the more efficient instruments because the former have a greater probability of success, the authors formulate another law that instruments are preferred "in inverse ratio to the degree of intervention that they involve" because intervention carries a psychological cost (I, p.243).

A final chapter in this part discusses time lags between the recognition of a public policy issue and the taking of decisions to deal with it. This a useful addition to the theory of economic policy, since economists generally emphasize the efficiency and time dimensions of instruments *after* the decisions have been made. The authors expect the time lag to increase because society is developing better-organized interest groups and international institutions which add a new consultative period to the decision-making process.

Part III concludes Volume I by highlighting the general characteristics of economic policies for two short-run objectives (inflation and recession) and two long-term objectives (expansion of production and improved income distribution). There are also two final chapters on international cooperation and European economic integration. This part of the book, unfortunately, makes little effort to utilize or illustrate the theories developed in Part II and, for those familiar with Maddison's account of postwar European economic growth, these chapters will add little.[2]

[2] Angus Maddison, *Economic Growth in the West*, New York 1964.

Volumes II and III contain eight country studies, each from 70 to 150 pages. While each was prepared by a different economist, there are only minor differences in description, analysis, and style. The terminology of objectives and instruments developed in Volume I is used uniformly to describe each country's economic policy between 1949 and 1961. A brief summary of the important political and economic events introduces each study, followed by a section on short-term policies and another on long-term ones. The principal objective was to present an exhaustive chronology of each policy measure taken, and for this reason these volumes will be immensely useful for further work, but timetables make painful reading. There are few similar studies, but anyone who has read, for example, Dow's analysis of postwar economic policy in Britain will be disappointed by these colorless accounts.[3]

There is very little correspondence between the complex power relationships which form the basis of the theory of economic policy presented in Part II of Volume I and the play-by-play description of policies actually implemented in Volumes II and III. In spite of the introduction's caveat that the method of presentation inadequately reflects the interdependencies, one cannot help but conclude that the implicit practices of the policy-practitioners confirm Tinbergen's ten-year-old description. He called them "incoherent" because the instrument changes are:

> . . . more or less arbitrary . . . and if after some time they appear to have been insufficient, something more is done. Or, if the first change overshoots the mark, a change is made back in the opposite direction. . . . Measures regarding various instruments are taken separately . . . to some extent based on the belief that there is a one-to-one correspondence between targets and instruments (*op. cit.* pp. 54-55).

Tinbergen hoped that the interrelations and simultaneous considerations could in the future be considered, but these country descriptions lead one to conclude that the practice of economic policy is far removed from contemporary theory.

In sum these are not easy volumes to evaluate. The theory of economic policy presented in Volume I adds a new dimension to Tinbergen's insofar as it defines the power relationships and time lags existing prior to applying an economic instrument, but no attempts were made to measure or evaluate the efficiency of the instruments now or to tell when or how objectives were attained. Explicit models would have made the volume more useful to economists. The two volumes of country studies are clearly not designed for continuous reading, but they do provide excellent references for further studies of economic policies.

WAYNE W. SNYDER

Organization for Economic Cooperation and Development

[3] J. C. R. Dow, *The Management of the British Economy*, New York 1964.

Business Fluctuations

Gold and the South African Economy—The Influence of the Goldmining Industry on Business Cycles and Economic Growth in South Africa 1886-1961. By LEO KATZEN. Cape Town: A. A. Balkema, 1964. Pp. 115. $3.00.

Leo Katzen's book, a revised version of his M.Sc. thesis written in 1954 for the London School of Economics, examines the influence of the gold mining industry in South Africa on the country's business cycles. Since the amount of gold produced in South Africa has shown a remarkably steady climb, with little trace of cyclical fluctuations, the influence of gold mining on business cycles has obviously been stabilizing. The notable declines in gold production in the early 1920's and mid-1940's follow such exogenous influences as strikes and war, while the accelerated increases in the industry's rate of growth in the late 1930's and 1950's are the result of devaluation and windfall new discoveries of gold.

The stability of the rise of the gold mining industry in South Africa is based on the fixed dollar price for gold since 1934. The theory of gold production, in a world anchored to a fixed gold price as a "built-in stabilizer," is outlined very well by Katzen in the first two chapters (pp. 1-14) of his book. These are the two most valuable chapters of the book, since they relate recent trade-cycle theory to older studies such as Professor Frankel's classic *Capital Investment in Africa.*

The remaining four chapters (100 pages) of the book are less likely to capture the attention or confidence of the reader. His conclusion, that gold mining has been a stabilizing influence on the South African economy, but not as much (particularly in the discovery stages) as a pure theorist might in his dreams expect, is pretty obvious and unexciting.

Katzen concludes that the future of gold mining as a major sector of the South African economy seems assured for some time to come, but it should be noted that its continued expansion is not assured. The mining companies are increasingly moving into industrial activities, in recognition of the limited scope for future growth of gold output. Failing Dr. Busschau's touted medicine of an increased price for gold, it is generally expected in South Africa that a ceiling on annual gold output will be reached in about seven years at the outside. Government planners and private entrepreneurs are looking to manufacturing industry to provide the expansion which will maintain the country's rate of growth at an average of 5.5 per cent.

Katzen's book has some valuable nuggets buried in it, but one has to dig. Trade-cycle theorists may be put off by the technicalities of South African gold statistics, while those specially concerned with the gold mining industry may find the application of trade-cycle theory to their subject an unfamiliar exercise. Chapter 4, on the importance of the gold mining industry in the South African economy, goes over old ground (see, for example, Chapter 5 of Houghton's *The South African Economy*). The following chapter, on the effect of the gold mining industry on South African trade cycles, is a rather

uninspiring application of gold mining statistics to Professor Schumann's studies of the South African trade cycle.

Katzen's main contribution is a refinement of the theory of the stabilizing influence of gold production in a fixed-gold-price world. It is, of course, a special case since in no other country does gold production play as important a role in the economy as it does in South Africa.

JOHN A. MARLIN

Washington, D. C.

Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Inflation and the Theory of Money. By R. J. BALL. Chicago: Aldine Publishing Co., 1964. Pp. 313. $7.50.

This is a selective survey of inflation theories, intended for graduate students. Much of the book develops the author's own theory, a synthesis of demand-pull and wage-push views.

The first four chapters compare simple Keynesian and quantity theories. Two long chapters then analyze the determination of wages under unions and the pricing policies of modern business. Another two chapters develop the connection between expenditures and the demand to hold money, following the conventional theory of asset demand. A final two chapters discuss the inflationary process and the problems of preventing inflation. For students in a hurry (and who isn't?), Chapters 6, 7, and 10 present the author's theory and can be read separately.

R. J. Ball wants to unify the confusing diversity of inflation theories, and for his own contribution he purposely combines wage push and demand pull, elsewhere usually treated as alternative explanations. The combination is achieved by making wages and aggregate expenditures a function of the same variable—expected profits. A rise in expected profits raises aggregate expenditures through an expansion of investment and a reduction of business money holdings (which raises monetary velocity). Expected profits depend in part on projections of current levels of activity and unemployment. A rise in expenditures and output, therefore, raises expected profits further. The upward movement does not continue without limit: when all idle money balances have been activated or interest rates rise fast enough, the expansion stops unless the money supply increases. The model takes the quantity of money supplied as exogenous and constant.

Wage costs are brought in on the argument that they determine prices in the short run. Corporations set prices, not to clear the market, but to maintain a "satisfactory" long-run growth. Management ignores short-run changes in demand but not in costs, of which wages are the key item. In the unionized sector, bargaining determines wages. Ball portrays unions as pushing relentlessly for higher and higher money wages, but tempering their demands when unemployment increases. This relates wages to expected profits, since the latter depend in part on current unemployment. Employers' resistance to union wage

demands also depends on expected profits. Union wage gains set the pattern for nonunion industries through a mysterious "transfer mechanism" which Ball borrows from the literature on labor markets but does not clarify.

His model specifies, given certain parameters and a constant money stock, a particular rate of change in prices. Even if profits are not expected to change, wages and prices still rise because of union pressures. If profits are expected to increase, aggregate demand expands, and wages (and prices) rise faster. The rate is not arbitrary: A higher rate is not possible, because expected profits would then rise less rapidly or be reduced, thus restraining the rise in wages. A lower rate is not possible, because the expansion in aggregate expenditures would then deplete inventories and raise expected profits, thus allowing wages and prices to rise faster. While an increasing money supply could keep the inflation going forever, a constant supply eventually stops it. When the rise in velocity tapers off, aggregate demand falls and expected profits decline, which increases unemployment and restrains wages. Expected profits then fall farther, despite the slower rise in wages. Autonomous investment acts as a floor to this downward spiral. The model seems to imply cycles rather than irreversible upward movements. Expected profits play a role here similar to that of inventory investment in accelerator theories of the business cycle.

What is new in Ball's model? Although consistent with much recent writing, it goes further and makes the pace of inflation depend explicitly on both wage bargaining and aggregate demand.

What does the model help explain? I believe it has quite limited applicability. It describes a velocity inflation, as might occur during an investment boom like 1955-56. The synthesis of cost and demand factors serves no purpose unless the rise in prices during such a boom is significantly affected by the rise in wages. I see no evidence of this and much to dispute it. Moreover, most inflations (including the creeping variety of current interest) do not depend on increasing velocity. Most reflect excessive growth in the money stock. A common view is that the monetary authorities, in order to keep unemployment from rising, reluctantly create enough money to underwrite wage bargains that exceed gains in labor productivity. Whether that view is correct or not, profit expectations do not play a central part. When expenditures rise more rapidly than velocity does, the crucial question is how and why the money supply increases, which Ball's theory does not deal with.

Ball applies his theory to a period like the 1950's in this country and Britain, when velocity did rise considerably. But the rise in velocity over the decade can be attributed initially to a belated adjustment to wartime increases in the money stock and then to the upward trend in interest rates and to related financial developments, and only in certain years to a rise in profit expectations. Union wage gains may have affected the timing of those price movements but not the over-all increase. To conclude, as Ball does, that the rise in velocity during the 1950's reflected a rise in expected profits implies, contrary to fact, that velocity would fall to its original level when expected profits returned to normal. Such expectations help explain short-run fluctuations in velocity but not the long-run movements. Substituting interest rates for expected profits in the model does not preserve the synthesis, because the two variables

do not always move in unison and because wage increases are not closely related to interest rates.

Although of limited empirical applicability, the theory in Chapters 6, 7, and 10 can still be recommended to graduate students (and their teachers). Ball gives a useful discussion of the many problems of devising a theory of inflation.

PHILLIP CAGAN

National Bureau of Economic Research

The Financial Sector and Economic Development—the Mexican Case. By ROBERT L. BENNETT. Baltimore: The Johns Hopkins Press, 1965. Pp. viii, 210. $6.95.

It is Robert L. Bennett's thesis that Mexican financial institutions and policies have played a strategic permissive role in the country's recent development. While he does recognize the important entrepreneurial activities of some of these institutions (the Nacional Financiera, for example), Bennett has preferred to analyze the ways in which policy has been used to prevent financial bottlenecks from spoiling the plans of businessmen and government officials who have been the primary undertakers of innovative activity.

It is probably impossible actually to demonstrate that financial policy has been crucial to recent Mexican development. Nevertheless, Bennett does present an interesting case. He first builds a model of an economy in transition from a traditional state, in which per capita income is constant, to a state in which per capita income is growing. Arguing that the Mexican case resembles his model in all essential respects, Bennett proceeds to evaluate the role of Mexican financial institutions and financial policies in terms of the model.

The traditional economy is dominated by traditional finance. Net and replacement investment are derived mainly from internal funds of the firm and from short-term bank loans. Poorly organized capital and money markets create severe difficulties for those who wish to raise funds outside the highly personalized sources of traditional finance. But a transitional economy requires innovative investment. Such investment is made by entrepreneurial elements outside the bailiwick of traditional business and traditional finance.

Most economists will recall the capitalist-banker, whose role it is in the Schumpeterian system to provide entrepreneurs with innovative, albeit inflationary, capital. In Bennett's traditional economy, there are few such bankers. Hence the problem for planners becomes one of innovational finance —the creation of institutions which can wrench financial capital from the traditional sector and put it in the hands of innovating entrepreneurs, preferably in a way which avoids inflation.

Apart from using the fisc, government planners might both manipulate existing financial institutions and create new ones to compete with or complement existing ones. Existing institutions can be forced to invest in innovative projects of both government and private business. Government banks can be created to service new industries. The trick is to use the system of regulation

and new banks in a way which avoids inflationary finance and which does not weaken existing traditional banking institutions. The latter's short-term liabilities militate against the heavy use of their assets for long-term innovative investment, and they tend to specialize in liabilities preferred by traditional surplus-spending units.

Government banks can only partially compete with traditional banking and financial institutions in providing acceptable liabilities to the saving public, mainly because the assets of government financial institutions tend to specialize in long-term securities from innovative borrowers. This means that short of additional governmental funds raised from taxes, government financial intermediaries must raise noninflationary finance from other intermediaries. This can be done in three ways. First, from government capital grants or loans financed by sales of government securities to private financial intermediaries who correspondingly reduce private loans. Second, from foreign borrowing. Third, from sales, forced or voluntary, of government or intermediary liabilities to private intermediaries.

So managed, such a financial system would come to take on the following characteristics: First, a system of private financial intermediaries whose liabilities, relative to the governmental intermediaries, are specialized to business and individuals and whose assets are relatively specialized to government securities and liabilities of other intermediaries. Within its holdings of private obligations, the private intermediaries would specialize in short-term, as opposed to long-term, loans and investments. Second, government financial intermediaries would have liabilities heavily weighted by borrowings from other intermediaries and by foreign debts. Their assets would emphasize long-term loans and investments and might even predominate over the private financial intermediaries in loans and investments made in the private business sector.

Basically, this is the story Bennett tells for Mexico. By the early 1940's, Mexican financial legislation had evolved an elaborate system of controls and governmental intermediaries. This system was manipulated in subsequent years for the purpose of channeling finance into the points of the economy which the government sought to promote—presumably the innovative sector —and also for the purpose of preventing such promotion from resulting in excessive inflation. The success of this financial program may be seen in the progressive mildness of inflation in Mexico from 1945 to 1960 and in the sustained rapid rate of growth in per capita real income. This experience may be sharply contrasted with the high degree of inflation both in Mexico before the system of controls and institutions were perfected and in other Latin American countries.

This generally favorable picture of Mexican growth was associated with changes in the characteristics of financial institutions which might be expected on the basis of Bennett's model. Between 1944 and 1959 private intermediaries increased their holdings of government securities from 8 per cent of the holdings of such securities by the whole financial sector to 55 per cent. In 1959, private intermediaries accounted for only 41 per cent of financial-sector loans (and 38 per cent of long-term loans and investments) to private business and individuals, as opposed to 60 (and 55) per cent in 1944. Private in-

termediaries in 1959 held 80 per cent of the total financial system's holdings of intermediary liabilities, whereas in 1944 they held 62 per cent. In the meantime, of course, the movement of the assets and liabilities of government intermediaries moved in exactly the opposite direction. In addition, government financial institutions contracted for all the foreign debt held by the financial system (mainly through the Nacional Financiera). While all this was happening, the government and private financial intermediaries maintained roughly the same size relationship to each other in terms of assets of the whole financial system.

This basic statistical story is elaborated in considerable detail in Chapters 3 and 4 and in a long appendix. Bennett is to be congratulated for his ingenuity and doggedness in telling a complex story. His results certainly do conform to the expectations of his model in many ways. But Bennett's contribution goes beyond Mexico's experience. He has blended the insights of Gurley and Shaw with the Schumpeterian theory of growth, whilst applying the mix to the planning experience of one country. In the process, he has supplied a framework for analyzing the place and task of financial planning for other countries in similar circumstances.

BARRY N. SIEGEL

University of Oregon

Monetary Equilibrium and Economic Development—with Special Reference to the Experience of Greece, 1950-1963. By XENOPHON ZOLOTAS. Princeton: Princeton University Press, 1965. Pp. xi, 223. $6.00.

This book is divided into two tenuously connected parts, one noticeably longer than the other. In the first part (22 pages), Xenophon Zolotas presents his hypothesis that relative price stability and growth are not mutually exclusive but complementary development policies. In the second part (201 pages), he describes and analyzes the problems and progress of the Greek economy during the period 1950-63. Attempting to show (1) that what has been successful in Greek economic development is due to the strong commitment of the Greek monetary and fiscal authorities to the above-mentioned thesis, and in addition (2) that whatever structural weaknesses remain in the Greek economy are due to *other* factors (especially quantitative and qualitative deficiencies in human capital), Zolotas uses his case study to support his general hypothesis.

In Part I, inflation, as a means of promoting growth, is rejected on the grounds that the rationale often used to defend it—the "income distribution effect"—is not a realistic possibility because the wage-earning class will not tolerate a reduction in their standard of living that is already at or near the subsistence level. Even if income could be redistributed through inflation, it will still be unlikely that expenditures would be shifted from consumption to investment because the profit-earning class more than likely would succumb to the lures of luxury-type consumption goods. On the other hand, price stability is not enforceable unless living standards are rising through growth. Therefore, only by steering a course between the Scylla of inflation, on one side, and

the Charybdis of stagnation, on the other side, is either growth or relative price stability possible.

Although he warns us that the size of the passage will depend on general confidence, on the extent to which money illusion prevails, and on other particular circumstances, the author gives us neither analytical devices nor any suggestions as to how to employ quantitative analysis that might help us navigate this narrow and treacherous passage. This description of what the economist in the position of monetary authority must be like is, indeed, more that of an artist with sensitivity and experience than that of an a scientist with his bag of analytical tools and laboratory devices.

In refuting the proposition that price stability and long-run growth are separate alternatives, Zolotas seems to mistakenly imply that not only extreme, but also moderate, monetarists and structuralists argue that they *are* alternatives. Furthermore, inasmuch as the author admits that the historical situation may require variations in emphasis (i.e., inflation-riddled countries need to emphasize price stability, while low-growth countries need to emphasize growth), it would seem that Zolotas' position is not so different as he would have his readers believe.

From this general discussion of his thesis Zolotas moves on to the case of Greece. It may hardly be necessary to point out that, even if the reader should agree with Zolotas' contention that the Greek case does support his thesis, the general thesis cannot be proved valid on the basis of a sample of one. Since Zolotas has been the Governor of the Bank of Greece for the greater part of the period under consideration, his survey of Greek monetary problems and policies within the context of development is most authoritative, but hardly most objective.

Although his defenses of the monetary and fiscal policies which Greece has followed generally since 1950, and wholeheartedly since 1956, are most convincing, they do not entirely remove the suspicion that Greece might have been able to attain a similar growth rate with either somewhat more conservative or somewhat less conservative policies. Had he compared Greece's successful policies of the postwar period with the dismal failures of the prewar period, he might have made his case even stronger.

The Zolotas defense of Greek credit policy in the postwar period is much less convincing. Greek monetary authorities have practiced a discriminatory credit policy which they justify on the basis of an assumed divergence of social productivity from private profitability. No one familiar with the Greek economy will be likely to doubt that there is such a divergence, but similarly no one familiar with the formidable difficulties of quantifying the extent of divergence from sector to sector will be likely to believe that the Greek Currency Committee could have been very accurate in its intuitive judgments about the magnitude and direction of the divergences. Little evidence can be found to show that the discrimination as practiced by the Greek monetary authorities has been successful. Despite the obvious limitations in the power of credit policy, one cannot help but wonder whether *some* of the structural weaknesses which continue to plague the Greek economy (as Zolotas repeatedly points

out) might not have been due to a less than optimal credit policy established by the monetary authorities.

In responding to the charge that the Greek monetary authorities have been too restrictive, Zolotas points out that supply of credit has always been greater than demand. Therefore, he argues, the real obstacle to further growth must lie elsewhere—namely, in the insufficient quantity and quality of Greek entrepreneurs and managers. Skilled entrepreneurs are scarce in Greece as in many other less developed countries. Nevertheless, is it not possible that Zolotas is using this argument to decoy us from what may be a more fundamental explanation for the excess of credit supply over credit demand, namely, that the interest rate is too high? It is the latter hypothesis, not the former, that would seem to be most consistent with the widely accepted view that the Greek banking industry is oligopolistic.

In any case the value of the second part of the book should be judged not only by the degree to which it supports the author's thesis, but also by the quality of the analysis which the author brings to the problems and policies in Greece's postwar economic growth. It is in this latter respect that the book is most valuable, especially considering the paucity of serious studies of Greek economic problems and performance. With the exception of tariff and trade policy which unfortunately is not dealt with in this book, Zolotas' presentation of Greek development policies and problems is both complete and concise. It contains much information which is not readily accessible elsewhere.

JEFFREY B. NUGENT

University of Southern California

The Independence of the Federal Reserve System. By A. JEROME CLIFFORD. Philadelphia: University of Pennsylvania Press, 1965. Pp. 435. $8.50.

The issue of Federal Reserve independence has received considerable attention over the years, officially from Congress and the Federal Reserve and privately from reflective policy makers such as Glass, Warburg, Willis, and Eccles. A. Jerome Clifford's book selectively reports the development of this official record.

The book begins with a discussion about the fears that the authors of the Federal Reserve Act had that the Federal Reserve might be dominated by politicians, if not by private bankers, and of the checks and balances that were devised to limit concentrations of authority within the System. It then goes on to describe the ensuing power struggles that were necessary to get around the Act's ambiguities in assigning operational objectives and implements, and decision-making authority. The author concludes with an assessment of the prospect for continued Federal Reserve independence. It is not possible to challenge the truth of his statement that the Federal Reserve can remain independent in the exercise of its powers provided Congress wants it that way, a choice that will depend on how well the Federal Reserve satisfies objectives and how clearly it explains its actions.

Though the book presents an often interesting chronology of the public re-

cord on the issue of independence, it probably does not make a very significant contribution. Sadly lacking is any reference to the rich historical experience of other countries with respect to relationships between the central bank and the government or the private banking system. Also there is no attempt to utilize numerical data in getting at the question of whether the independence of the central bank makes any real difference. And there was scant attention paid to the evaluation of the issue of Federal Reserve independence by economists or historians who did not have official status, e.g. Laughlin, Harris, Hardy, and most recently Friedman.

The narrative line of the book goes about as follows: Congress passed the Federal Reserve Act in 1913, and it has frequently altered this indenture under which the central bank operates. The Act gave the President authority to appoint Federal Reserve Board members. The Board, though frequently challenged by Reserve Banks through the 1920's, developed into the key power center within the system. Its renaming as the Board of Governors in the 1930's attached the traditional appellation of central bankers where the power actually was rather than, as before, with the heads of the Reserve Banks. The Board, however, is forced to share responsibility for monetary policy with five Reserve Bank Presidents, who along with the seven-member Board, form the Federal Open Market Committee. Nevertheless when the chips are down, the Board reigns supreme. Reserve banks are formally given authority to fix discount rates, but this power is subject to the Board's review and determination, which has been interpreted to grant the Board authority to fix rates. Reserve Bank directors formally elect the top Reserve Bank executives, but the Board retains the authority to approve or to remove them. In fact the Board's authority probably extends to every possible Reserve Bank activity through its authority to audit Reserve Bank operations. The emergence of the Board as the power center of the System has tended to increase the probable dominance of public over private interests in the operations of the Federal Reserve despite the remaining vestiges of the founders' hoped-for financial democracy. Clifford's book reminds us that the existing reasonably well-defined relationship between the Reserve Banks and the Board on the one hand and the Reserve Banks and the member banks on the other was once a very controversial issue.

By all odds the most significant power struggles historically have pitted the Federal Reserve against the Treasury, and Clifford supplies a very interesting account of them. Referring to this intragovernmental relationship, it is something of a paradox to the reviewer that an independent Federal Reserve is a creature of government designed to protect the government against its own propensity to spend excessively; yet the Federal Reserve actually has facilitated inflationary government spending during both World Wars; and these periods have been virtually the only periods during Federal Reserve existence when there was any significant inflationary impact from government fiscal actions. Clifford quite effectively analyzes the complex maze of governmental decision-making in the monetary field, returning repeatedly to the fact that the independence of the Federal Reserve is derived from Congress. Con-

gress created it, added to its powers during the 1930's, and helped free it from self-imposed Treasury domination after both World Wars. In part its independence stems from the willingness, if not the necessity, of Congress to entrust it with the detailed decision-making that goes into formulating and implementing monetary policy. Congress does require the Federal Reserve to account publicly for its actions and the reasons for taking them; and it regularly demands a detailed defense from Reserve spokesmen. But the central bank undoubtedly enjoys a large measure of freedom with respect to specific policy actions because of the wall of jargon and secrecy with which it has been permitted to respond to often-uncomprehending Congressmen. The final important source of independence is attributed to a Congressional grant of budgetary freedom to the Federal Reserve that permits it to keep its earnings separate from the Treasurey general fund and not to have account to the Treasury for its expenses. Congressman Patman and Senator Douglas notwithstanding, the Federal Reserve may have been able to preserve its financial independence in some part, not only by policies that leaned with Capitol hill winds but by a policy of judicious operational expenditures.

WILLIAM G. DEWALD

Ohio State University

The Federal Reserve and the American Dollar—Problems and Policies 1946-1964. By JAMES L. KNIPE. Chapel Hill: University of North Carolina Press, 1965. Pp. xiv, 321. $8.00.

James Knipe, currently an investment counselor, served as consultant to the Federal Reserve Board from 1959 to 1963. His book purports to be an account of the Federal Reserve System from its inception in 1913 to the present, with major emphasis on monetary policy during five postwar cycles.

Knipe begins with an analysis of the organizational structure of the Federal Reserve, makes a brief, elementary survey of how the stock of money is determined, and then turns to the operation and the goals of monetary policy. Unfortunately, the reader will not find an intellectually satisfying account of either the goals of monetary policy or the routes by which policy operates. The quality of the book can be conveyed by the following statement: "It is certainly possible over the course of the coming years, that statistical analysis may reveal more of an impact on ultimate economic objectives from a differently defined Money Supply, one which includes Time Deposits. . . . Until that time, the transmission of the original policy impulse can, perhaps, be thought of as an *influence*. A stronger word or phrase would seem to be unrealistic" (p. 31). Nevertheless, Knipe believes "The Federal Reserve holds in its hands the power to make sweeping economic and sociological changes in the nation, at least in the short run" (p. 34). The reader is not told what changes Knipe has in mind. Or again, he writes, "It is impossible, even in retrospect, to evaluate the usefulness of any monetary tool, general or selective. One's common sense though, leads to the conclusion that consumer credit control,

interrupted, criticized and attacked as it was, could scarcely have been very effective, except occasionally, during the 1946-48 period" (p. 57). In this manner, throughout the book, assertion replaces intellectual analysis.

Nor does the book provide an intellectually satisfactory account of the goals of monetary policy. Missing is any awareness of trade-off ratios between goals such as full employment, on the one hand, and price level stability, on the other. What percentage of unemployment is compatible with price stability? What percentage of rise in prices is associated with a given percentage of reduction in employment? Can these trade-off ratios be influenced by monetary policy? He makes no attempt to answer these questions, nor is any mention made of the important empirical work which has been done to determine these trade-off ratios.

Knipe does believe that the Fed should try to restrain the excessive degree of inflation which is being foisted on the economy by powerful economic blocs. (He does not mention any limitation that price indices may have in measuring prices.) He is opposed to the view that the Federal Reserve's main goal should be full employment without regard to inflation and the burden of the debt—a view, which, according to him, is increasingly prevalent, and whose proponents everywhere are "busily engaged in creating a brand new mythology of their own" (p. 33). The reader is not told who these proponents are.

He is on somewhat firmer grounds when he deals with the problems of the internal organization of the Fed. He is for competent, well-publicized research by the staff of the Board of Governors. He discusses the pros and cons, without taking sides, of whether the term of office of the Governor of the Board should be made coterminous with that of the President; he does the same with the question of whether the commercial banks should hold stock in the Fed. In Appendix C on the "Discount Rate," he comes out against a discretionary rate and for a discount rate automatically linked to market rates. Appendix B on "Bills Only" is a pastiche of questions from Congressional Reports highlighting the opposition of the New York Bank to the Board's commitment to bills only. Although Knipe gives no economic appraisal of the controversy, he is against a permanent commitment to any specific policy, and urges the System to issue a White Paper on bills only with a view to silencing critics outside the System.

Perhaps it is unfair to expect a nonacademic economist to keep in touch with an ever-growing body of professional literature. It is only fair, however, to say that Knipe's attitude towards the academic profession must be considered parochial. He writes of Keynes, "At least in universities and among members of the economic profession, he has triumphed so completely that opposition is practically invisible. To conservative thinkers everywhere, on the other hand, his name stands as a symbol for all they believe to be wrong with the postwar world" (p. 42). One can only hope that Knipe's attitudes towards the academic profession are not typically those of investment counselors.

ALVIN L. MARTY

City College, City University of New York

Public Finance; Fiscal Policy

Measuring Benefits of Government Investments. Papers Presented at a Conference of Experts Held Nov. 7-9, 1963. Edited by ROBERT DORFMAN. Washington: The Brookings Institution, 1965. Pp. viii, 429. $6.00.

This is an important book that merits careful reading, not only by economists interested in the resources field, but by others concerned with the intransigent problems of assessing the allocation of resources in areas wholly or partially divorced from the market mechanism.

The book is made up of seven essays, with critical comments, presented at a conference sponsored by The Brookings Institution in November, 1963. The purpose of the meeting was to bring the tools of microeconomic analysis to bear on a number of troublesome areas of government expenditure in which the identification and valuation of costs and benefits are beset by the familiar problems of interdependent production functions, nonpriced outputs, scale economies, etc. The full potential of benefit-cost analysis in areas other than water resources remains to be explored. It is, of course, no more than a technique for simulating the results to be expected from the competitive model under a full employment assumption, and is subject to the same limitations. But used with appropriate care, it can improve significantly the understanding of alternatives available to policy-makers and their relative economic merits. With all its problems, benefit-cost techniques have already produced much more satisfactory decisions in the development of water supplies and even in the incredibly complex maze of defense expenditures.

Seven areas of application are covered: government research and development (Scherer); outdoor recreation (Mack-Myers); prevention of high-school dropouts (Weisbrod); civil aviation (Fromm); urban highways (Mohring); urban renewal (Rottenberg); and syphilis control (Klarman). The fields selected for analysis are not trivial; each covers an important area of government investment where proper allocation assumes real significance to society. Since choices are already being made in these areas, a hard crust of noneconomic criteria for decision-making has grown up in each—some of it useful, but much of it steeped in mythology.

With some minor exceptions, the papers follow the same general format: a listing of the beneficiaries and cost bearers involved in the particular program; an evaluation of impacts on these groups of alternative types of investment in the area concerned; an attempt to quantify both benefits and costs, in total and for particular groups; and an evaluation of the potential usefulness of the procedure.

There is an equally strong similarity in the results that emerge from the seven papers. The first two steps, involving identification and qualitative analysis of impacts—actual and potential—on various groups, come off very well in each case. The critical problem is, of course, the extent to which benefits and costs can be estimated quantitatively to a degree sufficient to sharpen the decision-making process. There are really two elements involved. Are the estimates themselves sufficiently accurate, at least in a probabilistic sense, to pro-

vide a basis for evaluation of alternative programs? Is the range of benefits for which such evaluations can be made large enough relative to total benefits so that the tail is not wagging the dog? There is, after all, more than casual merit in the typical bureaucratic position that a high degree of precision in benefit-cost analysis involving perhaps 10 per cent of the total program is not very helpful and can be dangerously misleading if given undue weight in the planning of an investment program. These questions remain open after all the papers are read.

This is, of course, to be expected. No claim is made by any of the seven authors that benefit-cost analysis can provide final answers to government decision-making, and all recognize that the present state of the art is hardly satisfactory even for the more limited objective of identifying and quantifying all potentially measurable costs and benefits. The essays are intended to advance techniques rather than to provide methods of analysis of immediate and general applicability.

An assessment of the book as a whole is difficult, since it depends on one's attitude toward complete coverage of a minor issue vis-à-vis incomplete but imaginative treatment of an impossible one. On the one hand, it is refreshing to see conceptually sound, hard-bitten appraisals of the technical requirements of benefit-cost analysis as applied to the areas in question. If nothing else, the exercises justify themselves by providing seven examples of proper definition of benefits and costs—an achievement much less common than one might expect. On the other hand, it is hard to avoid the conclusion that all of the essays have failed in the sense that the demolition of incorrect concepts and techniques of measurement is not matched by even a good start toward evaluation procedures that are analytically sound and also operational.

Scherer's excellent statement of the problems of evaluating research and development, for example, is followed by a suggested technique that falls almost in the category of "tinker-toy" economics—ingenious but not a particularly useful guide to serious empirical work. Burton Weisbrod's paper on the school dropout provides an elegant analytical framework for empirical study, but appears to force more out of limited data on a single case study than should have been attempted, even before a professional audience. Good work in this field is so rare that even the shabbiest of empirical results are likely to receive public fanfare far beyond the intention of the conscientious author. In brief, all of the papers deserve an A for effort in delineating analytically the problems to be resolved. The fact that none provides, even for limited cases, a pro forma for general application is not an indictment of the author, but rather a testament to some glaring gaps in our factual and statistical information and general neglect of an important area of research.

There are some questionable spots in the book, as one would expect in experimental work of this sort. In several of the papers there was a somewhat surprising tendency to create uniqueness where none really need exist. Scherer's paper, for example, emphasizes the necessity of considering trade-offs relating to time—that is, the possibility of achieving earlier completion of a research and development project by incurring higher costs and the important

limitations on the technological substitutability involved. In his hands, the concept is handled skillfully indeed with respect to research and development, but surely the same problem is involved in education (or, to use the new jargon, investment in human capital) and, for that matter, in many types of sequential investment in physical capital.

A much more serious example is the analysis of the benefits from outdoor recreation in the Mack-Myers paper. There can be no quarrel with the usefulness of their analysis of the internal structure of the demand for outdoor recreation but it does not justify the conclusion that such services are therefore unique. Outdoor recreation, enjoyed in judicious moderation, doubtless contributes to economic welfare after the fact, in subjective satisfaction through subsequent recall and through delayed external effects on others of the physical and emotional well-being generated by the activity. But surely the same reasoning applies to a tasty and dietetically balanced meal, a concert by a competent symphony, and perhaps even the selection of a stylish and properly tailored suit of clothes. If one begins to chase the will-of-the-wisp of secondary and tertiary reactions to specific consumption decisions, the world of microeconomic choice becomes even more of a jungle than it need be. For purposes of benefit-cost analysis the economist is not particularly concerned with the individual components of the experience that lead a consumer to choose a particular type of outdoor recreation service over other uses of income. Rather, he is interested in the aggregated consumer response, the subsequent claim on land and water resources, and the extent to which the net contribution to economic output to be attributed to the resulting activity can be measured.

The real danger in the Mack-Myers analysis of the demand for outdoor recreation is its use as a basis for rejecting out of hand the usefulness of the price system as a guide to investment in recreation facilities. This is clearly one of the many areas in which outputs are not priced as a matter of policy rather than technical necessity. In some respects the excellent technical discussion by Mack and Myers and their interesting and potentially useful alternative to the pricing of outdoor recreation are weakened by this partial lip service to the emotional approach that regards outdoor recreation as a service somehow above the more mundane mechanisms of choice.

Several of the papers either ignore or deal only superficially with the problem of uncertainty. Perhaps this is an outgrowth of the presumed instructions to cast the papers in the framework of benefit-cost analysis, most of which is carried out in terms of conventional static microeconomic theory. Nevertheless, in several of the papers—most notably in those dealing with research and development and urban highway and renewal programs—it is not made completely clear that the degree of uncertainty and alternative methods of dealing with it may alter the whole structure of an investment program, at a point in time and sequentially.

As editor Dorfman points out in his introduction, the battle lines between theorists and practitioners were clearly drawn at the conference, and they are evident in the remarks of the commentators. Some merely confirm the fact that inertia and mythology die hard. Others reflect warranted concern with

the degree of abstraction in the papers and the potentially dangerous errors that may arise from application to situations where key assumptions cannot hold.

Sitton, commenting on Mohrer's analysis of investment in urban highways, raises one such issue that cuts across the whole volume. Where the real world poses government investors with plural aims, can we assume safely that a value system based on segmental optimization of individual choices provides correct answers? Ferguson, dealing with the same paper, raises another intriguing problem that is relevant to most of the other essays. Is it not possible that application of user costs, given present uncertainties, may lead to self-perpetuating overinvestment in some fields, particularly where practical politics make it difficult to prevent full utilization of all revenues in the segment from which they are derived? Further, where government investment alters the structure of society it can, to a considerable extent, "create its own demand."

One word of caution might be introduced in conclusion. This reviewer would align himself firmly with those who argue that economic analysis—as one highly useful and economical method of isolating and evaluating choices—should be pushed much further in the allocation of resources in government investment of the types explored in this volume. But there must be concern lest the enthusiasm for benefit-cost analysis warp the very research process itself. There may well be a tendency, with increasing insistence on justification of government programs in benefit-cost terms, to discriminate against more basic types of research—precisely those where government participation is most productive—and even to alter, not always desirably, priorities for mission-oriented research to favor projects that promise readily quantifiable results. The same argument could be extended to the programming of projects in other fields covered by the authors of the seven papers and might be even more applicable in the Bureau of the Budget's final review of possible departmental trade-offs. Benefit-cost must be regarded as a tool to assist in decision-making, not a rigid framework to which the planning itself should be directed.

JAMES A. CRUTCHFIELD

University of Washington

Federal Lending and Economic Stability. By GEORGE F. BREAK. Washington: The Brookings Institution, 1965. Pp. ix, 185. $5.75.

This study ably develops data and analytical tools needed for appraisal of the past influence of federal credit programs on economic stability and for interpretation of their future potential as an instrument of economic stabilization policy. The book, which is well written and readable, is a basic source for those interested in government credit programs. Because of the careful way in which Professor Break interprets the effects of loan expenditures, his approach may be a useful reference point in connection with discussion of the effects of other stabilization policies. The study reaches interesting conclusions on particular questions, but does not offer any general prescription as to the use to be made of government lending programs.

The central substantive chapter (2) deals with the economic effects of federal lending (effects upon total demand rather than upon resource allocation). In this, the author considers six distinguishing characteristics of such loan programs and derives from his discussion results applicable to six policy questions: (1) a suggested system for improved budget reporting of loan programs; and the conclusions that (2) theoretical analysis does not suffice to support the common belief that loans affect total demand less than other government expenditures; (3) sales of loans by the government during periods of monetary ease and excess unemployment cannot be recommended on stabilization grounds; (4) loan programs are not well suited to be automatic stabilizers; (5) government loan programs designed to fill a private credit gap might desirably be set up as independent agencies financed by sales of their own securities, and (6) terms on government loans should not be based upon the government's borrowing costs but rather upon a system based upon welfare aspects of federal lending (pp. 39-46).

An interesting feature of this chapter is its use of the concept of the "fiscal offset" to net loan expenditures (pp. 19-27). It is pointed out that: "Like any other kind of federal expenditures, loan disbursements can be financed in four ways: by the creation of new money, by borrowing from private investors, by reducing federal expenditures for other purposes, or by increasing taxes" (p. 19). The interpretation, then, involves an effort to identify in particular cases the "fiscal offset" that "financed" a change in loan expenditures. Was a net increase in loan disbursements "financed," in effect, by increasing government borrowing over what it otherwise would have been, by holding other government expenditures below what they otherwise would have been, etc. This is a subtle and demanding interpretive framework, one that does not lead to easy generalization. It involves an effort to answer with reference to each historical situation the question: "If loan expenditures had been different from what they were, what, realistically, would have been the offsetting transaction?" Extended to fiscal policy in general, this approach would involve analysis in terms of alternative packages of net government surplus plus defined financing transactions.

Within this framework, the author's judgment is that during most of the period 1947-62 federal loan disbursements were marginally "financed" by restraint in other expenditures and in tax reductions rather than—the association that might offhand have seemed most plausible—by additional government borrowing. Given that the central constraint on fiscal policy has been concern over the government deficit, this interpretation is not unreasonable. It leads to the observation: "To argue that loan disbursements have frequently been made at the expense of other types of federal spending is to raise the intriguing possibility that expanding loan programs have actually exerted a net deflationary impact on aggregate private demand" (p. 81).

In analysis of the effect upon total demand of "autonomous" changes in investment or government expenditures, an accounting for the financing of the expenditures and its effects has not always been made. Earlier versions of neo-Keynesian theory seemed to justify this on the basis that financing constraints are generally unimportant. Since such a presumption now seems less supporta-

ble, the technique used here by Break may come to be more widely applied, and developed to permit more effective generalization of findings.

A chapter (3) on the characteristic behavior of net expenditures of lending programs interprets this in terms of program life-cycle and of relation to business cycles, including a chronological review of the behavior of lending programs in relation to economic fluctuations. If the test of being adjudged anticyclical in a recession is that major loan programs must show net outlays larger than during the preceding business-cycle expansion, the test is passed in only one of the three recessions covered (pp. 78-79). Within the terms of reference of this study, such a test is to be modified on the basis of an estimate of the "fiscal offsets" to changes in loan programs. The author offers a set of estimates of "most probable fiscal offset to federal loans" in each year (p. 80), but of course is not able to tie this information into any quantitative package.

The study also considers the characteristic behavior patterns of different classes of loan programs in relation to their potentialities as a stabilizing device (Chap. 4) and concludes that some programs would be susceptible to use for stabilization purposes and could generate sizeable swings in net loan disbursements. An appendix gives information on the major lending programs.

J. M. CULBERTSON

University of Wisconsin

The Tax on Value Added. By CLARA K. SULLIVAN. New York: Columbia University Press, 1965. Pp. xii, 340. $10.00.

Dr. Sullivan had an opportunity to write an extremely valuable book on her chosen subject. By and large, value-added taxation has not received much attention from public finance specialists in their treatises or textbooks. At the same time there have recently been important practical developments, with the near-introduction of this tax in Japan and the success story of its operation in France.

Unfortunately, it cannot be said that the author has made the most of her great opportunities, as there are a number of disappointing features of the book. First, it is rather long-winded and at times tedious. Accurate footnotes and references are of great importance, but, to give one example only, it is surely straining the patience of readers to the limit to refer us (p. 221) to a U.S. Treasury source for the proposition that high tax rates create an incentive to evasion. Secondly, the order of treatment could have been better. We start with a 50-page introduction to the concept, switch over to 100 pages on the Japanese and French systems, revert to another 60 pages or so of theory, turn to administration, and finally back to theory again. Some improvement on this seesaw system could surely have been made.

Thirdly, there is a lack of decisiveness in dealing with some of the theoretical issues. These are certainly complicated and no one would dream of denying the need for a cool and balanced approach. But this is a different thing from setting out thesis and antithesis at length, without ever reaching a synthesis. Nailing one's colors to the fence is not always the most profitable of

occupations. Fourthly, despite the author's assurance in the preface that the original Ph.D. dissertation has been revised up to date, there is not much discussion of the recent material in this field, e.g., the Neumark Report of 1963 and the developments in the E.E.C., or the arguments surrounding the Richardson Report (of March 1964) in the United Kingdom.

Obviously, some of these faults are due to the transformation of a Ph.D. thesis into book form, with insufficient editing, pruning, and reshaping. But one suspects that there were also some unresolved difficulties in the presentation of the original thesis. One is that the attempt to combine history of thought with modern analysis was really a form of self-crucifixion. Another is that the author wanders somewhat unhappily from the realm of absolute to that of differential incidence, without ever settling down in either. It surely makes a good deal of difference whether one is dealing with the case for value-added taxation versus sales taxation, value-added taxation versus profits taxation (the genesis of the recent U.K. interest in the subject), or value-added taxation together with extra spending or with higher budget surpluses.

Despite these reservations, one must recognise and pay tribute to the very solid and very real merits of the book. It is certainly comprehensive—at least up to the end of the 1950's; it is carefully and clearly written; and the last thing that anyone could say about it is that it is unscholarly. As a meticulous history of the literature of many countries on this subject, it will unquestionably be definitive. What cannot be said is that it is a simple guide for students or an incisive analysis for theorists.

A. R. Prest

University of Manchester

Nigerian Public Finance. By P. N. C. Okigbo. Evanston: Northwestern University Press, 1965. Pp. xiii, 245. $6.50.

In the last decade Nigeria has not only attained a position of prestige and leadership in African affairs, but also has become a center of attention for students of economic development. With the largest population of any African country, with substantial resources, with (at least until January 1966) a relatively stable government despite the diversities of people and cultural backgrounds, Nigeria offers fruitful ground for study. Though less dynamic in its growth policies than Ghana, and lacking the contributions of the dual economy of Kenya, it had avoided the most serious problems which beset these countries.

While a number of studies of various aspects of Nigerian development have appeared, literature on public finance has been limited to Royal Commission reports, the Orewa book on taxation in the western region, and brief summaries in broader studies of African taxation. Yet government expenditure and revenue programs obviously have great significance for economic growth. This volume by Dr. P. N. C. Okigbo, one of Nigeria's most distinguished economists, fills the gap very effectively. The work is an outgrowth of material prepared by Dr. Okigbo for his work on the Kenya Fiscal Commission of 1962. A thorough review of the development of the public finances of Nigeria serves as

the background for the analysis of the present structure, at federal, regional, and local levels.

Public finance in Nigeria has been greatly influenced by the federal nature of the governmental structure, and intergovernmental fiscal relations have been the center of attention of various Royal Commission and other studies in the country. Since the regions cannot collect adequate funds to meet their expenditure needs, sharing of federally collected revenues has been imperative. Colonial emphasis on allocation of revenue on a derivation basis has given way to increasing attention to need, but conflicts among the regions on this and other issues remain.

Indirect taxation, for the most part federally administered but with revenue shared with the regions, is the major revenue source, as it must be under the circumstances. Customs are still primarily revenue-oriented, but with increased use for protection and foreign-exchange control purposes. Export duties are significant revenue sources. An excellent beginning has been made in the income tax field. Antecedents of this tax are to be found in precolonial Northern Nigeria, and these levies were continued and expanded under the indirect-rule policy and since independence. But Eastern Nigeria was the pioneer in the use of a modern income tax applying to most families, and collected in part through PAYE (withholding). All regions now use the income tax—in the East and the West the tax extends down to the lowest income levels; in the North and Lagos the lower levels are reached by a separate personal tax levy. Local taxation is still somewhat fragmentary, with property tax significant in only a few urban areas. Corporation tax is a federal prerogative.

Okigbo recognizes that indirect taxes must continue to be the primary revenue source, but stresses the need for further development of income taxation, particularly to increase the built-in stabilizing feature of the revenue system. He concludes that the country could raise substantially more revenue to aid economic development without injuring incentives through improved fiscal administration and improved tax structure, but recognizes the dangers to incentives of excessively high taxes. The book stresses the revenue side, but also reviews expenditure patterns. A major thesis of the book is that too many resources are allocated to elementary education and too few to mid-level education.

The book is highly readable; Okigbo has a facility for summarizing complex material without superficiality on the one hand or excessive detail on the other. Only a few questions can be raised. Most of the data do not extend beyond 1962. However, there has been little drastic change since that year, except for the establishment of a fourth regional government, that of the Mid-West Region, with Benin as the capital. The role of the Income Tax Management Act, federal legislation which provides the basic framework for regional income taxation, and the effectiveness of the tax holiday legislation might have received greater attention. But on the whole the book is an excellent contribution to the growing but still limited literature on public finance in tropical Africa and developing areas generally.

JOHN F. DUE

University of Illinois

Macroeconomics, Fiscal Policy, and Economic Growth. By NORMAN F. KEISER. New York: John Wiley and Sons, Inc., 1964. Pp. xx, 525. $9.75.

This book is intended "primarily for use in the Fiscal Policy half of the year course in Public Finance, although it should be easily adapted to other courses with a macro-economics and policy orientation" (Preface, p. vii). This objective separates Professor Keiser's text from numerous others planned more for semester- or year-long courses in macroeconomics, which are broader in scope and which give less coverage to fiscal policy.

The volume is divided into six parts. The four chapters of Part I cover "Goals, Conflicts, and the Role of the Federal Government." After a brief statement of the recent development of fiscal policy, Keiser introduces the fiscal policy goals of full employment, price stability, economic growth, and the balance of international payments. He presents various views of full employment from recent and contemporary economists and public figures. The possible conflicts between the various goals, especially between price stability and full employment and growth, are highlighted.

Part II includes three chapters devoted to income theory. Starting first with only consumption and investment, the multiplier and acceleration principle are developed. Next, government spending and taxation are introduced, followed by the effects of monetary and interest rate changes on the components of national income.

Fiscal policy to raise incomes and to combat inflation is spelled out in detail. Generally, at each step in this part, the new idea is presented via graphs and algebraic equations. Almost every possible change in fiscal policy is illustrated by an example in which likely numerical amounts are substituted in the algebraic equations to show the process by which change, say in taxes or government transfer payments, affects national income and its components. Unfortunately, so many examples are presented that it makes the section unnecessarily long and tedious.

"Problems and Theory of Economic Growth" is the topic of Part III. The first of this part's two chapters includes the factors making for growth, including increases in productive capacity. The Domar and Harrod growth models are presented and appraised. The other chapter compares the potential economic capacity and growth of the economy with the actual amounts, relying heavily on studies of the Council of Economic Advisers, Joint Economic Committee of the Congress, and the National Planning Association. Here, and elsewhere in the book, Keiser emphasizes the lack of sufficient demand as the major cause of excessive unemployment and inadequate growth.

In the next section, Chapter 12 traces out the effects of changes in the level and progressivity of taxes and various government expenditure programs on consumption. The following chapter covers the influence of certain tax changes on investment. Especially good is the treatment of various methods of depreciation, tax credits, and investment allowances relative to incentives to invest. Numerous examples are cited from recent U.S. and Swedish experience.

Part V, "Policy Problems in Fiscal Policy," covering seven chapters of al-

most 200 pages, is the heart of the book. The various measures of the federal budget in the United States are presented critically. The concept of the "full employment surplus" is well covered, together with examples of its size and importance over the last decade. Both the structural transformation thesis and the inadequate saving-investment thesis are presented and refuted. Keiser finds the inadequate demand and full-employment surplus ideas much more convincing.

The strengths and weaknesses of discretionary and automatic stabilizers are presented, and also the problems of forecasting and timing. Possible use of discretionary tax cuts is discussed at length. Separate chapters cover the "Monetary Aspects of Fiscal Policy," "Inflation," and the relation of the public debt to fiscal policy.

The final chapter in this part deals with possible improvements in stabilization policy, emphasizing alternative organizational arrangements for more effective policy in the executive and legislative branches of the federal government, including the Federal Reserve Board.

Part VI covers the history of fiscal policy since 1930, with one chapter dealing with the period up to 1952 and the other chapter covering 1953 to 1962.

This book is strongly oriented toward economic policy issues. Keiser identifies proponents of various viewpoints in his discussion of policies. The economic policies of the national administrations since 1930 are evaluated, with greatest attention to the postwar period. William McChesney Martin receives much attention as a proponent of economic policies which Keiser thinks are often unwise. A three-page extract (pp. 46-49) of an interchange between Martin and the late Senator Kerr from a Senate Finance Committee hearing is included, and also a three-page appendix (pp. 405-7) devoted entirely to a critical presentation of Martin's views.

References to recent and current journal articles and reports of the Council of Economic Advisers and the Joint Economic Committee give teachers and students abundant sources to develop different ideas and controversies much more fully. This is a major feature of the volume. Empirical data are generously provided, with numerous tables and graphs presenting income, employment, prices, and various measures of the federal budget. For example, quarterly GNP data in constant dollars from 1947 through the first quarter of 1963 are presented in a six-page table.

Keiser devotes very little attention to the balance of payments. He does identify the balance of payments as one of the four problems which are the major objectives of fiscal policy early in the book. For the remainder of the volume, no significant attention is given to possible uses of fiscal policy to meet the balance-of-payments form.

The text is concerned almost exclusively with the recent fiscal policy in the United States. Except for a reference to the Swedish investment reserve, the examples of fiscal policy are chosen from U.S. experience. Fiscal policy for countries with other institutional arrangements and at other stages in their economic development is not discussed.

Keiser writes as a political economist. He makes his value judgments clear; he evaluates as well as describes the fiscal and economic policies in the United

States. I felt he was unfair at one point where he describes at some length the Eisenhower administration's fiscal policy in 1953-54, emphasizing the reduction of federal purchases of almost $15 billion but omitting on these pages (pp. 494-95) any reference to the end of the Korean War, which accounted for a large part of the decline in expenditures.

I believe the book would be much improved if the material had been covered in fewer pages. It would be further improved if the space saved were used to broaden the coverage of fiscal policy to other topics and types of economies as suggested above.

All in all, Keiser has presented an adequate text for the course for which he planned it. It offers abundant opportunities for lively discussions of tax, expenditure, debt, and monetary policies in the United States since 1930, especially if used with readings which highlight different points of view.

WILLIAM F. HELLMUTH, JR.

Oberlin College

International Economics

The Balance of Payments Statistics of the United States—A Review and Appraisal. Report of the Review Committee for Balance of Payments Statistics to the Bureau of the Budget, April 1965. Washington: Superintendent of Documents, 1965. Pp. xiii, 194. Paper, $1.50.

In recent years so much has been written and so much has been done about the deficit in the U.S. balance of payments that it must come as a surprise to the outsider to learn that the experts have been very far from agreement as to how it should be measured. The Bureau of the Budget appointed a committee[1] to review balance-of-payments statistics in April 1963. The Committee submitted its unanimous report two years later. A subcommittee of the Joint Economic Committee, after hearing a number of witnesses, submitted its recommendations, and the Department of Commerce, together with the Budget Bureau, has recently announced its intention of implementing many of these recommendations. The most interesting recommendation—that henceforth the official measure of the deficit be given in two forms—is shortly to be put into effect. The Department of Commerce will continue to measure the imbalance in accord with the "liquidity" concept it has been using in recent years. But it will also provide an alternative measure of the imbalance employing the "official settlements" concept urged by the Bernstein Committee. With two figures of the deficit to choose from, one can only hope that the masochistic drive which seems to imbue those who have responsibility for policy in this field will not lead any of them to add the two figures in order to get a proper measure of the deficit.

Many of the Report's findings, though important, are scarcely controversial. The Committee has appraised the basic data which go into measuring the balance of payments and has found serious deficiencies—surely far more of them that most users have suspected. The accuracy of the figures even for

[1] The members of the Committee were Richard E. Caves, George Garvy, Walter E. Hoadley, Harry G. Johnson, Peter B. Kenen, Roy L. Reierson, and Charles F. Schwartz, with Edward M. Bernstein as chairman.

merchandise exports and imports is questioned: "The procedures which generate the export and import statistics, even if perfectly administered, would still provide data that would need adjustment for which the necessary information is not now at hand" (p. 33). Valuations are suspect for both exports and imports. Data on travel expenditures are subject to doubt. The information about government transactions is far from clear-cut, especially in respect to timing. Figures on direct investments are uncertain, and those for portfolio investments are no better. And the balancing item "Errors and Omissions" has been far too large, and its changes in certain periods have been so big as to make any analysis of the forces operating on the balance of payments impossible. Moreover, this category probably contains an item which is of special importance in measuring the imbalance on the basis of the liquidity concept; but by its very nature the size of that item cannot be ascertained.

There can be no doubt about the enormous value of the Committee's work in spotlighting deficiencies in the data and in suggesting remedies. Obviously not all its suggestions for improvement will be adopted, largely because of the expense involved, but the Committee seems to have shown good judgment in making its appraisals about data being collected and recommendations for their improvement. From now on if we are going to follow their particular set of guides, we will have some reason to hope that they are adequate. The Report also recommends changes in the format of the balance-of-payments accounts which should make them more convenient to use. On these technical matters the members of the Committee have done an excellent job.

But the Report has attracted most attention because of what it had to say about the conceptual issues: how should a balance-of-payment disequilibrium be measured in the accounts? Here too the Report makes an important contribution in raising questions and sharpening issues, although it is by no means clear that the Committee has found the best solution.

If *all* the payments and *all* the receipts of a country were measured accurately, the totals of each would be identical. If an imbalance is to be detected, it is necessary to isolate those items in the record that give evidence of an imbalance, putting them "below the line." Then the difference between the payment items and the receipt items that are left above the line (the deficit—if the difference is positive) must equal the corresponding difference for the entries below the line (the means of financing or settling the deficit). The one big question is where the line ought to be drawn.

The Department of Commerce of course puts change in U.S. holdings of gold and convertible currencies and in its IMF position below the line, and recently it has also put there changes in "liquid" U.S. liabilities to foreigners. Incidentally, the Committee does well to remind us that the official concept itself has been modified from time to time. The Bernstein Committee recommends as an alternative that the below-the-line items include changes in: U.S. holdings of gold and convertible currencies, its IMF position, and foreign official monetary institutions' holdings of U.S. government bonds and notes and short-term capital (in the United States). Thus the difference between these two measures is that the liquidity concept treats every increase in foreign *private* (and official but *nonmonetary institutions'*) holdings of short-term

capital in the United States and U.S. government bonds and notes as *belonging below the line,* while the official settlements concept puts these items above the line and hence regards them not as a means of financing a deficit but instead as a receipt which reduces the deficit. With relatively few exceptions, whether on a seasonally adjusted or unadjusted basis, the official settlements concept has shown a smaller deficit over the last decade than the liquidity concept, and this difference may be expected in the future.

Obviously there must be more compelling reasons for shifting to a different concept than that it provides for a lowered measure of the deficit. Here, I suggest, the Committee has stopped short of the solution to which its logic was leading. The official settlements concept, assuming that any measure of the imbalance is to be used, is superior to the liquidity concept; but it seems to me to have its own faults. To establish this, I shall first consider very briefly the notion of a balance-of-payments disequilibrium, and then examine its measurement for a country in three different situations: (a) when the country is not a reserve-currency country; (b) when the country is the reserve-currency country performing the functions of a bank for others, and their institutions and policies allow this activity to be performed; and finally (c) when the country with whose balance of payments we are concerned serves as the reserve-currency and banker country but does so in a world which fails to act so as to allow it to function in this role. This last situation is partly the one in which we find ourselves today.

The economists' concern with a balance-of-payments disequilibrium arises because the situation is one that cannot be sustained indefinitely. If a deficit situation could last forever with both the deficit and surplus countries acquiescing, there would be no disequilibrium whatsoever, and the term "deficit" in this context would be devoid of all operational significance. Thus the figure for the deficit ought to be a measure of the pressures that prevent a particular situation from continuing indefinitely.

If we supposed that a deficit had to be completely financed by paying out gold and that all other receipts—from borrowing or anything else—were sustainable, the measure of the deficit would be simple. Since a country's reserve of gold is necessarily finite, and a gold outflow could not be continued indefinitely, the gold drain would serve perfectly to measure the deficit. But in the real world, a country keeps reserves in other forms; moreover it may not be able to count upon an indefinite flow of receipts from some of its operations. On these accounts, the measure of the deficit of even a simple nonreserve-currency country is open to question. Most countries implicitly assume, when settling their accounts, that they are unable to increase their net short-term liabilities, but that they can continue to import long-term capital indefinitely. Accordingly, they treat their long-term capital imports as above-the-line receipts; while they put their *net* short-term capital imports below the line, regarding them as a method of financing their deficit. But these assumptions appear to be less than fully convincing. A moderate increase in short-term interest rates perhaps is all that is needed to stimulate short-term capital imports; it may be far more difficult to encourage the import of long-term capital. And there is no reason to suppose that a so-called long-term capital

import is less likely to be quickly reversed than a short-term one. Although it may be convenient to distinguish between sustainable capital imports and others by classifying all long-term capital imports under the former heading, and all short-term capital imports as temporary, such a criterion, unfortunately, can be decidedly misleading. Nevertheless, as we have said, many countries tend to follow it.[2] Thus, measuring the deficit in a meaningful way for the ordinary country is by no means as simple a task as we frequently suppose. And it is doubtful whether the deficit as it is commonly measured really provides the clear evidence for disequilibrium that we want. But we might note in passing that for such a country the liquidity concept is quite inappropriate.

When the task is that of measuring the deficit for the reserve-currency or banker country, the difficulties are multiplied. The characteristic of the banker country is that its short-term liabilities constitute a special kind of asset for private and official institutions abroad. And not only does the banker country supply such an asset (analogous to the commerical bank's role in making demand deposits available), but it also directs "finance"; after collecting it from a large number of sources it divides it amongst the various claimants. Presumably the accounts of the banker country should then be different too because they should reflect the special value its short-term liabilities to other countries possess for the claimants. Incidentally the Bernstein Committee, which was aware of the special role of the United States, argued that we should adopt the official settlements concept partly because we would, by doing so, be making use of the concept now employed by many other countries. But other countries are not reserve-currency countries, and it makes no sense to urge the United States to keep its accounts as they do.

If there were a complete acceptance by other countries of the U.S. role as banker, it would be proper to treat a rise in our short-term liabilities to others, whether to private or to official institutions, as an above-the-line capital import. Indeed, the Bernstein Committee accepts this much of the argument, at least for everything but our liabilities to foreign official monetary institutions. It recommends that a rise in foreign short-term private, or official but nonmonetary, claims against us be treated as an above-the-line capital import. But if this much of the argument is valid, there is no reason for treating an increase in the short-term (or long-term) claims of foreign official monetary institutions differently. Central banks, having regard to their responsibilities, have as much reason for accumulating reserves as private firms. If they hold their reserve in dollars, an increase in our dollar liabilities to them is no less sustainable than is an increase in dollar liabilities to private firms. To argue that the private firm has a liquidity motive for permitting its dollar claims to accumulate while a foreign central bank allows its dollar claims to increase only in order to prevent an appreciation of its currency is to pretend that the cen-

[2] Some countries base the classification upon whether the capital importer is official or private, with capital inflows to private individuals and institutions being treated as sustainable while an increase in foreign claims of official institutions is not. But this is no easier to justify.

tral bank is a purely passive recipient which has no need for reserves of the reserve-currency. But this is scarcely compatible with the view that the United States is the reserve-currency country. For this reason, I believe that the distinction between an increase in the claims against us of foreign official monetary institutions and of all others is not helpful.[3] For the reserve-currency country some increase in liabilities whether short term or long term and whether to official or private institutions should be regarded as an above-the-line capital import. But this does not mean that every increase in these liabilities should be treated in this way, without limit.

While it may be true that any country or institution would prefer larger reserves to smaller, other things being equal, too rapid an increase in (or too large a volume of) reserves can be damaging. Reserves can be too high just as they can be insufficient, and the bank or the reserve-currency country must exercise responsibility to keep them at the appropriate level. It is this consideration that should determine how much short-term capital the reserve-currency country should import. If it increases its short-term liabilities at a more rapid rate than this, it creates a disequilibrium situation, and its *excess* of short-term capital imports should be put below the line. And if it imports the proper amount, and thus generates the proper increase in foreign reserves, its balance-of-payment accounts should be in equilibrium whether the creditors are private or public, monetary or not. But if we were to follow the Bernstein Committee, and were to place all short-term capital imports except for those from foreign official monetary institutions above the line, our measures could point to equilibrium even though the increase in world reserves were far too fast or were quite inadequate.

The objection will undoubtedly be raised that we have no way of determining how large the world's reserves ought to be. Admittedly the decision would be somewhat arbitrary, just as it is for those who set domestic monetary policy; but the mere fact that an element of judgment is needed does not sentence the monetary authority to immobility. And in the brave new world of the Triffin, or Roosa, or even the Bernstein Plan, decisions like these would have to be taken.

There is another limitation to which we should be responsive at the present time. Some countries, France for example, are willing to hold only a limited amount of dollars in their official reserves. This means that for such countries the dollar does not fully serve as a reserve currency.[4] Then, short-term capital imports from France's monetary institutions would have to be placed below the line. But this would not be a problem, since a central bank which is unwilling to add to its dollar reserves presumably converts them into gold directly. So even in our imperfect world the conclusion would remain unchanged.

[3] A second objection to the Bernstein Committee's suggestion is that these claims are shifted from foreign central banks to their commercial banks and vice versa, for reasons which have nothing to do with us or our policy. And yet with every such shift our balance of payments would be affected.

[4] Insofar as France's private firms are prepared to hold dollars, the dollar is of course a reserve currency.

U.S. short-term capital imports, from whatever source, ought to be put above the line so long as they are not excessive in terms of the need for reserves of foreign private and official institutions. Only the excess belongs below the line. And it is no more arbitrary to decide that short-term capital imports of less than, say, $800 million a year be put above the line than it is to classify *all* short-term capital imports except those from official monetary institutions as above-the-line entries; it is no more arbitrary and it makes more sense.[8]

The Bernstein Committee has without a doubt performed an invaluable service—to the formulation of policy and to economists too. I hope there will, however, be a next step in which the official settlements concept is replaced by one which comes closer to providing a measure of the distance from disequilibrium of the reserve-currency country. We have still, I believe, a good way to go.

LORIE TARSHIS

Stanford University

Money in the International Order. Edited by J. CARTER MURPHY. Dallas: Southern Methodist University Press, 1964. Pp. ix, 110. $3.00.

Hardly a week passes without the publication of a new monograph, collection of essays, the results of a conference or symposium, or hearings before a Congressional Committee, etc., on the subject of the international monetary system, balance of payments, international liquidity, and all that. The present volume contains lectures by Walter S. Salant, Frank A. Southard, Jr., Lawrence B. Krause, Robert A. Mundell, Attiat A. Farag and David J. Ott, presented at a conference on these problems in March 1964. For the expert, the meat is in the essays of Salant and Mundell. Southard's paper, "International Monetary Arrangements," gives a short, readable but nontechnical description of the basic institutions and modes of operation of "the international monetary system today." Krause discusses on twelve pages some implications for the demand and supply of international liquidity of "the fact that five of the world's convertible currencies are issued by countries joined together through the Treaty of Rome"—a fact that is, in his opinion, often overlooked but turns out to have precious little to do with international liquidity, at least at the present stage of integration of the EEC countries. The paper by Farag and Ott, "Exchange Rate Determination Under Fluctuating Exchange Rates: Some Empirical Evidence," presents in eighteen pages the results of a few econometric exercises. The conclusion that "the study does not yield many sweeping generalizations" shows disarming modesty, but is in fact an overstatement.

Salant's article, "Does the International Monetary System Need Reform?"

[8] Bernstein in his testimony before the Subcommittee on Statistics of the Joint Economic Committee (May 11, 1965) stated: "But I think that by the definition of the Balance of Payments Division, some deficit between $500 million, say, and a billion dollars, might be regarded as an average over a long run that could be sustained by the economy indefinitely without any adverse consequences." While his figure seems small, since it would provide for a growth in world reserves of no more than 2½ to 3½ per cent a year, it does at least imply that an estimate is possible.

(the longest in the series, thirty pages), is an able, succinct, and useful summary, elucidation, and defense of the well-known Brookings book, *The U.S. Balance of Payments in 1968,* and touches on all the major problems. He defines the functions of an international monetary system: It should not interfere with free trade and payments and should provide sufficient liquidity without endangering full employment or price stability. Then the present system is tested in the light of these criteria and is found wanting. Its defect is that it does not supply sufficient liquidity. The meaning of adequate liquidiy is carefully discussed and the conclusion reached that international liquidity has been scarce already "for some years." "I accept the definition that 'adequacy' means an amount of liquidity which permits the sum of the subjective valuations of excessive accumulations to equal the sum of subjective valuations of deficiencies" (p. 25)—hardly an operational definition. "This definition implies that liquidity is insufficient if it does not put as much pressure on surplus countries to reduce their surpluses as it puts on deficit countries to reduce their deficits." "This condition exists now."

It has been objected that the surplus countries in Europe have actually accepted a good deal of inflation, i.e., "much pressure was put on them." The author's counter argument is that they should reduce import barriers, contribute more to defense and to aid to less developed countries. While he is right that these steps would at the same time relieve inflation in the surplus countries, unemployment in the deficit countries, and the balance-of-payments disequilibria, it is really not very helpful to suggest all sorts of structural reforms and basic policy changes, desirable as they may be, to justify inflationary demands for increasing international liquidity.

The adjustment mechanism is not discussed at great length. Exchange flexibility is rejected and it is assumed that the mechanism works, but works slowly. Hence large reserves are required to provide sufficient time for smooth adjustment. In that, the Brookings group agrees with the gold standard advocates who, too, blame the fractional reserve system. Both are subject to the criticism that, if countries are somehow provided with large reserves, at least some of them will spend at least part of their reserves and go back to the fractional reserve system. Salant is aware of this difficulty but argues that the developed industrial countries have and will show sufficient discipline to withstand that temptation. I cannot share that optimism and it would be easy to quote cases where large reserves have been lost in a few years through inflationary policies. But this does not alter the fact that Salant's is a stimulating and thought-provoking contribution which will force many readers to rethink and reappraise problems that they had already put away in their minds as having definitely been solved and needing no further consideration.

While Salant deals primarily with the liquidity problem, Mundell concentrates entirely on the problem of adjustment which he calls, quite rightly in my opinion, the more fundamental and difficult of the two. He contrasts the fixed rate with flexible exchange rate systems, both with respect to spot and forward rates. The present system is really a mixed one, because only spot rates are fixed while forward rates are free. But in my opinion, the author should have pointed out that the flexibility of the forward rate lends only very

little flexibility to the system as a whole, because the forward flexibility cannot amount to much so long as the spot rate is fixed and is generally accepted as unalterable.

But the core of Mundell's contribution is the attempted demonstration that the relative effectiveness of monetary and fiscal policies in stabilizing employment and equilibrating the balance of payments is radically different under the system of fixed rates and the system of flexible rates, *if there is free international mobility of capital.* It may strike the reader as paradoxical that, given the italicized condition, under fixed rates the effect of monetary policy on employment is said to be nil while fiscal policy is highly effective, whereas under flexible rates it is the other way round. The reason why this sounds paradoxical is that we are not used to assuming perfect mobility of capital. If it exists, it means that under fixed rates an attempt on the part of the monetary authorities to tighten money simply leads to large capital imports, a result which improves the balance of payments but leaves the supply price of capital unchanged and hence does not stimulate employment. Similarly, an attempt to ease money simply chases capital out of the country, thus changing the balance of payments but having no effect on employment.

It certainly is useful to think through these various cases. But in practice capital is never perfectly mobile and there are, of course, different types of money capital (short term, long term, mortgage capital, etc.) with different degrees of mobility. It would, therefore, be dangerous if central bank governors and ministers of finance took the author's advice literally and memorized and applied mechanically the rules which he lays down in a "policy table" on page 73. Also, the application to the Canadian policy between 1959 and 1962 seems to me misleading. The author is certainly right when he says "that the Canadian experience brought discredit to the flexible exchange rate system—unjustly because the *system* was blamed instead of the *inappropriate policies followed under the system.* In 1959, when Canada experienced severe unemployment, monetary policy was tightened" (p. 73, italics in original). Assuredly, to tighten money and raise interest rates in a depression is perverse. But it would have been perverse also under a fixed rate system. Easy money plus a budget deficit would have been the right policy whilst the author says credit should have been expanding ". . . to reduce the budget deficit" (p. 74).

But despite a certain oversophistication, which makes it dangerous advice to less sophisticated policy makers, Mundell's paper is exceedingly stimulating for a theorist. There is a useful appendix setting out in ten points the arguments against flexible exchange rates and the rebuttal.

GOTTFRIED HABERLER

Harvard University

Multilateral Commercial Diplomacy—The General Agreement on Tariffs and Trade and Its Impact on National Commercial Policies and Techniques. By GERARD CURZON. London: Michael Joseph, 1965. Pp. xii, 367. 60*s.*

This thorough study of the General Agreement on Tariffs and Trade begins with a short historical introduction. Chapter 2 discusses the organization of

GATT, Chapter 3 the most-favored-nation clause, and Chapters 4 and 5 tariff problems. Barriers to trade other than tariffs are discussed in two succeeding chapters: quantitative restrictions in Chapter 6, agricultural protection in Chapter 7. The next three chapters are devoted to GATT actions in the fields of economic development, economic regionalism, and state trading. The book concludes with a brief discussion of prospects, including GATT's relations to UNCTAD.

Although the author announces early that he will resist the temptation to enter into the welfare theory of international trade, the book abounds in welfare pronouncements, by no means confined to international trade. (E.g., there is talk of the "vicious circle of planning" in underdeveloped countries; "non-collectivist countries' trade assures that factors of production are used to their optimum within the nation. This principle is extended to foreign trade as well. . ." And we are told that "a good case can probably be made for substituting unilateral trade concessions for aid by the rich countries.")

In spite of the author's belief that economic theory has established the case for free trade (which he supports by mutually exclusive arguments as if they were additive), while political obstacles prevent the realization of this ideal, the case for GATT and freer trade is probably philosophical and political rather than economic.

A system of rules on which a large number of countries can agree, so the argument runs, must be simple. More complex rules, though they may be preferable on economic grounds, are more open to abuse and more difficult to police. It is presumably for such reasons, rather than on theoretical grounds, that discrimination in favor of nationals is permitted, while discrimination between foreigners is prohibited; that 100 per cent preference is permitted in free trade areas and customs unions, while anything short of it is ruled out; or that preferential free trade must apply, in principle, to all products and must not be confined to, say, new production. (The interpretation of the new Chapter 4 is still too uncertain to cast doubt on this statement.) It is well known that none of these principles can be justified by welfare *economic* arguments. Discrimination in favor of the domestic market makes nondiscrimination between foreigners no longer optimal; partial preferences are more likely to be beneficial than 100 per cent preferences, etc. Even more difficult to understand is the rule that, temporary restrictions apart, protection should be provided exclusively through tariffs, not through other commercial measures. Not only economic but also political arguments point to the advantages of subsidies over tariffs (and surcharges over quantitative restrictions). But Gerard Curzon has not yet even advanced to the Neo-Panglossian position that all is for the second-best in the best of all feasible worlds (and everything in it is a necessary constraint). Nor is the author more convincing in his attempt to persuade us that the GATT bargaining procedure is the most promising approach to freer trade. He does not meet the difficulty that in the bargaining process high-tariff countries have an advantage over low-tariff countries. This means not only a conflict of interests but also a built-in tendency to resist tariff reductions, which would weaken future bargaining power.

Not only the economic but also the *philosophical* argument for simple and

general rules is fallacious. The political philosophy of GATT is founded upon a long-standing confusion, of which even such a clear-headed thinker as David Hume is guilty. Hume contrasts the highly specific reactions when we are seeking our own self-interest with the "universal and perfectly inflexible" laws of justice. He seems, like many others (including GATT), not to make a necessary distinction between *general* principles (the opposite of specific ones and therefore necessarily simple) and *universal* principles (which may be highly specific and highly complicated, provided that they contain no uneliminable reference to individual cases). Thus, Hume says, in one place "universal and perfectly inflexible," but lower down "general and inflexible." And the use of the word "inflexible" conceals a confusion between a principle being able to be *altered* (which has nothing to do with its universality *or* generality) and its having a lot of exceptions written into it (which is consistent with universality but not with generality). Hume evidently thinks that the rules of justice have to be simple, general ones. He argues that unless the rules are *general,* people will be *partial* in their application of them and "would take into consideration the characters and circumstances of the persons, as well as the general nature of the question . . . but this would produce an infinite confusion in human society, and . . . the avidity and partiality of men would quickly bring disorder into the world, if not restrained by some general and inflexible principles." But this is fallacious. In order to prevent people from being partial, the principles have to be universal, i.e., not contain references to individuals; they may, and indeed should, not be general; surely our judgments based on them *ought* to "take into consideration the characters and circumstances of the persons, as well as the general nature of the question."

Like Hume, the philosophy of GATT, by confusing "general" with "universal," arrives at the wrong conclusions of the need for simplicity. A highly complex system of multi-tier preferences may well be best and most just for a group of developing countries at different stages of development entering into a union.

The descriptive content of the book is valuable. History, institutions, and the adaptations of GATT to new requirements are admirably set out in great detail, and the author has brought together material previously dispersed over a large number of sources.

PAUL STREETEN

Balliol College, Oxford

Germany's Economic Dilemma: Inflation and the Balance of Payments. By PATRICK BOARMAN. New Haven: Yale University Press, 1964. Pp. 344. $7.50.

This is an extremely interesting study of Germany's problems in coping with the inflationary consequences of its enormous export surpluses in the 1950's. It provides a fascinating case study of the difficulties encountered in trying to combine independent domestic monetary-fiscal policies, fixed exchange rates, and an equilibrium in international payments. It gives us not a chronicle of events, but a thoughtful discussion of the policy issues as they ap-

peared to German economists and the monetary authorities at the time. It is particularly successful in communicating the *flavor* of the German balance-of-payments controversies as they occurred.

Professor Boarman's own retrospective judgments on those controversies and his own analyses and generalizations about the substantive economic issues involved, while evincing wide scholarship, dialectic skill, and an expert handling of both statistical and historical data, seem, nevertheless, distinctly less original and less successful, on the whole, than his discussion of the evolution of German thought about the problems. He unduly ignores broad sociopolitical factors that were overlooked by all sides in the technical economic controversies. Moreover, the book's fiery ideological commitment to the *Soziale Marktwirtschaft* philosophy of Ludwig Erhard and Wilhelm Röpke (which is invariably shown as providing the right answer) gives it a somewhat doctrinaire character.

Germany's export surpluses were truly enormous. From 1952 to 1961 they averaged (in goods and services) 3 per cent of GNP—about five times the U.S. ratio. Boarman argues that they resulted in the first instance from the practice of the neoliberal economic philosophy, which by means of tight money and budgetary surpluses, stabilized prices, stimulated private saving and investment, but made it easier and more profitable to sell in foreign than in domestic markets. Moreover, by spurring competition and freeing the price system from arbitrary governmental controls, the German economy was made extremely flexible and efficient in discovering and responding to the particular requirements of its individual customers both domestic and foreign. Boarman's discussion of structural and nonprice factors in German export competitiveness and their relation to domestic economic policy is particularly interesting and suggestive.

These giant surpluses, however, had powerful inflationary effects which the Germans, with their deep emotional horror of inflation, were determined to avoid. The monetary authorities therefore raised interest rates very high and the government ran large budget surpluses. The high interest rates attracted an inflow of short-term capital, adding still further to the balance-of-payments surplus and requiring further credit stringency to sterilize the expanded monetary base. The large budget surpluses and the tightening of domestic credit restrained domestic purchases and costs, discouraging imports and spurring exports—on which profit margins were higher and which could often be financed by foreign funds. In short, the effort to combat inflation by traditional means resulted in the "import of inflation" from Germany's neighbors who were pursuing more expansionist monetary policies. This put the German authorities in an apparently insoluble dilemma. Not only did the attempt to fight inflation apparently result in still more inflation, but it drew criticism from abroad that Germany was not acting either by the old gold standard "rules of the game" or by the new standards of "a good creditor." Furthermore, it involved restraining domestic consumption and investment in Germany for the benefit of countries which in most cases had higher consumption levels and more capital than Germany.

"A most ingenious paradox" to all appearances! And there is much truth in this picture. Is it, however, the whole story? Despite Boarman's persuasive presentation, I cannot really believe that the Germans very much wanted to give up their export surplus or that they would have faced any insuperable difficulty in doing so.

Consider the first point. According to Boarman: "Nothing could have been farther from the truth than the assertion that the Germans were anxious to add to their holdings of gold and exchange. Their incessant concern, as has been shown, was to get rid of such 'wealth' where this could be done without producing a domestic inflation. If Germany were a miser, she was, like Midas, more deserving of sympathy than of anger" (p.251). To me, this view seems naïve. To be sure, certain German economists and public officials said such things. But one risks major error if one ignores the psychological-political contexts in which economic policy is made. The book completely abstracts from the fact that Germany, a nation with a formidable power-drive, had just lost a disastrous war and been deprived of decisive weapons. It seems entirely unreal to discuss German economic policy without realizing that Germany turned naturally to the pursuit of power by the one means left fully open to her—both on the individual and on the collective level—namely by the acquisition of wealth. In this context it is easy to understand both the high level of saving among individuals, and the major national effort made by Germany to re-establish itself as a financial power by acquiring large stocks of gold and foreign exchange. Such accumulation had of necessity to be at the expense of current consumption—but a restoration of power had an unquestioned priority. Some restraint in consumption was not too painful in any case in view of the great improvement achieved over early postwar levels. A too-flamboyant rise in consumption levels, moreover, might have been resented by Germany's neighbors whose own economies had been damaged by German attack and occupation, and whose citizens had accepted taxation and restrictions to help restore the German economy and defend its security.

Boarman incidentally fails to indicate the extent to which the German export surpluses can be explained by allied military contributions to her defense. Relevant data, which are introduced only in connection with a minor point (the possible disadvantages of revaluation), and the broader significance of which is not discussed, show that *receipts from foreign troops accounted for 40 per cent of net surpluses on goods and services* in the period 1951 to 1960 (Table 23, p.293). The book does not mention at all that as a result of allied contributions to Germany's defense, Germany spent only 3.5 per cent of GNP on defense during these years, compared to 6 per cent by France, 7.5 per cent by the U.K., and 10 per cent by the United States. This 2.5-6.5 per cent of GNP saved on defense relative to her main commercial rivals—much of which directly benefited her balance of payments and burdened those of her rivals—will certainly go a considerable way to explain how it was possible for Germany to achieve her export surplus of 3 per cent of GNP in these years.

I also find it hard to believe that Germany could not have found the means to reduce her export surplus if she had been determined to do so. Boarman

spells out in great detail the multitudinous objections to various adjustment policies that might have been adopted. They do not seem to me to be decisive: essentially they add up to an unwillingness to pay the cost of abandoning the export surplus. It is admitted for example that the export surplus was largely based on an abnormally low German propensity to import—in turn resting on the very low import content of German exports, and the abnormally low ratio of consumption to GNP. All the more reason, one would suppose, for eliminating import barriers. Boarman's main argument here is that Germany led its neighbors in liberalization and tariff reductions. This is true, but doesn't quite answer the point: if Germany had been really determined to end its export surplus it could have gone even further and faster, especially on agricultural imports.

Similarly, more effective measures could have been taken, one would suppose, to reduce the severe inequality in income distribution in Germany and to reduce unemployment (which stayed above 3 per cent until after 1958), with benefit to personal consumption and imports. Such measures could have been justified under the social justice aspect of the *Soziale Marktwirtschaft* philosophy. Röpke's argument that unemployment could not be further reduced until there were more savings and capital seems absurd; temporary public works projects of low capital intensity, and credits to finance imports of the required equipment for private projects could certainly have helped—*if* there were a real willingness and desire to sacrifice the export surpluses. The real emotional objection to such policies comes out in comments on the unreasonableness of expecting surplus nations to adjust themselves to the inflationary developments in the deficit countries rather than vice versa.

As for the frequently heard request that Germany reduce its surpluses by contributing more to foreign aid, this is parried by the explanation that the German export surplus *was* an indirect contribution to foreign aid: i.e., by shifting resources to the aid-giving nations, the accumulation of U.S. dollars in German reserves was "equivalent to an involuntary financing by the Germans of portions of American military and economic aid programs"—programs which they themselves would sometimes have rejected. It is also argued that Germany was too deficient in capital (as proved by the high interest rates) to afford foreign aid.

One final prescription—of a wholly orthodox type—was always available, but continuously rejected until 1961, and then adopted only in a homeopathic dose. This was the prescription of an upward currency revaluation, advocated by Röpke as early as 1956, and held off, Boarman admits, largely through the implacable political opposition of the export interests and their banks. Can one believe, then, that the politically dominant forces in Germany really wanted to rid themselves of their export surpluses or were really displeased by the rise of Germany, by 1955, to the position of the second country in the world in the size of its gold and foreign currency reserves?

Emile Benoit

Columbia University

Japan and the United States in World Trade. By WARREN S. HUNSBERGER. New York and Evanston: Harper & Row, 1964. Pp. xvii, 492. $6.00.

"This study of Japan's foreign economic relations seeks to illuminate not only the Japanese side of the story but also American interests and the issues of American policy involved" (p. vii). The main focus is on the period 1952-62 (the cut-off for statistical data). Subsequent events have not altered substantially the trends detected by the author or the validity of his policy conclusions. Warren Hunsberger emphasizes the overwhelming economic significance of the United States for Japan as its largest trading partner, main foreign source of new technology and capital, and perhaps most important as patron in international economic relations. Japan has become of increasing political and economic importance to the United States as well. America's thirteenth largest trading partner in 1952, Japan in the past few years has been second only to Canada.

Part I, "Japan in the World Economy," uses Japan's postwar balance of payments as the framework for discussion. The author stresses the structural importance of imports as inputs for Japan's industrial production process, and of foreign capital and foreign technology. Most of the discussion centers on the nontrade components of Japan's balance of payments: U.S. military and economic aid, special procurement, reparations, various kinds of short-term and long-term capital inflow, and arrangements for the inflow of foreign technology. Part II, "Japanese Commodity Trade," straightforwardly describes the growth, commodity and geographical composition, controls, prices, and policies of Japanese imports and exports. Two following chapters treat the U.S. market for Japanese goods in general, and cotton textiles in particular.

In Part III, "Policy Needs and Possibilities," Hunsberger begins by stating in rather broad, policy-oriented terms the general desirability of trade liberalization and specialization based on comparative advantage. He then examines Japan's reservations against trade liberalization, and points out that Japan has become sufficiently strong that it no longer requires extensive U.S. sponsorship. Rather, it can be expected to develop its own foreign economic policy. After a discussion of the "costs" to the United States of trade with Japan, the author suggests that U.S. and Japanese economic interests are basically harmonious—both want flourishing multilateral world trade.

A striking feature of U.S.-Japan trade relations is that while most imports from the United States do not compete directly with Japanese production, almost all imports from Japan compete vigorously via prices with U.S. production. In certain industries competition has been so severe as to arouse strong cries of market disruption and attempts to obtain protection. Hunsberger examines the resultant action on tariff and quota restrictions, and on the Japanese development of the "voluntary" export quota system in hard negotiation with the U.S. government as representative of the cotton textile and other industries. Export quotas are easier to adjust and to expand than other forms of trade restriction, but they too are restrictive. The proper evaluation of export quotas depends on whether protectionists could have obtained other restrictions. Hunsberger is not clear, but seems to feel that U.S. protectionists would

have won the day in the late 1950's. Japan has opened a real Pandora's Box, with producers of other commodities and other countries pressing for similar limitation on exports by exporters. This technique may well presage the way in which the high-income countries will handle the imports of highly competitive, labor-intensive manufactured goods from low-income countries. Indeed, as Hunsberger points out, the nominally liberal but in practice protectionist international cotton textile agreement bodes ill for the poor countries.

Writing style (avoidance of technical economic terminology) and the general level of discussion suggest that this book was written for an audience of intelligent laymen. The author provides a welter of factual descriptive detail, involving exhaustive synthesis of data and analysis from a variety of sources. A statistical appendix and an extensive bibliographic note on English-language materials (many of which emanate from Japan) are particularly useful reference materials. From all this Hunsberger has rather loosely drawn some general policy conclusions and recommendations. These reflect more the author's liberal trade position and optimism than a detailed analysis of costs and benefits of particular policies. Nonetheless, I find his conclusions reasonable and full of common sense: Hunsberger presents a good case to his audience.

Economists, however, will be disappointed that the author does not go much beyond conventional wisdom or, despite the masses of data, engage in close empirical analysis of a number of important questions underlying policy issues, instead of simply relying on general comments and assertions. For example, there is no estimate of what the inflow of imports from Japan would be in the absence of (a) export quotas, and (b) tariffs and other forms of restriction; i.e., the nature of the supply schedules in U.S. import-competing industries. Nor does he examine the degree to which resources in these industries are mobile. Is Japan's policy of restricting foreign direct investment rational, in that it can achieve the same objectives more cheaply by purchases of technology and portfolio capital inflow? Precisely (or even roughly) how important has foreign technology been in the growth of Japanese exports and GNP? How has Japan achieved such a successful export performance? Hunsberger explains only by general reference to comparative advantage, low wages combined with a high level of technical skill, Japanese behavior patterns, and the like.

I found the resolution of certain policy issues unclear. Hunsberger urges Japan to engage in more foreign aid, yet notes that the inability to pay for the level of imports needed will continue as a major constraint on Japanese growth. He does not resolve this apparent contradiction. Or another issue: should I, sharing the author's liberal trade persuasion, favor export quotas or not? We cannot blame Hunsberger for giving no full answer, since obviously much in a practical policy sense depends upon the strength of the protectionist forces on any particular issue at any particular point in time. Hunsberger does make sensible suggestions on procedures for arriving at export quotas and for built-in expansion of quotas, if export quotas are the only viable alternative.

In summary, this book is useful to economists mainly for its descriptive detail, as a data and reference source, and for its broad delineation of policy fa-

cets of Japan's trade relations, particularly with the United States. Analytically it is not strong.

HUGH T. PATRICK

Yale University

Integración de América Latina—experiencias y perspectivas. Edited by MIGUEL S. WIONCZEK. Mexico: Fondo de Cultura Economica, 1964. Pp. xxi, 381.

The purpose of this book of essays is to provide the nonprofessional Latin American reader with a description and evaluation of the "perspectives and possibilities" of Latin American integration and to give wider currency to the influential body of ideas on regional integration formulated by the U.N. Economic Commission for Latin America (ECLA) during the 'fifties under the leadership of Raúl Prebisch. The editor has brought together eighteen essays of varying quality written by well-known Latin American and non-Latin American economists who share an outlook uncritically sympathetic to what can be called the "ECLA doctrine" on regional integration. The book joins the growing *corpus* of literature available in Spanish on the theoretical and practical aspects of economic integration in less developed areas. (e.g., *Problemas de Pagos en América Latina,* CEMLA, 1964; *Cooperación Financiera en América Latina,* CEMLA, 1964; *Coordinación Monetaria Regional,* CEMLA, 1963; S. Dell, *Problemas de un Mercado Común en América Latina,* CEMLA, 1959; B. Belassa, *Teoría de Integración Económica,* UTEH, 1964).

The book is divided into three sections. The three essays in the first section (B. Belassa, S.B. Linder, and H. Kitamura) deal with theoretical approaches to regional integration. In the second section on the Latin American Free Trade Association (LAFTA), four essays review the history (M. Wionczek), the actual operation (S. Dell and G. Magariños), and problems (R. Prebisch) of LAFTA; three essays cover specific sectors: industry (P.G. Reynoso and J. Tinbergen) and transportation (Enrique Angulo H.); five essays concentrate on questions of financing LAFTA integration (F. Herrera, R.F. Mikesell, and E. Galveas) and proposals for a regional payments system (B.N. Siegel and R. Triffin). The final section includes three essays analyzing economic integration (J. Moscarella), industrialization issues (J.C. Mills), and payments arrangements (J.G. del Valle) in Central America. Appendices provide complete texts of the treaties establishing LAFTA and the Central American Common Market. A brief bibliography on regional integration completes the volume.

The principal value of *Integración de América Latina* lies in the ideas and notions of ECLA which provide the basis for many of the arguments of the essays and which have been so influential in technical and official circles in Latin America and elsewhere. The ECLA doctrine can be summarized as follows: An external sector "bottleneck" is retarding Latin American economic development. Primary commodity exports (due to a secular decline in the terms of trade and protectionist policies of the industrial countries) are lag-

ging behind the rate of growth of imports creating a structural payments disequilibrium. Latin American capital formation depends, to a considerable extent, on the capacity to import investment goods, technology, and raw materials, which is not growing fast enough to achieve a satisfactory growth rate. Therefore, economic development is impossible without import-substitution industrial expansion, which reduces nonessential imports and conserves scarce export income to pay for essential imports. However, the easy stage of import-substitution policy has come to an end in much of Latin America; it is now more difficult and costly for a single country with a limited domestic market to substitute local production for *essential* imports. ECLA's remedy is the creation of a Latin American political-economic bloc which will permit further import-substitution vis-a-vis the rest of the world and offer an alternative to inefficient industrialization by promoting liberalized intra-regional trade and specialization. Furthermore, the formation of LAFTA increases the bargaining power which Latin American countries can bring to bear on the developed countries to obtain nonreciprocal tariff reductions for primary commodities and preferential treatment for regional industrial exports. Thus, Latin American integration is an important part of ECLA's development strategy aimed at overcoming the "external bottleneck" via industrialization and improving Latin America's terms of trade.

The most serious shortcoming of the book is that not a single essay questions the soundness of the assumptions on which the ECLA doctrine rests. The Prebisch-Singer thesis which predicts the secular decline of the terms of trade of primary producers based on alleged past trends is accepted as an article of faith in spite of the scepticism aroused by the works of Morgan, Kindleberger, Lipsey, and Haberler. The overemphasis on import-substitution and the neglect of the relevance of comparative-cost considerations in the formulation of a Latin American development strategy continues to be a central weakness of ECLA doctrine. Demand deficiency (lagging external demand) is stressed to the virtual exclusion of supply deficiency (lagging domestic supply) resulting from government policies (taxation and overvalued exchange rates) which discriminate against primary producers. Together with nationalist *ressentiment* against the traditional, upper-class agricultural-export sector, the ECLA approach has helped to create the exaggerated prejudice against primary exports in official circles which hampers economic development in many Latin American countries.

JAMES D. THEBERGE

Harvard University

Business Organization; Managerial Economics; Marketing; Accounting

Soviet Management—With Significant American Comparisons. By BARRY M. RICHMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. vii, 279. $5.95.

Professor Richman in this book is concerned with describing the organiza-

tion and planning of Soviet industry viewed from the perspective of the individual Soviet enterprise. His primary concern is with the attitudes and behavior of enterprise managements, but these are discussed within the context of macroeconomic problems.

The novelty of the book is twofold. One is that the Russian sources used are entirely of postwar origin. The second is that, in the course of trips to the Soviet Union during 1960 and 1962, Richman visited more factories (16) than has any other Western writer on the same subject.

This is the fifth English-language book which has treated this subject. The first three dealt entirely with the period prior to World War II (G. Bienstock, S. M. Schwarz and A. Yugow, *Management in Russian Industry and Agriculture*, 1944; D. Granick, *Management of the Industrial Firm in the USSR*, 1954; J. Berliner, *Factory and Manager in the USSR*, 1957); the fourth (D. Granick, *The Red Executive*, 1960) also leaned heavily on prewar materials although it was significantly supplemented by postwar Soviet writings and interviews. Given this rather extensive literature, one might have expected Richman's book to be heavily oriented toward showing continuities and discontinuities between prewar and postwar Soviet management. In fact, however, Richman virtually ignores this theme. Curiously, his footnote references mention only two of the four books—and one of these is mentioned only with regard to subject matter which is virtually untouched by Richman.

Richman's book might be viewed as having added value because of its inclusion of comparisons with American managerial behavior. Some of these are rather interesting, although quite limited in scope. Again curiously, no indication is given that similar comparisons are provided in the literature cited and in an article by Berliner.

The scholarly contribution of this book would seem to lie in its indications of change between the prewar and postwar eras of Soviet management. Richman's key statement here seems to be that Soviet enterprise managers have acquired greater authority and independence since the middle-1950's. This may indeed be the case, but no documented evidence whatsoever is presented in support. At least one of the supporting but undocumented claims (that, particularly during the Stalin era, personnel problems were not of significant concern to enterprise managers) is clearly incorrect. Presumably, the source of Richman's view as to the growth of power by enterprise managements consists of his interviews. But the results of such interviews, when they relate to such a broad position, can scarcely be accepted as authoritative without close examination. This Richman makes no attempt to do. Although it is mentioned in another connection, a major article by A. Nove (in *Soviet Studies*, July 1962) is completely ignored in this context despite the fact that it presents evidence which, on the face of it, casts doubt on the Richman thesis.

Judging from the materials presented, this reviewer would conclude that the position of Soviet enterprise-managers did not change particularly between the prewar period and the experiments of 1964. If this conclusion is correct, then, aside from institutional shifts such as the change to the regional system of administering firms which occurred in 1957, one might properly conclude that the earlier Berliner and Granick monographs are still up-to-date descriptions

of Soviet reality. However, this reviewer feels no great confidence in such a conclusion. For, out of three major areas of potential change, one is virtually ignored (control over the enterprises by the State Bank) and the other two are examined so sketchily as to inspire no confidence whatsoever in the results obtained. These two areas are those of Party relations with enterprise management and of management backgrounds and careers.

According to the flyleaf of the book, Richman is writing for both specialists and nonspecialists. For the former group, some tables are worthy of attention —although these are based on a very tiny number of enterprises in which Richman carried on interviews. These are presented on pages 67-68, 135, 161, and 212. Other interesting data are presented on pages 167-68 and page 176. However, even the table on page 135—which deals with bonus payments to management, and which this reviewer finds the most interesting of all—suffers seriously from a failure to define "management"; one can think of three alternative definitions and there is no inkling of which is intended. Some other specific statements of fact (on pages 66 and 92 especially) may well be correct and are quite interesting if they are reliable; but since they are left totally unfootnoted, the specialist can hardly pay them much attention.

As for the nonspecialist, other works cover much the same ground and are written in a more interesting style. Richman's book also suffers from the absence of an index.

DAVID GRANICK

University of Wisconsin

Industrial Organization; Government and Business; Industry Studies

Law and Economic Policy in America: The Evolution of the Sherman Antitrust Act. By WILLIAM LETWIN. New York: Random House, Inc., 1965. Pp. vi, 282. Appendix. $5.95.

This is one of those rare studies that slip back and forth between diverse but related fields of scholarship and between pure methodological issues and those of empirical substance. The effort and the results justify William Letwin's often tedious excursion into legal and constitutional history, the history of economic doctrine, and the pragmatic background of legislating economic policy. His search has been into the primary sources, especially the legislative debates and the over-all effect is quite impressive. I have no hesitation in hailing Letwin's study as a model of how to study the historical development of national economic policy.

Letwin has three points to make. First, the Sherman Antitrust Law of 1890 was much more novel in its legal frame than is commonly supposed. The cliché (of the times and of subsequent text writings about the law) is that the Act codified the "common law." Letwin clearly expands this hypothesis to one more reasonable of acceptance by delving into the common law relating to monopoly and restraints of trade and tracing its dynamic and fluctuating de-

velopment prior to the passing of the Sherman Act. Letwin also tackles another cliché common in conventional historical wisdom: passage of the Sherman Act was a crumb thrown by Congress to a hungry public. Congress had no intention of restricting the merger movement or prescribing competitive rules of the game, historians of several generations have argued. Instead it passed a law so simple in wording as to be ineffective against the ills claimed, but so sweeping in its condemnation as to assauge an hysterical public opinion fearful of the octopus monopoly. This just wasn't so, according to Letwin. Congress grappled with the issues inherent in designing legislation to cover so broad a range of business issues and opted not for a specified bill of particulars, but for a broad statement of economic philosophy, and one which could be given substance by judicial interpretation and where the myriad and complex issues could be adjudicated on their relative merits.

Letwin's third objective is to convince the reader that legislation which regulates business conduct and prescribes limits on industry organization and structure is at best Hobson's choice. Legislation such as the Federal Trade Commission and the Clayton Acts, offered as weapons for closing the holes opened by the Sherman Act, is no less successful than broadly conceived rules of conduct and may even be worse. In short, if public opinion demands legislation which affects industry structure, conduct, and performance, such rules had best be purposefully broad and capable of relative and individual flexibility in interpreting them. What emerges from this tripartite approach is a rewarding study of the elusive nature of achieving a balance between what is equitable, what is economically rational, and what is politically feasible.

I accept without reservation Letwin's conclusions. Economic historians have just not been correct in interpreting the motives of Congress in passing the Sherman Act. Letwin's examination of the evidence is far more complete and seemingly objective and just. Similarly, economic historians have accepted, without investigation into primary sources, the supposedly intimate link between the common law and the Sherman Act. Letwin claims for the framers of the Sherman Act much more innovation than hitherto believed. His third conclusion could be stated in even stronger terms. It is not possible, I think, to accept the view that the mix of antitrust legislation in this country (starting from the fount of the Sherman Act up to the Cellar Amendments) has in a measurable way influenced the performance of the entire economy, substantially altered industry structure, improved price flexibility, or aided our long-term economic growth. One can point to a few cases where results are visible though not quantifiable, but the over-all accomplishments are nominally small. Letwin's point is that this is not a surprising result. The economic philosophy embodied in the Sherman Act is so fundamental to American national character and values, however, that even if on a cost-benefit analysis the super structure of antitrust laws cannot be vindicated, there is something comfortable to the national psyche about believing that the "robber baron" of the past could not exist today. Comforting, but wrong.

RALPH L. ANDREANO

University of Wisconsin

Government-Business Relations—A Pragmatic Approach to the American Experience. By ARTHUR M. JOHNSON. Columbus, Ohio: Charles E. Merrill Books, Inc., 1965. Pp. xvi, 446. $8.75.

The novelty claimed by Arthur Johnson and the organizing theme of his textbook is a pragmatic approach to public policy. The book is arranged in five parts. Johnson discusses public policy in terms of legislative, executive, and judicial policy (Part I, 91 pages), government supports (Part II, 117 pages), government restraints (Part III, 158 pages), government ownership of productive facilities (Part IV, 34 pages), and concludes with a summary of government-business relations since the turn of the century (Part V, 29 pages). In my judgment this book suffers more from a lack of analytical depth than from scope of coverage.

Johnson, recognizing the importance of "processes by which private and public interest are adjusted to one another" (p.vii), finds descriptions of statutes and court decisions that govern business, as well as economic analysis, of limited analytical utility. He argues that the latter is "limited to our acceptance of the premises on which economic theory is built . . . the decision of legislators, judges, and administrators, not to mention the public, seem to have ignored, contradicted, or confounded economic theory" (p.vii).

A careful reading of the book, particularly pages 292-302, reveals that "the premises on which economic theory is built," are not the usual postulates used in economic theory, e.g., individuals prefer more to less substitution, etc. Rather, economic theory is erroneously equated with perfect competition. As no single evidence is cited in the book that is held to contradict economic theory, the contradiction presumably comes about when Johnson does not find all the industries conforming to perfect competition.

The author's confusion about the nature of resource allocation, the role of private enterprise, and the market system has led him to summarize the discussion of several subsidy cases in the following manner:

> The point to be made here, however, is that, responding to pressure and interest groups, government is allied with a very broad range of business endeavors. In view of the overemphasis frequently given to governmental restraints on business, it is well to keep in mind the many ways in which public funds and public servants help to protect and sustain private enterprise (p. 160).

The fact of the matter is that a subsidy as well as a restraint may misallocate resources. An economist (if not the beneficiary of the subsidy) will not cheer an attempt to balance the two kinds of government actions.

I consider lack of a frame of reference for evaluating the outcome of numerous types of governmental subsidies and restraints a significant shortcoming in a text for an economic course in government-business relations. Moreover, while the organizing theme of the book is to describe processes by which public and private interest are brought together, the author says that "public policy" defies precise definition, and he does not even attempt to define "public interest."

I am sympathetic with his effort to incorporate interrelationships and to

discuss phenomena in a particular institutional arrangement, but his focus on what "works" leaves the reader with only cursory knowledge of the general topics that are usually treated in such a text. Thus, the interrelations are discussed in their legalistic setting, with little insight to the motives behind actions or reactions of different participants in the processes. For example, on pages 5-6 one reads that pragmatic public policy:

> . . . involves interrelated elements, political, social, and ideological as well as economic, specific to time and place. Problems are dealt with as they arise and not in accordance with predetermined plans. Experimentation and *ad hoc* solutions are important parts of the resulting process. Moreover, the process bears not only the imprint of public officials and their personalities but also that of private citizens and groups whose actions or reactions give public policy with respect to business, whatever its form, its ultimate significance.

The book might have become more than a narrative of various events if there had been an analysis of the behavior of the participants in decision making in the nonmarket world of public commissions who are subject to the reactions of voters. Perhaps this omission was due to Johnson's undue pessimism of the utility of economic analysis. Recent advances on this front are encouraging and what appear as "*ad hoc* solutions" may become predictable if subjected to economic analysis.

In closing, I would like to raise some criticisms that apply to other available texts as well. It is all too often that "problems" are stated without critical examination; not only is government regulation or restraint assumed to be necessary, but the outcome of the governmental action is not evaluated in terms of the original goals for which the solution was sought.

Consider the case of radio frequency allocation. Only recently analysis of the episode of the 20's, the formative years leading to creation of the Federal Communications Commission (FCC), has revealed that the problem was not defined correctly. Although interference caused by simultaneous emission of radio signals was the major consideration in that period, the FCC has been empowered to decide the quantity of broadcast service that is forthcoming. Moreover, by making subscription system of sponsorship illegal, the FCC in effect has affected the quality of the entertainment and educational services made available today. Yet both officials and students of broadcasting seem to be surprised and dissatisfied with the outcome. Although the outcome is the result of the institutional arrangement chosen, the blame is put either on the broadcasters and/or the officials of the FCC for not pursuing a more "rigorous" policy.

One can only hope that problems that have concerned us will be subjected to analyses, and assessment will be made of the outcomes of different institutional arrangements.

JORA R. MINASIAN

State University of New York at Buffalo

Report of the Committee on the Economic Impact of Defense and Disarmament. Washington: Superintendent of Documents, 1965. Pp. ix, 92. 40¢.

This is the third pamphlet on disarmament published by the U.S. government.[1] The general conclusion of the two earlier studies, which were published by the U.S. Arms Control and Disarmament Agency (ACDA), was that a major depression would not result from disarmament since the economic problems are solvable with appropriate fiscal and monetary policies. With advance planning by all levels of government, by business firms, unions and other private organizations, the funds and resources released from defense spending would allow additional public and private expenditures, creating an unqualified social blessing. The fact that little advance planning had occurred was not reflected in the optimistic views expressed by ACDA.[2] This latest study, although recognizing that the general solutions are known, admits that the problems of maintaining general full employment and of minimizing the regional and industrial dislocations will be difficult to solve.

The scope of this report has been substantially broadened. First, the authorship includes not only ACDA, but also representatives from the Departments of Defense, Commerce and Labor, the Atomic Energy Commission, the National Aeronautics and Space Administration, the Bureau of the Budget, the Office of Emergency Planning—all under the chairmanship of Gardner Ackley, chairman of the Council of Economic Advisers.[3] Second, the general topic was expanded to include a study of the economic effect of changing defense requirements. Since the regional economic impacts are similar and since the possibility of disarmament is presently quite dim, the additional breadth will make the study more useful and the expenditure of additional research funds politically more acceptable.

The first two chapters consider the size and distribution of expenditures and employment in defense and defense-related activities, outlining the problems caused by changing expenditure patterns and by the closing of obsolete facilties. As the report points out, "Readjustments are most successful when the economy is expanding vigorously and when employers and workers have strong incentives to make the shifts that are required" (p.18).

Chapter 3 makes a forecast of GNP for 1970 ($870 billion). With the increases in tax revenues generated by our existing tax schedules, federal spending increases and/or tax reductions amounting to $40 to $45 billion over the next five years must occur if the economy is to remain at full employment.[4]

[1] U.S. Arms Control and Disarmament Agency, *Economic Impact of Disarmament*, 1962; and *The Economic and Social Consequences of Disarmament*, 1962, revised 1964.

[2] In general, political attitudes in the United States will not allow for the necessary government planning or authority to shift unemployed resources except in periods of crises. Thus the U.S. plan for partial disarmament, which was spread over a 12-year period to minimize adjustment problems, greatly reduced the possibility that the federal government would receive the necessary power to successfully combat the economic repercussions.

[3] Added to the above-listed agencies, which were listed in the memorandum by the President establishing this committee, are the General Service Administration, the Small Business Administration, and the Office of Science and Technology.

[4] Since most federal nondefense spending is for transfers rather than expenditures for goods

Assuming that defense outlays remain constant, nondefense spending (excluding interest) must increase by 70 to 80 per cent over the five-year period; or across-the-board tax cuts of 15 to 18 per cent must be made. (The 1964 tax cut was approximately a 10 per cent reduction in total federal revenues.) If a 25 per cent reduction in defense spending occurs by 1970, then the rise in nondefense outlays over five years must be 95 to 100 per cent or the reduction of all taxes must be 23 to 26 per cent. "If the very likely assumption is made that social security payroll taxes, customs duties, and miscellaneous (mainly non-tax) Federal receipts would not be subject to fiscal policy adjustments, the needed tax reduction would be equivalent to a cut of about one-third in all other Federal taxes" (p. 28).

The committee readily points out that macro-governmental programs such as tax reductions cannot prevent regional and industrial dislocations. "Without appropriate fiscal policies, however, not only will specific measures to solve distress be frustrated, but widespread national unemployment could result" (p. 29). The remaining three chapters (the unique part of this study compared to the earlier reports) are concerned with (a) the analysis of present governmental machinery being used to minimize the effects of economic dislocations, (b) recommendations for improving existing micro-governmental programs, and (c) the informational and research needs for strengthening micro-governmental programs. The criteria established for effective micro-programs are:

(1) consistency with macro-policies promoting full employment and satisfactory growth;
(2) consistency with standards of equity between sectors of the economy and regions of the country, e.g., special disarmament benefits should not be restricted to government employees but should be open to all employees;
(3) consistency with the efficient allocation of the nation's resources;
(4) consistency with local and private initiative;
(5) consistency with national security needs.

The committee was impressed by the variety of governmental services that now exist. A basic problem is to coordinate these available services into an over-all program. Recommendations to assist in this necessary coordination are (a) the establishment of a Federal Task Force on Community Assistance to provide on-the-spot coordination in depressed areas, and (b) the establishment of a permanent committee, with a full-time staff, to expedite and give continuity to research outlined in this report.

The committee points out that the data are generally adequate in analyzing the impact of changing defense needs on the aggregate economy but are inadequate for studying regional and industrial dislocations. While reasonable estimates can be made on dollar magnitudes of prime contracts, the employment change of the prime contractor, and the amounts and employment impact of subcontracting are generally unknown. The time lags involved in the award-

---

and services, the same multiplier with different signs is used for expenditure increases and tax reductions.

ing of contracts and the resulting economic impact are also generally unknown. Research in these areas by the Department of Defense and ACDA is discussed as are the attempts to apply the latest input-output tables in determining industry impacts. Detailed studies are being made of South Bend, after the closing of the Studebaker plant; of Richland, Washington, after the closing of the A.E.C.'s atomic energy plant; of Seattle-Tacoma, after the reduction of Boeing's labor force; and after the closing of various military installations. The results of this research will greatly assist in understanding the problems of changing defense expenditures.

Although still overly optimistic in assuming that the economy can be made to perform satisfactorily if there is a significant amount of disarmament, this report has outlined many of the crucial problems which must be faced in such an adjustment. Even though the study is not as technical or as tightly organized as the professional economist might prefer, it does provide an up-to-date summary of the problems and of the efforts to solve these problems which are being made by the federal government.

ROBERT E. BERNEY

Arizona State University

Railroads and Regulation 1877-1916. By GABRIEL KOLKO. Princeton: Princeton University Press, 1965. Pp. vii, 273. $5.00.

I consider myself lamentably well qualified to review this book. When it appeared I had already worked seven months during a sabbatical on a similar—nay, identical—project. After spending another month evaluating the book, I decided that there was little point to continuing with my own study. From this the reader may deduce that I have a high opinion of the volume, a presumption I shall proceed to verify.

Professor Kolko, an historian at the University of Pennsylvania, has set out to reinterpret the origins and early history of the Interstate Commerce Commission. He takes as orthodoxy the position that the ICC was established to engage in trade regulation of a monopolistic industry as a consequence of public revulsion to the industry's excesses. More specifically, he endeavors to refute the *locus classicus* of such views, I. L. Sharfman's compendious, but adulatory and uncritical history of the Commission.

Kolko demonstrates that the purpose of the Act of 1887 was simply to lend stability to railroad cartels which the expansion of the industry had rendered chronically unstable. Rail rates had fallen steadily throughout the post-Civil-War period so that it was their instability rather than their absolute level which was a source of widespread dissatisfaction. Merchants, farmers, and railroad managers alike found the characteristic pattern of alternation between collusive pricing and rate wars intolerable. Kolko in his discussion of the origins of the Act of 1887 is principally concerned with demonstrating political support by railroad management for establishment of a federal commission. Railroad magnates typically preferred Cullom's formulation of the Act of 1887 in the Senate, which included establishment of the ICC, to Reagan's

bill in the House, which provided for enforcement of published rates by recourse to the courts.

Kolko's history of the early behavior of the ICC is a devastating demonstration that the Commission looked upon itself as a cartelizing body from the outset. Cooley, the first chairman, had a solid background in arbitration of disputes within the railroad cartels. The Commission's early jurisprudence centered about acceptance of discriminatory pricing, putting down of rate wars, and encouragement of collusive rate making. The Commission's statutory authority for all this was grossly inadequate. It had no statutory power to set rates, and the Act of 1887 neglected to legalize the collusive activity it was designed to facilitate. Kolko interprets the Commission's early reverses in the courts not as interferences by a *laissez faire* oriented judiciary with a progressive regulatory body, but as inevitable consequences of the vagueness of the Act of 1887. The Acts of 1903, 1906, and 1910 he interprets as efforts to strengthen the Commission's original puny powers for cartelization. Kolko appears somewhat more at home in the period 1900-1910 than in the earlier years. His treatment of the Elkins Act of 1903, the most potent of the pre-1920 statutes in requiring adherence to collusively set rates, is particularly impressive. He demonstrates that the Act had its origins in the legal department of the Pennsylvania Railroad and was the product of railroad pressures throughout. By the eve of the first World War, The Commission was succeeding in the course on which it had been set in 1887: cartelizing the railroad industry without pooling.

Kolko's interpretation seems to me perfectly correct. It surprises me only that the academic community took until 1965 to produce a book which sees the ICC for what it was—and is. Being so familiar with the author's sources, I found the historical record so unambiguous that I have difficulty conceiving how a long line of scholars can have avoided the conclusions of this volume.

In spite of the book's general excellence and correctness, it will leave most economists unsatisfied in several respects. The author is almost exclusively devoted to demonstrating that railroad pressure was mainly responsible for the development of public policy. There is a strong implication that the policy was undesirable. I am convinced of it, but there is nothing in the book to provide one with a framework for reaching this conclusion. Kolko is writing about collusions, but he makes no use of the theory of cartels which he could easily have found in an intermediate or even elementary text in economics. I suspect that some explicit theoretical basis would have helped him pick up some major points that he missed. Notably, Section 5 of the Act of 1887, a prohibition on pooling inserted in a Senate-House reconciliation of the Cullom and Reagan bills, was inconsistent with the rest of the Act, which was almost entirely devoted to facilitating collusion. Similarly, Kolko fails to recognize that the practice of charging more for a short haul than for a long one was both a form of discrimination against areas without water transport and a means of rate-cutting to points at which the railroads were competitive with one another. Accordingly, his treatment of the differing texts of Section 4 of the Act of 1887 in the Reagan and Cullom bills and of the Commission's troubles with Section 4 in the Courts in the 1890's seems to me less perceptive

than it should be. Finally, Kolko's discussion of *Smyth v. Ames* (1898) is entirely inadequate. The ICC was not established to engage in public utility regulation in the usual sense of valuation and simulation of a competitive return for a monopolistic firm. Insofar as it behaved that way in its later history, it was largely because of the common law obligations placed on it by *Smyth v. Ames*. Kolko mentions the case only once, almost as an aside.

Withal, this volume represents a distinct advance in our knowledge of economic history. Let us hope that the book also prompts the intellectual community to see the Interstate Commerce Commission for the cartel that it is, and thus contributes toward its eventual elimination.

GEORGE W. HILTON

University of California, Los Angeles

The Economics of Nuclear Power. By J. A. HASSON. London: Longmans, Green and Company, Ltd., 1965. Pp. vii, 160. 35*s*.

This book is more or less unique in the literature dealing with the economics of nuclear power. Most earlier works can be classified into one or more of the following categories: (a) quantitative projections of a specific country's (or a group of countries') energy consumption total and of the likely role of nuclear power within that total;[1] (b) economic analysis of the long-range effects of nuclear power on specific industries and/or regions via input cost reductions and/or changes in technology due to postulated energy costs (presumably) below costs from alternative sources of power or heat;[2] (c) discussion and analysis of the political economy of nuclear power, covering the economic effect of government policies toward the development of nuclear energy and of government regulation and/or operation of substantial components of the nuclear energy industry;[3] (d) relatively more technical studies reviewing the current status and/or future prospects of nuclear costs and technology, including, as a major subcategory, nuclear fuels;[4] (e) studies of the conceptual

[1] National Planning Association, Reports on *Productive Uses of Nuclear Energy* (studies of Japan, Brazil, Italy, India, Israel, Pakistan, United States, and Puerto Rico) (Washington 1958ff.); Organization for European Economic Co-operation, *Towards a New Energy Pattern in Europe*, Report prepared by the Energy Advisory Commission under the chairmanship of Professor Austin Robinson (Paris 1960); International Atomic Energy Agency, Technical Reports Series, *Prospects of Nuclear Power in the Philippines*, and other countries. Reports of Missions sent to the various countries by the Agency (Vienna 1961ff.).

[2] Sam H. Schurr and Jacob Marschak, *Economic Aspects of Atomic Power: An Exploratory Study* (Princeton 1950); Walter Isard and Vincent Whitney, *Atomic Power* (New York 1952).

[3] Philip Mullenbach, *Civilian Nuclear Power: Economic Issues and Policy Formation* (New York 1963).

[4] United Nations, *Proceedings* of the International Conferences on the Peaceful Uses of Atomic Energy, various papers (Geneva 1955, 1958, and 1964); U.S. Atomic Energy Commission, *Costs of Nuclear Power*, TID-8531 (Rev.), 1961; various articles in *Nucleonics* magazine, monthly.

problems involved in finding the cost of nuclear power;[5] and (f) in addition, there are studies covering nonpower applications, such as desalting sea water, food preservation and other radiation applications, nuclear ship propulsion, isotopic power sources, etc.[6]

J. A. Hasson's book falls largely outside these categories. His primary concern is the application of some of the newest theoretical tools of economic analysis to the question of the economic costs associated with the development of nuclear power. His book is therefore more a discussion of these several tools against the background of nuclear energy than an analysis per se of its promises and problems. This distinction is essential if the reader wishes to avoid some serious misunderstanding.

In Chapter 1, Hasson devises a linear programming model of a nuclear energy "industry" (defined to include the entire vertical range of economic activities from mining uranium, through ore concentration, fuel fabrication, fabrication of special nuclear equipment, operation of the nuclear power plant and chemical reprocessing),[7] and he elucidates the technological constraints on the system and the objective function to be optimized. He next presents a cost function of a nuclear power plant, and, in defining parameters and explaining formulas, takes advantage of the oppportunity to review some of the technological relationships underlying nuclear energy. In Chapter 2 he considers the development of a nuclear power industry from the standpoint of welfare economics, and he devotes ten pages to an excursion into the topic of consumers' and producers' surplus as applied to nuclear power.

The next major topic of discussion (in Chapter 3) is the application of benefit-cost analyses to public investments in nuclear power development, first in the United States, then in the United Kingdom, and finally in India.

In his fourth and final chapter, entitled "Factors in the Choice among Alternatives," Hasson seeks to provide some perspective regarding energy problems in general, as well as in the several countries mentioned, and he reviews factors to be taken into account by an electric utility executive, or nationalized-power authority, in meeting future load growth in choosing between alternative types of generating capacity. The treatment is more descriptive than

[5] International Atomic Energy Agency, Technical Reports Series No. 5, *Introduction to the Methods of Estimating Nuclear Power Generating Costs* (Vienna 1961); U.S. Atomic Energy Commission, *Guide to Nuclear Power Cost Evaluation,* TID-7025, Vols. 1-5, study by Kaiser Engineers (Washington 1962).

[6] Office of Science and Technology, *An Assessment of Large Nuclear Powered Sea Water Distillation Plants* (Washington, March 1964); United Nations, Report of the Secretary-General on *Economic Applications of Atomic Energy: Power Generation and Industrial and Agricultural Uses* (New York 1957); Joint Committee on Atomic Energy, 84th Congress, 2nd sess., *Peaceful Uses of Atomic Energy,* Report of the Panel on the Impact of the Peaceful Uses of Atomic Energy (McKinney Report) (Washington 1956); U.S. Atomic Energy Commission, *Systems for Nuclear Auxiliary Power: An Evaluation* (Washington, January 1964).

[7] This list of nuclear activities omits uranium enrichment in diffusion plants, which constitutes a substantial cost component in any reactor system employing fuel enriched in uranium 235, as well as waste disposal, including storage, which under any reasonable projection of future nuclear capacity could loom large.

ological, in contrast to the preceding three chapters, and may contain ıtion useful to the nonspecialist.

pt for some comments regarding the cost-benefit analyses, I have not :ed to review the application of any of the analytical tools enumerated Hasson demonstrates considerable mastery of these techniques and s, as well as of the esoterica of consumers' and producers' surplus, and rves to be praised for his imagination and ingenuity in seeking to apply ools to the problems raised by nuclear energy. As suggested, this repre- significant and eminently worthwhile broadening of the subject matter ı covered by nuclear economics. In this connection, however, and as a ly minor footnote, it must be observed that Hasson's style tends to es- use of mathematics—quite often perfectly simple and understandable nts are promptly restated as mathematical formulas (on several occa- correctly) and almost as promptly forgotten without any apparent rea-

occasion arises in formula (4.1), page 103—here the formula is in my superfluous and, what is worse, misleading. The a_i terms should y bear a second subscript to denote the particular region they refer e a_i in region r differs from a_i in region s. Otherwise one might con- ıat the only difference between $\Pi_1 = f_1 (a_1, a_2 \ldots, a_n)$ and $\Pi_{11} =$ $a_2 \ldots, a_n)$ is the functions f_1 and f_{11} applied to the same set of a_i ı case. A second example occurs in formula (4.3) on the following ı this case there is a parenthesis missing around the term $(\Pi - \gamma)$ and ıominator λ is incorrectly defined; λ should be expressed in units of of hours of operation per year corresponding to the desired plant and not as a per cent, as stated. This is necessary in order to convert :tion $(\Pi - \gamma)/\lambda$, the denominator of which is given in units of Kwh, ıits of KW, so as to be consistent with the other two terms in the n. A third instance occurs on page 107. In formula (4.5) the sym- s used twice, each time with a different meaning; the first meaning been given on page 16, and the second on page 107.

e are matters more of style than substance and need not detain us The more serious shortcomings of this book are primarily concerned, ested earlier, with the substantive aspects of energy in general and of energy in particular.

most prominent deficiency centers around that fact that Hasson's infor- is hopelessly outdated. Most of his references to nuclear costs and to technology are to the late 1950's; rare indeed are references later '60 or 1961. Yet, in the period since then, the competitive prospects for power have been markedly altered. As late as November 1962, the which has never been accused of pessimism in regard to nuclear pros- vas projecting 4 mill/Kwh costs in large nuclear power plants built only 5-80. Barely a year later, however, the famous Jersey Central an- ment heralded the likely attainment of this low generating cost figure a decade sooner.[8] Much of this speed-up has resulted from a drastic

eful summary of the present status of nuclear costs and technology can be found

drop in the capital cost/KW of nuclear plants, down close to the capital cost of conventional plants, a development of enormous significance, both domestically and abroad, insofar as the future competition between nuclear and conventional plants is concerned. While it is recognized that it is extremely difficult for one person, in undertaking a work covering such a wide range of subject matter, to be completely up to date, I cannot believe that a book published in 1965 and hence, presumably, in manuscript form in, say, mid-1964 could not have been more recent. And in a field like nuclear energy, the technology of which is changing so rapidly, the nonspecialist who relies on Hasson's costs and technical parameters will be as out of touch with the current status as was Rip Van Winkle.[9]

Not only have the current status and near-term cost prospects of nuclear energy been dramatically altered, as indicated, but significant institutional factors have also been changed. It is surprising to read on page 24, ". . . in practice a 4 per cent carrying charge is levied [on the nuclear fuel inventory] by the A.E.C." when, in fact, this rate was raised to 4¾ per cent in July, 1961, an event which was widely discussed at the time in the trade journals and elsewhere. There is not even a footnote mentioning the change in a book published in 1965.[10]

It should be added for the benefit of the nonspecialist that the institutional framework within which nuclear fuel costs are determined has been further changed by the passage of legislation in August 1964 permitting (and after January 1, 1971, requiring) private ownership of nuclear fuels, thus ending the government ownership monopoly on all fissionable materials whereby nuclear fuel was leased to users at rates involving a subsidy.

Other examples, indicating what can only be termed unfamiliarity with nuclear "facts of life," both current and not-so-current, can also be cited. Lack of space prevents mentioning more than one of these. In Chapter 3, starting on page 70, Hasson presents a cost-benefit analysis of the U.S. Civilian Reactor Development Program. In deriving a base for comparison with generating costs in subsequent nuclear plants, in order to determine the benefit terms of his cost-benefit ratio, Hasson utilizes the well-publicized generating cost of the Shippingport plant (62 mills/Kwh), generally referred to as the first com-

in "Nuclear Power Economics—Analysis and Comments—1964," Joint Committee on Atomic Energy, 88th Congress, 2nd sess., October 1964.

[9] The author was undoubtedly aware of this, since he stated the following in the Preface (p. viii): "While empirical analyses exist, events have moved and I do not assess whether or not time, increased knowledge and experience have treated these kindly. It is nevertheless anticipated that this work fulfills a useful purpose."

The question here is whether the nonspecialist in nuclear matters is enlightened or misled by the use of obsolete costs and parameters. If the data are wong—or at least no longer valid as Hasson admits—and if the task of incorporating more up-to-date and relevant data is in fact insuperable, perhaps it would have been preferable to use entirely hypothetical data so to avoid this danger.

[10] In another context, Hasson does refer to the AEC schedule of charges for enriched uranium which was also changed on the same date (p. 74, footnote 2).

ized nuclear power plant in the United States. He thus secures a gen-
ɔst differential between the 62 mill figure at Shippingport, completed
and the 11.1 mill figure at the Yankee plant, completed three years
hough this is described as "recently" in the book). But this is a gross
retation of the Shippingport figure, which includes the considerable
nnumerable test facilities, extra loops and instrumentation, etc., built
search purposes. It is more correct to characterize the Shippingport
a laboratory operated primarily for information, and producing elec-
r as a by-product, than to consider it a nuclear power plant in any
sense.[11] To attribute all the Shippingport costs to power generation in
nalysis betrays a consideable naïveté regarding nuclear matters.
e my earlier blanket endorsement of the analytical tools used by Has-
ıst at this point make one comment regarding the application of cost-
nalysis to nuclear power development. On page 76 Hasson states:

> ssessing nuclear power, benefit-cost ratios can be calculated at
> it levels: (1) for individual plants; (2) for reactor concepts;
> ally, (3) for the entire nuclear power industry. Assessment at the
> two levels entails accounting for indirect effects which involve
> l economies, transferable know-how, and other kinds of inter-
> lencies.

st cost-benefit analysis is concerned with the individual plants and
ı "immediate" or direct benefits. And despite the statement quoted
goes on to say (p. 79):

> ranking of reactors with a benefit-cost ratio less than one would
> ltered if allowances for indirect gain—external economies; trans-
> know-how, etc.—were made. In any case, evaluation of indirect
> s from investment in individual reactor concepts is complicated;
> ill receive from, and impart benefits to other projects.[1]
>
> e will take the form of reduced capital costs, higher fuel exposure levels, changes
> ıake-up, improvements in fuel fabrication, chemical reprocessing, etc. Accounting
> dependence may occur only through side calculations not an integral part of
> benefit-cost ratio. Careful empirical study would be essential.

ot clear from these two quotations whether Hasson believes indirect
hould or should not be included, if data were available, in the partic-
-benefit analyses under discussion. It is my belief that they are indis-
since, as Hasson admits later on the same page, all of the plants in-
ı the analysis are of a predominantly prototype nature. Their main
être is for the gain in knowledge to be secured—knowledge not re-

omic Energy Desk Book, by J. F. Hogerton (New York 1963), the Shipping-
is described in the following terms: "Its main importance, however, is as a
l water development facility. . . . It was designed and is equipped to test
ore designs and explore operating variables as well as to obtain experience in
nuclear power generating. It is much more heavily instrumented and has
le (*sic*) more operating flexibility than any other U.S. power reactor of com-
e" (p. 500).

stricted in applicability to that particular type of reactor technology—whereas the actual generating costs of each and their comparison with Shippingport as a base, which forms the sole component of the benefits term, is of a distinctly secondary importance.

I would also question Hasson's assertion, in the first sentence of the second paragraph quoted, that the ranking of reactors would be unaltered if allowance were made for indirect gains, since these indirect benefits will differ among themselves. Hasson's bland assertion is unsupported by any visible evidence, and it is not clear to me whether it is intended merely as a mathematically correct assertion deriving from the theory of cost-benefit analysis or whether it rests on an empirical evaluation based on rough estimates over the reasonable ranges of the relevant indirect benefits.

My final comment concerns Chapter 4, which is a hodgepodge of empirical information concerning installed generating capacity in West Germany and coal transport costs in West Germany and India, of commonplace generalizations regarding fuel competition and factors determining the growth of electric power demand, and of mathematical formulas intended to accord these generalizations a symbolic dignity. All of these factors, identified as being relevant in extending electric power capacity, reduce essentially to short-run and narrowly defined cost comparisons, such as might be faced by a private electric utility in the United States. There is therefore no connection between this chapter and the three preceding, and none is attempted, even when the author discusses the case of nationalized electric authorities, such as those in India or the United Kingdom, which should properly and do concern themselves with the questions raised and the conclusions of the preceding discussions regarding nuclear power development.

It is difficult to summarize my impressions regarding this book. It is a welcome attempt to apply sophisticated analytical tools and concepts to an admittedly complex and rapidly evolving technology, and the author's failure to master the technological part of this job does not detract from the fact that he was the first with sufficient courage to undertake the task. Having guided a research and development effort which by now has essentially achieved competitive nuclear power in low conversion ratio reactors of large size, the AEC is now pressing forward at an annual rate of well over $200 million to develop advanced converters and to master breeder reactors. Mastery of the breeders is expected during the 1980's, with advanced converters available at an earlier date. The ostensible justification for this effort is that only breeding will permit us to make efficient use of our fissionable fuel resources, and so avert an alleged, but questionable, shortage of conventional fossil fuels one or two centuries hence. No one has tried to test the economics of this effort, and Hasson's effort will be useful when and if this is ever undertaken.

PERRY D. TEITELBAUM

Resources for the Future, Inc.

Land Economics; Agricultural Economics; Economic Geography; Housing

Economic Crises in World Agriculture. By THEODORE W. SCHULTZ. Ann Arbor: University of Michigan Press, 1965. Pp. viii, 114. $3.50.

Soviet Agriculture: The Permanent Crisis. Edited by ROY D. LAIRD and EDWARD L. COWLEY. New York: Frederick A. Praeger for the Institute for the Study of the USSR, in cooperation with the University of Kansas, 1965. Pp. xii, 209. $7.00.

The title of these two volumes suggests that a new and major crisis has developed recently in the world agricultural situation. In fact, of course, almost the opposite is true in that most of the world's agriculture is operating much as it has in the past, despite technical aid, central planning, and numerous other social, political, and economic schemes to change it.

Essentially, Theodore Schultz's book is an examination of why farmers in most countries have changed so little, given the observable fact that major increases in agricultural productivity have been achieved in a few nations. For those familiar with Schultz's recent *Transforming Traditional Agriculture* the present book contains little that is new, although one has a feeling that he has somewhat expanded his horizons to take into account some of the criticisms of his earlier volume. Since this present volume was a series of lectures it is somewhat shorter and more general and omits the case documentation of the other work.

The Schultz reasoning is as follows: Traditional agriculture is well organized around the resources available to it. Productivity is low because there is little economic incentive to add more of the conventional inputs that are available. Incentive is lacking because the available inputs offer low returns, the system is organized to provide insufficient incentives to farmers, or the farmers are incapable of utilizing the complex inputs necessary in modern agriculture. Schultz argues that in the poor countries agricultural productivity can only be increased by providing new inputs that will provide farmers with higher rates of returns for investment in agriculture and by advancing the level of education of farmers. He puts relatively less emphasis upon the social and economic organization of underdeveloped areas and the barriers these provide to the expansion of output. Even so, he gives such matters more concern in passing than previously. However, in discussing the problems of Russian agriculture he puts all of the burden of unsatisfactory performance upon the system of organization and does not discuss the nature of the inputs available to Russian agriculture and their potential productivity.

The volume *Soviet Agriculture: The Permanent Crisis* is a more detailed examination of the same question posed by Schultz. Why has Soviet agriculture failed to perform better? The book consists of papers given by outside experts of Russian economics and politics. Some of the papers measure the performance of Russian agriculture, some of them compare its performance with

other areas of the world, and others examine the Russian agricultural system and recent changes in it.

To one who claims no expertise in matters relating to the Russian economy, including agriculture, the book is somewhat frustrating, as is often the case of material prepared by and for a highly specialized group of experts. For an outsider it tells both too little and too much. It tells too little about the general situation, which any Russian expert would know; and it tells too much about things which to the nonexpert would appear to be details which may obscure rather than illuminate these general issues. However, to the specialist in Russian affairs these may well be strengths rather than weaknesses.

The conclusion of this book would appear to be that the system of organization is the reason for the failure of Russian agriculture. In a broad sense this is undoubtedly true, but the question that remains is what specific parts of the system? What is the level of technology available to Russian agriculture and the quality of inputs in terms of their potential productivity? What is the level of education and technical competence at the different levels of decision making? How important is the transportation and distribution system for inputs and output in determining the usable agricultural output produced in Russia? How important is the complex delivery and pricing system in providing incentives for increased output? These are the questions Schultz is mainly concerned with in the world scene because he believes them to be major variables in explaining the differential performance of agriculture. Few of these questions are directly addressed in *Soviet Agriculture: The Permanent Crisis*, and even fewer are answered.

While the book on Soviet agriculture answers too few of these questions, it seems that Schultz may answer too many, too assuredly in his *Economic Crises in World Agriculture*. For instance, he asserts that our surplus disposal program to underdeveloped countries is an important contribution to the poor performance of agriculture in some countries via the program's price-depressing effects in the recipient country. No firm evidence is given for this position, nor do I know of any available elsewhere. A similar problem exists regarding his assertion that there are almost no inputs or techniques available to farmers in underdeveloped areas which would be profitable for producers to adopt. This is not to imply that Schultz is wrong in his assessment of economic conditions in world agriculture, but merely to suggest that substantially more specific economic information on these issues is needed.

In general, both of these books are addressed to the important question of the reasons for the varying performance of world agriculture. Both point a finger at one or more general factors that appear to be the root of the problem. Both deal with questions of fact which are subject to some differences in interpretation. For those persons concerned with improving the performance of agriculture, either as teachers, researchers, or policy-makers these books would appear to be useful inputs which ought to help increase the productivity of their efforts. Despite these improved inputs, much clearly remains to be done.

DALE E. HATHAWAY

Michigan State University

The Economic Demand for Irrigated Acreage—New Methodology and Some Preliminary Projections, 1954-1980. By VERNON W. RUTTAN. Baltimore: Johns Hopkins Press for Resources for the Future, 1965. Pp. xi, 139. $4.00.

Concern about natural resource adequacy for the future has led to a substantial professional effort to project or predict future requirements. Economists have agreed to the making of many such projections. There has been no particular reason that economists should have engaged in such activity because most of the contribution that their discipline could make in such work has been ruled out by assumption. In this study Vernon Ruttan attempts to do something about this state of affairs. It is heartening that Resources for the Future has supported his work. Despite the trenchant economic analyses that RFF-supported economists have made of past developments and contemporary government programs and agencies, their record in utilizing economics in projecting future "requirements" has not been as bright.

The book is a scholarly one and represents an advance in methodology. At the same time, the empirical findings should not be disregarded even though Ruttan is conservative in his interpretation of the results. This reviewer is willing to predict that Ruttan's work will have an indelible impact on future projection studies.

The technique was to estimate Cobb-Douglas regional production functions for agricultural production by the use of census data. Three models were developed: a productivity model, a demand model, and an equilibrium model. The productivity model is of a simple form with the value of farm products sold being a function of irrigated land and operating expenses. From this a marginal-value productivity function for irrigated land could be obtained. The demand model permitted the derivation of a demand function for irrigated land in each region. Given the specified level of output in each region and the equation for the production function, the model was solved to obtain the number of acres of irrigated land in each region which equates the annual marginal-value productivity of irrigated land with a specified annual return per acre of irrigated land. The equilibrium model consisted of three equations. One was the Cobb-Douglas production function mentioned earlier. Another was MVP functions for irrigated land and operating expenses for each region. The third was two identities equating the calculated MVP levels with budgeted average-cost levels.

The empirical results are interesting to anyone concerned with water resources policy. Space limitations prevent a thorough review here. Suffice it to say that the models bring into serious question the economic feasibility of much irrigation in the West under current conditions. Ruttan makes projections to 1980 using the demand model and the equilibrium models. These projections are then compared with estimates made by the Departments of Agriculture and Interior in their requirements approach. Ruttan's projections are more consistent with those of the Department of Agriculture than they are with those of the Department of Interior. They also suggest that agencies may be overestimating irrigation potential in the West and are underestimating it in the East. This reviewer would feel more comfortable with the results if he

knew more about some of the empirical data. For example, the costs per acre have considerable effect on the equilibrium solution. If these costs are not accurate in future estimates, the resulting equilibrium solutions would also be in error.

In making his projections Ruttan does not become a captive of his models. When the results appear "unrealistic," he examines the situation carefully to discover "why." He then introduces restraints to correct that which was incomplete in the original model. Although some may be critical of the admittedly arbitrary procedure, Ruttan makes the valid point that his projections "contain fewer arbitrary elements and are much more sensible from an economic point of view than many requirements projections upon which important decisions have been based."

Ruttan is justifiably cautious in arriving at his policy conclusions. The principal ones appear to be:

1. There is little, if any, economic justification in terms of meeting food and fiber needs of the next several decades in investing in irrigation facilities in which total costs are less than total returns on the basis of current prices.
2. Subsidizing a factor input such as irrigation water is a relatively inefficient form of income redistribution.
3. Federal policy with regard to irrigation in the West should be re-examined.
4. A substantial growth in irrigation may be economically practicable in eastern water resource regions.

In the opinion of this reviewer, the major contribution of the work is the methodological achievement of permitting economic forces to play a role in the making of projections. Ruttan is well aware that he has only opened the door a crack. Nevertheless, it may be the more important variables that have been admitted. Even if they have not, Ruttan has moved in the right direction. Should we wish to move farther in this direction, a more sophisticated basis than the rather elementary Cobb-Douglas production function will undoubtedly be needed. Nevertheless, a start has been made.

Emery N. Castle

Oregon State University

The Economics of Subsistence Agriculture. By Colin Clark and Margaret Haswell. New York: St. Martin's Press; London: Macmillan & Co., 1964. Pp. x, 218. $6.00; 30*s*.

"Subsistence agriculture" is a kind of catchall term. We come across it frequently these days, applied loosely to practically every kind of agricultural production not directly aimed at the market. Sometimes the unit of "subsistence agriculture" is the individual family, sometimes a village, sometimes a region, sometimes a whole rural sector, sometimes a whole country.

Colin Clark and Margaret Haswell had a splendid opportunity to introduce some order into this jumble-shop. They might have staked out the limits which separate subsistence agriculture from other kinds of farm economy.

They might have specified the level of analysis. They might have tried their hands at sketching the over-all functioning of subsistence agriculture as delimited and defined. Far from attempting any of these tasks, the authors have not even told us what they mean by subsistence agriculture.

The book begins with a rehash of the senior author's running controversy with the FAO about how hungry or ill-fed the world is today. This clash has previously been published, notably in the pages of the *Journal of the Royal Statistical Society* in 1961 and 1962 and again as part of the volume entitled *Food Supplies and Population Growth* (Edinburgh and London, 1963).

The FAO side of the controversy is by far more popular these days. The FAO tells us that between one-third and one-half of the world's population are hungry or malnourished, and that, with the upswing in population, things are likely to get much worse unless there is a sustained drive for a great increase in food production. It seems churlish to challenge this, particularly if one is living in one of the better-fed countries.

Far from being afraid of appearing churlish, Clark seems positively to enjoy it. He contends that a much smaller minority of the world's population, perhaps as low as 10 per cent, are really "hungry." A much larger percentage, perhaps 30 to 40 per cent, may be living on a poor and monotonous diet, but they are getting by.

My own feeling is that the issue between the FAO and Clark has not been properly posed. Both sides seem to exclude the possibility that people can eat more and, subjectively, feel hungrier. Yet this is precisely what I suspect has been happening in the one large area on which I feel qualified to speak, India. In the past 15 years the trend increase in food output has been of the order of 3 per cent per year, whereas the trend increase in population has been 2.5 per cent per year. Although more food has been available and ordinary people, until the recent steep price rise, have been eating more food and better food, nonetheless they will tell you in the villages that they are worse off. The fact is that their standards and levels of living have both risen, and that they are no longer content to accept the inadequate diet of the preceding generation.

After their presentation of the earlier controversy with the FAO, our authors take up a series of such topics as level of agricultural output, utilization of labor, man-land ratios, productivity, rents and prices of land, role of trade in relation to different methods of transport, and aid and development policies. Any and all of these topics are worth discussion, but the way the authors have gone about it is disconcerting. Their chief concern seems to have been to heap up mounds of quantitative data at any level (national, regional, village, or family) and of any quality. The material is drawn from all periods of history: ancient, medieval, or modern; from all continents; from all kinds of disparate inquiries—answers to oral and written questionnaires prepared by present-day economists, sociologists, or anthropologists, budget investigations of the 19th or 20th centuries, agricultural department data, statistical yearbooks. Perhaps since Africa is the continent about which I know the least, the data for that area seem to me the least objectionable!

When Clark's *Conditions of Economic Progress* first appeared in 1940, it was a pioneer, valuable, and exciting contribution. Nonetheless, the way in

which he threw together heterogeneous data to make tables, charts, scatter diagrams, functions and regressions was often and severely criticized, not least of all by Simon Kuznets in 1941. But Clark is irrepressible. Together with M. R. Haswell he has put together in *The Economics of Subsistence Agriculture* a series of 60 odd charts and tables which are open to the same basic objections as those in his *Conditions of Economic Progress*, while the text has none of the redeeming qualities or the interest of the earlier work. Prospective readers of *The Economics of Subsistence Agriculture* should look with a wary eye, for example, at the diagrams of average and marginal returns of agriculture, the treatment of rents and prices of agricultural land, the presentation of "preferred [*sic*] amount of agricultural work by Indian farm families," and the figures for income elasticities of demand for food and nonfood products in various eras and countries.

To a significant extent the haphazard character of the statistical materials purveyed by Clark and Haswell reflects their failure to define the nature, time-range, and coverage of subsistence agriculture. This vagueness means that they have no explicit criteria for judging what data are relevant or irrelevant. So long as the term subsistence agriculture remains a residual category, the present reviewer is inclined to feel that other researchers may be no more successful with it than Clark and Haswell have been. Perhaps the first thing to do now is to chop the term up.

DANIEL THORNER

Sorbonne, Paris

Labor Economics

The Emergence of an Industrial Labor Force in India—A Study of the Bombay Cotton Mills, 1854-1947. By MORRIS DAVID MORRIS. Berkeley and Los Angeles: University of California Press, 1965. Pp. 263. $7.50.

Professor Morris seeks to describe and explain "the creation of an industrial labor force." He selected one of the most important work forces in an underdeveloped area: the 200,000 cotton textile workers of Bombay City. The historical resources for such a study are rich. The book is largely based on a scrutiny of various government labor investigations and testimony presented thereto, the official written records of the Bombay Millowners Association and various individual mills, and on contemporary articles published in trade and professional journals. Unfortunately, the study stops at India's year of independence, 1947.

Two quite different (and sometimes incompatible) strains are incorporated: (1) this is an economic history of the Bombay cotton textile work force; and (2) it is a polemic against what Morris regards as an erroneous view of the nature of the Bombay cotton textile worker. Let us consider each of these in turn.

The book contains a provocative, albeit too brief, history of the Bombay cotton textile industry, a comprehensive analysis of the supply of labor, a brief history of the hours of work, a penetrating historical sketch of work reg-

ulations and the administration of the work force, a thoughtful investigation of the relation between wages, trade unions, and labor discipline, and an excellent bibliography.

We have little that can match the quality of this book on the economic history of the American labor force, let alone that for an underdeveloped country. Morris worked on this topic for over ten years, and the product fully reflects the painstaking effort expended. We need more such studies on the emergence of a labor force in the course of economic development.

This book is also a polemic. And he treats his predecessors roughly. Interwoven in his analysis is a systematic effort to destroy what he regards as certain well-established "myths" about the Bombay cotton textile labor force. It has been assumed by many writers (but not all) that the Bombay cotton worker was not fully "committed" to an industrial way of life. This has allegedly been reflected in high absenteeism, labor shortage, high labor turnover, low mobility between mills, and the existence of a "village nexus" which drove workers to return to their villages for extended periods each year.

Morris' study constitutes a broad scale attack on the above description of the Bombay cotton worker. This attack on the traditional view of the worker's instability seems well taken and is a valuable antidote to much that has been written. However, he probably overstates his case. He argues that absenteeism among Bombay cotton textile workers has actually been relatively low —probably never more than 10 per cent, and that illness, not lack of commitment to factory life, accounted for most of this. He concludes that with the exception of the plague years of 1896-98 the industry has never experienced a "labor shortage." He maintains that labor turnover was probably never high. Attachment to the individual mill greatly increased over time, and, as I have shown for the period since 1947, extremely low turnover is currently characteristic of the industry.

He appropriately emphasizes that until recently the jobber played the crucial role in hiring and firing and that workers often needed an attachment to a jobber to obtain employment. Yet, he strongly implies that a good deal of inter-mill movement occurred. These two contentions strike me as inconsistent. His excellent historical description of the jobber system in Chapter 8 suggests a much more compartmentalized labor supply (in keeping with the view of other writers) than he describes in Chapters 4 or 5, and assumes in Chapter 9.

Concerning the "village nexus" thesis, he presents scattered data suggesting that a substantial minority have not been returning to their villages each year. However, his evidence indicates a majority did, and my impression is that they still do.

The loose discipline and low level of labor utilization which Morris appropriately describes may contradict his contention that this has been a well-attached labor force. Certainly, it is possible that employers permitted such poor control over their workers because they were afraid they would be unable to hold them otherwise. Morris argues that because wages were low and represented a low percentage of total cost, labor productivity could be neglected, that the important thing was machine productivity and "the critical object

was to guarantee the continued operation of the [high cost] machines." This argument fails to allow for the fact that in cotton textiles loose labor discipline almost inevitably leads to idle machinery.

Morris' basic conclusion is that the disciplining and commitment of the Bombay cotton textile labor force did not pose a great problem. He argues that this has also probably been true of developing labor forces in general.

He presents his case well. We now need a first-rate current labor market study of the Bombay cotton textile workers. This will not only tell us about the present, but will also shed light on the validity of what Morris tells us about the past.

RALPH C. JAMES

University of California, Davis

The Market for College Teachers—An Economic Analysis of Career Patterns Among Southeastern Social Scientists. By DAVID G. BROWN. Chapel Hill: University of North Carolina Press, 1965. Pp. xv, 301. $7.50.

It is curious how few of us are willing to employ the equipment of economics to analyze the workings of our own profession and of the rather curious market for its services. How often we are content to direct our activities in that market on the basis of pure judgment, intuition, and perhaps some crude estimates of what constitutes a just price. Our economic training seems to have provided us relatively little insight into the subject—perhaps even less than most businessmen have into the operations of the markets in which they are involved.

Professor Brown has undertaken to increase our understanding of the academic market. While other books have been written on the subject, his is one of the few to be written by an economist and to be addressed primarily to his professional colleagues. The book adopts a supply-demand framework and deals with such important subjects as the cost and availability of information, impediments to mobility, and the objectives of those who seek jobs and of those who have positions to fill.

The bulk of the information on which the analysis is based was obtained from interviews. The sample was drawn from among economists, sociologists, and historians at 18 of the largest southeastern educational institutions; altogether 103 faculty members and 50 department chairmen were interviewed. To avoid problems arising out of faulty memory or inexperience with current market conditions the faculty respondents were all chosen among persons appointed since 1951, and most of them had taken their present post much more recently. Brown was well aware of the dangers inherent in the interview method and employed a questionnaire carefully designed to avoid at least the most obvious pitfalls.

The most interesting information that emerges from the study relates to the imperfections that characterize the market for our professional services—particularly the incompleteness of the information that is available to the typical buyer and seller. The author develops an admittedly crude but serviceable "Ig-

norance Index" for the job candidates, utilizing six questions such as "Did you visit the campus?" and "Did you know if other candidates were being seriously considered?" The index reports the proportion of questions answered negatively. The median index value was .24. The median candidate was uninformed on nearly a quarter of the questions at the time of his appointment. A similar index for chairmen indicates that their information was even more incomplete at the time the appointment was made. Brown argues that knowledge is so poor because the marginal cost of further information is so high and points out that where tenure appointments were involved information was generally far more complete. He recognizes that, particularly on first appointments, full information is very difficult to obtain—the institution from which the candidate comes is rarely candid, and he is usually not known by others in the profession.

The academic market's imperfection is also increased by its "balkanization." Though geographic impediments are less important for academics than for many other occupations, there are other bases for separation of our markets. Those who teach in church-related institutions tend to constitute a group that competes little with public liberal arts colleges; these in turn compete only slightly with the universities. More important, colleges and universities are segmented by quality of institution, and the institutions with considerable prestige do not participate to a great extent in the same markets as those with poorer reputations. Indeed, the author points out that only the institutions in the upper echelons operate in a truly national market, drawing their faculty members from schools of comparable reputation. Schools that are less well known are much more likely to draw their faculties from a narrower region.

Brown also concludes from his data that in selecting jobs, once a given salary level is met, faculty members are less interested in further salary differentiation than in other matters. While most respondents said they would be unwilling to accept a position that did not meet their annual salary requirement, more said that they chose their present position on the basis of reputation of the department and workload. Moreover, Brown concludes from questions based on published AAUP figures that the faculty members did not even know much about general salary levels at their own institutions (though this question may have been unfair in expecting them to have performed rather complex calculations with the AAUP data and to have remembered the results).

Brown's work is weakest when he turns to pure theory. His vague accusation that "since Adam Smith, respectable economic theory has held that wage and salary differentials are the primary forces acting upon the distribution of human resources" (p. 223) is surely unfounded, as a cursory reading of *The Wealth of Nations* would show. (See esp. Book 1, Ch. 10, Part 1.) To our knowledge theorists generally affirm that the wage is only part of the package that determines the attractiveness of a job. Similarly, his implication that an optimal allocation of resources requires us to know how to evaluate socially the social benefits of alternative uses of resources (pp. 260-61) seems to miss the point of the invisible-hand argument. But these matters do not constitute

the core of the book which, on the whole, is an illuminating and clear depiction of many of the characteristics of the highly imperfect market for professorial services.

PEGGY HEIM

American Association of University Professors

W. J. BAUMOL

Princeton University

Population; Welfare Programs; Consumer Economics

Report of the Commission on the Cost of Medical Care, Vol. 1. Chicago: American Medical Association, 1964. Pp. 182. No charge.

The Economics of Health. By HERBERT E. KLARMAN. New York: Columbia University Press, 1965. Pp. 200. $3.95.

The Demand for Hospital Facilities. By GERALD D. ROSENTHAL. Hospital Monograph Series, No. 14. Chicago: American Hospital Association, 1964. Pp. viii, 101. $3.00.

The first sentence of the introduction to the AMA's report of the Commission on the Costs of Medical Care is: "The physicians of America are the primary members of a health team which provides the highest quality medical care in the world." This leads to the expectation, which is unfulfilled, that evidence will be presented to support this contention. No effort is made either to present evidence that is relevant for evaluating the truth content of this assertion or to discuss what evidence could conceivably demonstrate the truth or falsity of this assertion. Hence as it stands, it constitutes a self-serving propagandistic effort by the financial sponsors of this volume.

Nevertheless, this volume is not without appeal for economists. It contains three chapters of interest: (1) "The Economics of Medical Care," dealing with the structure of the medical care market; (2) "The Medical Care Price Index," concerned with the measurement of the medical component of the Consumers Price Index; and (3) "The Demand for Medical Care," which is a statistical study of the variables related to various classes of medical care expenditures. Only the author of the third of these chapters, Paul J. Feldstein, is identified. An impressive list of consultants, which includes many economists, appears in the Preface.

The chapter on the economics of medical care is interesting because of what it does and does not contain. It primarily consists of a set of data that shows how medical care expenditures vary with income, race, medical insurance, sex, education, time, GNP, disposable income, etc. Another set of figures shows how the number of physicians, nurses, and dentists varies as a fraction of the total population, along with a breakdown of physicians in practice by specialty. Associated with these data are some elementary lessons in economics with applications to the medical care industry. These lessons often confuse demand and supply schedules with points on these schedules. Adding to the confusion generated thereby are statements such as: "Supply is a function of demand."

The unknown author or authors assert: "The medical care industry has as its prime social goal the development and maintenance of optimum health levels," and a physician, if he is to succeed, will ". . . subordinate his personal interests to those of his patients." No data on the income of physicians are presented that reveal the financial sacrifices associated with holding these lofty ideals.

The chapter dealing with the medical care price index takes up a problem that the Price Statistics Committee of the National Bureau of Economic Research considered more broadly and comes to roughly the same conclusions. The medical care component of the Consumers Price Index measures, with some exceptions such as obstetrical care, the prices of medical inputs instead of the conceptually correct prices of output, which are more difficult to measure. In a society characterized by rising real per capita incomes, one would expect input prices of products embodying large conponents of human capital to rise in a regime of stable final product prices. Unfortunately medical care is often not sold on a per ailment or per complaint basis but in terms of inputs. Consequently inputs typically are measured and the changes in the productivity of these inputs over time in treating specific ailments are not captured in the medical component of the Consumers Price Index. This suggests that this index is biased in its measurements of the changes in the costs of medical care; over time it overstates the rise in medical care prices. Yet if one compares the change over time in the obstetrical costs of having a baby and visits to a physician's office, these price changes are almost the same.

The analysis of the trends in medical care relative to other prices leaves something to be desired. "Thus, it has been only in the past 11 years that medical care prices have increased at a faster pace than All Items. This has occurred, however, not because medical care prices have been increasing at a faster rate than they had previously, but because the increase of other prices has moderated considerably." This is surely wrong. The author is implicitly contending that if the previous rate of change of All Items had been maintained in the past 11 years, the rate of change of the medical care index would remain unchanged. Presumably the relative rise in the cost of medical care is independent of changes in the level of the All Items index.

Probably the most interesting of the three papers, and of all of the works under review, is the statistical study by Paul Feldstein. Feldstein, using cross-section data, attempts to analyze the influence of tastes, income, and relative prices upon the demand for hospital care, physician services, drugs, and dental services. He is most successful in isolating the influence of income upon the quantities demanded, and clearly shows that the demand for medical care is income-elastic. To isolate the effects of tastes, he utilizes variables that are less than perfect indicators of tastes, such as age of family head, per cent of families with one or more members 65 and over, per cent of families with one or more members under five years of age, urbanization, marital status, and education. Income is more successfully isolated than tastes; should education increase or decrease expenditures for medical services? Feldstein's results show a decrease. It is with respect to price elasticity that Feldstein's results are especially interesting and noteworthy. Clearly most forms of medical in-

surance reduce the private marginal costs of medical care. Hence, the quantities utilized should increase; Feldstein's results show that they in fact do increase and are relatively more important for hospital expenditures than they are for physician expenditures. He also estimates price elasticity by looking at the relationship between average expenditures per physician visit for each of his primary sampling units, and the number of physician visits. He finds a weak negative relationship and interprets this finding as evidence of an inelastic demand. Possibly this is right if patient visits can be regarded as a homogenous commodity. If they cannot, then one would suspect that variations in expenditures for physician visits are associated with variations in the quality of the services provided. Feldstein has shown that the market substitutes more frequent visits to low-quality doctors for less frequent visits to high-quality physicians.

Herbert Klarman's study, which is sponsored by the Ford Foundation and carries an introduction by Marshall Robinson, tries to ". . . engage the interests of economists in the problems of the health field," and ". . . acquaint the intelligent lay public and professional health personnel with the approaches, analytical methods, and viewpoints that economists bring to the health field. . . ." The volume consists of a survey, often very uncritical, of the literature in the field of health economics. Preceding each chapter is a little lesson in economics which makes no contribution to the subsequent discussion and upon occasion is incorrect as in the case of demand when Klarman describes the prices of goods other than the good under investigation as being given.

In his chapter on demand, Klarman rejects the notion that the discriminating monopoly model is appropriate for analyzing the pricing behavior for doctors. The rationale for this position is that the demand curve for physicians' services is inelastic. The principal piece of evidence available for this assertion is the Feldstein study which assumes, erroneously, that differences in the costs of visits to physicians are not equalizing differences, i.e., that visits to physicians are homogeneous among his primary sampling units.

Another interesting problem Klarman discusses is the relatively high medical and dental expenditures of those who carry medical insurance as compared with the uninsured. He concludes that the purchasers of insurance have stronger tastes for medical insurance: "The insured spend more than the uninsured because they want to . . ." but then goes on to say that there is little reason to believe that there is adverse selection of risks for insurance carriers. It is difficult to see how both of these positions can be accepted unless adverse selection refers to something other than economic costs for insurers. Surely those with strong tastes for medical expenditures must find medical insurance especially attractive, even if it has no impact on their consumption of medical services. Medical insurance seems to be a proxy for strong tastes for medical services; this is a point that Feldstein failed to consider in his analysis.

Klarman deals with the question of the appropriate level of state intervention in private choices for medical care by citing various positions taken by writers in the area from Seymour Harris to Dennis Lees but never taking a position of his own or attempting to integrate disparate views.

Klarman's chapter on supply is preceded by a little lesson in the economics of cost and supply curves which is totally irrelevant for the discussion of the output of physicians if, as many believe, output is controlled by the AMA. Under these circumstances, what is relevant is the AMA physician output policy which will be geared to the demand conditions for medical services. This chapter contains almost no discussion of the control over medical schools by organized medicine, which is an important distinguishing characteristic of this profession and is highly relevant for understanding what determines the output of physicians.

Also pertinent, but not discussed, is the role of standards of medical education upon the costs of medical care. Many mistakenly agree that any increase in the quality and rigor of medical training is desirable, forgetting the impact of such a change upon the prices of physician services. Insofar as prices of physician services rise, the public is induced to substitute the services of chiropractors, faith healers, and self-treatment for physician services. Hence an increase in the standards of training and costs does not imply an increase in the standards of medical care for the population at large. Klarman notes that the Russans have a higher frequency, approximately 30 per cent more, of physicians per unit of population. Since these physicians are on average of lower quality than ours, the Russians have implicitly substituted quantity for quality.

In his discussion of physician incomes, Klarman takes up the question of whether the incomes of physicians are underreported. Those who contend they are underreported cite the existence of vacancies in health departments, and in HIP in New York, that carry salaries equal to or greater than the means reported by the Department of Commerce. Klarman counters this argument with the statement that doctors prefer not to undertake administrative work or be salaried and prefer solo practice. This is an incorrect argument: What is relevant is what's true at the margin; are there no doctors among the more than two hundred thousand in solo practice who do not have these tastes? Surely there must be a variety of tastes about these matters in the profession, and since a miniscule fraction are either salaried, doing administrative work, or both, one finds this argument difficult to accept.

One of the ideas that recurs in the literature of medical economics, and is reported by Klarman, is that the intensity with which hospital beds are used is independent of their frequency in the population. The reason for this phenomenon is not discussed by Klarman and the reader is left guessing as to the rationale for its existence. Such a rationale could be nonprice rationing of bed space. If there are unsatisfied buyers, an increase in the frequency of hospital beds per unit of population could be associated with no change in the intensity of their use. This is one of the questions Gerald Rosenthal's study of the demand for general hospital facilities seeks to answer. Unfortunately, this statistical study of the demand for general hospital facilities suffers from some shortcomings that raise serious questions about the validity of its findings.

The most useful variable that most investigators have found in studying the demand for medical care is income. This is typically the cornerstone of any

demand study in this field or in the field of consumption generally. Rosenthal's treatment of this variable is difficult to understand, given what has become common knowledge in recent years. He considers two categories of income, under two thousand, and six thousand and over. Such a procedure, looking at the extremes of the income distribution, will maximize the effects of transitory income and mask the very effects he seeks to uncover. This is by far the most important shortcoming of this very ambitious study. Rosenthal implicitly assumes that all charges for hospital beds measure the same thing. Hence all variations in prices are presumed to reflect differences in price for the same commodity. Yet there clearly are differences in quality and differences in what is included in the price of hospital beds. Consequently some of the price differences Rosenthal observed are equalizing differences.

Rosenthal is silent about the problem of multicollinearity among his independent variables when one suspects that they are highly correlated. All in all, it is difficult to accept Rosenthal's findings without a great deal of qualification.

REUBEN A. KESSEL

University of Chicago

Manpower and Education: Country Studies in Economic Development. Edited by FREDERICK HARBISON and CHARLES A. MYERS. New York: McGraw-Hill Book Co., 1965. Pp. xiii, 344. $9.00.

This volume is a valuable addition to the rapidly growing body of literature on the critical role of human resources in economic development. A companion piece and supplement to the editors' *Education, Manpower, and Economic Growth* (New York 1964), it contains a series of case studies on human resource problems and planning in some twelve developing countries, each written by an individual with on-the-scene experience in the country about which he writes, frequently as an adviser to national planners.

In their four-page introduction to the volume, Frederick Harbison and Charles Myers stress the multidimensional nature of human resource development and the consequent necessity for a "strategy" that integrates a broad range of programs. While the country studies collectively cover most of the relevant aspects, there is considerable variation among them in the problems chosen for analysis. For example, Immanuel C. Y. Hsü's interesting essay on higher education in Communist China focuses exclusively on a description and evaluation of trends in the objectives and content of Chinese higher education, with no reference to methods of manpower planning; on the other hand, two-thirds of Rudolph Blitz's paper on Chile presents detailed supply and demand projections for nine specific "high-level" occupational categories, together with a discussion of the methods whereby they were derived.

Despite this diversity in approach and the range of characteristics represented by the countries covered in the volume (Argentina, Peru, Puerto Rico, Iran, Indonesia, Senegal, Guinea, Ivory Coast, Nyasaland, and Uganda, in addition to those already mentioned) many of the manpower and educational problems alluded to in the papers are common to all or most of them. In addi-

tion to the twin problems of shortages of high-level manpower and surpluses of the unskilled that are virtually universal among developing economies, most of the authors refer to such matters as the geographical maldistribution of professional employees (particularly health personnel); the low ratios of subprofessional to professional personnel (e.g. one nurse for every three doctors in Chile); the qualitative deficiencies in all levels of formal education (e.g. emphasis on rote learning); inadequate arrangements for on-the-job training; and the dearth of innovative and entrepreneurial talent.

At the same time, there are important differences among countries that make each to some degree a unique case. For example, in his essay on "Manpower and Education in Puerto Rico," William H. Knowles points out that the success of Puerto Rico's "Operation Bootstrap" is of only limited usefulness as an example to other developing economies because of the special advantages that inhere in Puerto Rico's relationship to the United States. In an appropriate concluding essay in the volume, Guy Hunter shows how a complex of social, economic, and political factors have created quite different kinds of human resource problems in East Africa (Kenya, Uganda, and Tanganyika) and Malaya on the one hand, and in other Southeast Asian countries (Burma, Indonesia, Thailand, and the Philippines) on the other. The former are characterized by high-quality university education, but extreme shortages of native high-level manpower. The latter, on the contrary, have unemployed surpluses of university-trained manpower, and have sacrificed quality to quantity in educational expansion. Quite different human resource "strategies" are called for in the two instances.

The contributions of George B. Baldwin ("Iran's Experience with Manpower Planning") and of William F. Whyte ("High-level Manpower for Peru") were particularly rewarding to the reviewer. Baldwin sets forth explicitly a conceptual framework for manpower and educational planning in the light of which the Iranian experience is evaluated. He points out that manpower planning and educational planning are overlapping fields in the context of general development planning. Although the formation of occupational skills is a common ground of both, each may make significant contributions to development in other ways. "In addition to training activities outside the educational system, manpower (planning) is concerned also with systems of incentives and mechanisms of labor allocation. Education is concerned with values and goals fully as important to nation building as occupational skills" (p. 146). Although Baldwin's paper does not fully explore all of these dimensions of Iranian experience, it has the merit of defining the problem completely and accurately.

Whyte's paper, prepared in cooperation with Graciela Flores, is a broadguaged economic and sociological analysis of the human resource problem and prospects in Peru. An unusual aspect of the paper is the investigation of the degree to which the occupational preferences of Peruvian youth and the underlying values of Peruvian society are consistent with the requirements of economic development and growth. The findings are somewhat disquieting, and the authors raise the question whether it is possible to "devise a strategy . . . for changing the values of Peruvian culture" and "for changing the social

structure so that individuals with values appropriate to economic growth will more readily rise to leadership positions?" (p. 72). These are not technical "manpower" questions, but their fundamental importance cannot be doubted by any student of economic development.

The editors' deliberate policy of giving the contributors a relatively free hand with respect to the structure and precise content of their essays has made for a publication that should have wide appeal. Considering the volume as a whole, one might have wished for a somewhat longer introduction by the editors that would have elaborated more fully the several aspects of human resource planning—particularly those relating to labor market processes. Secondly, more attention might have been given in some of the papers to the political and social characteristics of the country that have such profound significance for successful planning. Finally, an index would have increased the volume's utility as a reference work. Nevertheless, the book provides a rich source of data on manpower and education, as well as insight into human resource problems in developing economies. Both students and practitioners in the fields of economic development, manpower, and education will find it indispensable.

HERBERT S. PARNES

Ohio State University

TITLES OF NEW BOOKS

General Economics; Methodology

BERLE, A. A. La réorganisation de l'économie américaine. Paris: Presses Univ. de France, 1965. Pp. viii, 251.

BOULDING, K. E. The year's developments in the Arts and Sciences: social sciences. Reprint from The Great Ideas Today 1965. N.P.: Encyclopedia Britannica, Inc., 1965, Pp. 255-86.

DOWD, D. F. Modern economic problems in historical perspective. 2nd ed. Boston: Heath, 1965. Pp. viii, 198. Paper, $2.75.

FLASH, E. S. Economic advice and presidential leadership—the Council of Economic Advisers. New York and London: Columbia Univ. Press, 1965. Pp. x, 382. $8.95.

LEAMER, L. The economist as teacher—informal essays on the collegiate teaching of economics. Monogr. C-13. Chicago: South-Western, 1965. Pp. vii, 77.

LEES, D. A., MEADE, J. E., SMITH, H., HERBERT, A. P., HARRIS, R., AND ILERSIC, A. R. Freedom or free-for-all? Essays in welfare, trade, and choice. Vol. 3 of the Hobart Papers. London: Inst. of Econ. Affairs, 1965. Pp. 319. Paper, 10s 6d.

LOMAX, K. S. The assessment of economic performance. An inaugural lecture, Leeds University, 7 December 1964. Cambridge, Eng.: Leeds Univ. Press. 1965. Pp. 25. Paper, 2s 6d.

MCGUIRE, M. C. Secrecy and the arms race—a theory of the accumulation of strategic weapons and how secrecy affects it. Cambridge: Harvard Univ. Press, 1965. Pp. vii, 249. $5.95.

MEADE, J. E. The stationary economy. Being volume one of a principles of political economy. Chicago: Aldine, 1965. Pp. 238. $6.

MELMAN, S. Our depleted society. New York: Holt, Rinehart and Winston, 1965. Pp. x, 366. $4.95.

SHONFIELD, A. Modern capitalism—the changing balance of public and private power. London, New York and Toronto: Oxford Univ. Press for the Royal Inst. of Internat. Affairs, 1965. Pp. xvi, 456. $10.50.

The Canadian Journal of Economics and Political Science decennial index, Vol. XXI-XXX (1955-1964). N.P.: Canadian Political Sci. Assn., 1965. Pp. 61.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

AGUILAR DA SILVA LEME, R. Contribuições à teoria de localizaçao industrial. Bull. no. 39. São Paulo, Brazil: Univ. de São Paulo, Faculdade de Ciencias Econ. e Adminis., 1965. Pp. x, 365. Mimeo.

ARROW, K. J. Aspects of the theory of risk-bearing. Yrjö Jahnsson Lectures. Helsinki: Yrjö Jahnssonin Säätio, 1965. Pp. 61.

BHATTACHARYYA, S. K. Capital longevity and economic growth—an analytical study. Calcutta: Bookland Private Ltd., 1965. Pp. vi. 146. Rs. 20.

CARNEGIE, A. The gospel of wealth and other timely essays. (Edited by E. C. Kirkland.) Cambridge: Belknap Press of Harvard Univ. Press, 1962, Pp. xx, 239. $1.75.

DEWEY, D. Modern capital theory. New York: Columbia Univ. Press, 1965. Pp. x, 238. $6.50.

ELLINGHAUS, U. W. Die Grundlagen der Theorie der Preisdifferenzierung. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. xii, 275. DM 35, Lw. DM 40.

HEUSS, E. Die verbindung von mikroökonomischer und makroökonomischer verteilungstheorie. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. 40. Paper, DM 6.

JAFFÉ, W., ed. Correspondence of Léon Walras and related papers. Vol. 2: 1884-1897. Amsterdam: North-Holland Pub. Co., 1965. Pp. xxvii, 763. $14; set of 3 volumes $40.

KAUDER, E. A history of marginal utility theory. Princeton: Princeton Univ. Press, 1965. Pp. xxii, 248. $6.50.

KRELLE, W., ed. Multiplikator, Gleichgewicht, optimale Wachstumsrate und Standortverteilung. Berlin: Duncker & Humblot, 1965. p. 137. DM 26.
Contributions by H. Gülicher, E. Scheele, W. Vogt, and H. J. Vosgerau.

KUCZYNSKI, M., ed. Tableau économique von François Quesnay [3, Ausgabe, 1759]. Berlin: Akademie-Verlag, 1965. Var. pp.

MEADE, J. E. Efficiency, equality and the ownership of property. Cambridge: Harvard Univ. Press, 1965. Pp. 92. $4.

OŻGA, S. A. Expectations in economic theory. London: Weidenfeld and Nicolson, 1965. Pp. 303. 55s.

PARETO, V. Libre-échangisme, protectionnisme, et socialisme. Geneva: Lib. Droz, 1963. Pp. viii, 346.

———. Oeuvres complètes. Vol. 3: Ecrits sur la courbe de la répartition de la richesse. New ed. Geneva: Lib. Droz, 1965. Pp. xxi, 48.

RAMAKRISHNA, K. T. Lectures on advanced economic theory. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. xii, 315. $10.50.

ROTH, G. J. Parking space for cars: assessing the demand. Univ. of Cambridge Dept. Applied Econ. occas. paper. Cambridge: Univ. Press, 1965. Pp. 81. Paper, $2.

SCHEELE, E. Einkommensverteilung und Wirtschaftswachstum. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. x, 263. DM 39.50, Lw. DM 44.

SHACKLE, G. L. S. A scheme of economic theory. New York: Cambridge Univ. Press, 1965. Pp. x, 209. $7.50.

ZAWADZKI, K. K. F. The economics of inflationary processes. London: Weidenfeld and Nicolson, 1956. Pp. 288. 50s.

Economic History; Economic Development; National Economies

ALBA, V. Alliance without allies—the mythology of progress in Latin America. New York, Washington and London: Praeger, 1965. Pp. vii, 244. $6.95.

APTER, D. E. The politics of modernization. Chicago and London: Univ. of Chicago Press, 1965. Pp. xvi, 481. $7.50.

BAER, W. Industrialization and economic development in Brazil. Homewood, Ill.: Irwin, 1965. Pp. xix, 309. $8.50.

BASTOS, H. Desenvolvimento ou escravidao. São Paulo, Brazil: Martins, 1965. Pp. 391.

BECKERMAN, W. and others. The British economy in 1975. Cambridge and New York: Cambridge Univ. Press, 1965. Pp. xxiv, 631. $15.50.

BELSHAW, C. S. Traditional exchange and modern markets. Englewood Cliffs, N. J.: Prentice-Hall, 1965. Pp. v, 149. $4.50; paper, $2.25.

DAVIDSON, B. R. The northern myth—a study of the physical and economic limits to agricultural and pastoral development in tropical Australia. Melbourne: Melbourne Univ. Press; London and New York: Cambridge Univ. Press, 1965. Pp. xviii, 283. $12.50.

DONALDSON, P. Guide to the British Economy. Baltimore: Penguin Books, 1965. Pp. 256. Paper, $1.25.

FURTADO, C. The economic growth of Brazil: a survey from colonial to modern times. Berkeley and Los Angeles: Univ. of California Press, 1965. Pp. x, 285. Paper, $1.95.

GUNEWARDENA, E. External trade and the economic structure of Ceylon 1900-1955. Colombo: Central Bank of Ceylon, 1965. Pp. xiv, 234.

GUTH, W., ed. Die Stellung von Landwirtschaft und Industrie in Wachstumsprozess der Entwicklungsländer. Berlin: Duncker & Humblot, 1965. Pp. 69. DM 11,60.
Contributions by O. Schiller, C. Hofmann, W. Hankel, A. Kruse-Rodenacker.

HANSEN, B. AND MARZOUK, G. A. Development and economic policy in the UAR (Egypt). Amsterdam: North-Holland Pub. Co., 1965. Pp. xv, 333. $9.80; 70s.

HEALEY, J. M. The development of social overhead capital in India 1950-60. Oxford: Blackwell, 1965. Pp. ix, 180. 35s.

HERMANN, M. V. Contribution and reward: an old and ever-new approach to economic development. New York: Exposition Press, 1965. Pp. 279. $6.50.

HOFFMANN, W. G. Das wachstum der Deutschen Wirtschaft seit der mitte des 19. Jahrhunderts. New York: Springer-Verlag, 1965. Pp. xxvi, 842. DM 138.

HOU, C. Foreign investment and economic development in China 1840-1937. Cambridge: Harvard Univ. Press, 1965. Pp. xiii, 306. $8.95.

JONES, P. d'A. The consumer society: a history of American capitalism. Baltimore: Penguin Books, 1965. Pp. 407. Paper, $1.25.

KLATT, W. ed. The Chinese model—a political, economic and social survey. New York: Oxford Univ. Press; Hong Kong: Hong Kong Univ. Press, 1965. Pp. xix, 233, iv. $4.

LINDER, S. B. Teoría del comercio y política comercial para el desarrollo. Mexico, D. F.: Centro de Estud. Monetarios Latinoamericanos (CEMLA), 1965. Pp. xiv, 247.

MAYHEW, H. Selections from *London Labour and the London Poor.* Introduction by John L. Bradley. New York: Oxford Univ. Press, 1965. Pp. xl, 238. $2.75.

MCKINNEY, J. C. AND THOMPSON, E. T., eds. The South in continuity and change. Durham: Duke Univ. Press, 1965. Pp. xii, 511. $10.

MIHALY, E. B. Foreign aid and politics in Nepal—a case study. New York: Oxford Univ. Press, 1965. Pp. vii, 202.

NEWMAN, P. C. New York's economic destiny: an economic analysis of the New York harbor area. Ridgewood, N.J.: Foreign Studies Inst., 1965. Pp. ii, 69.

ONSLOW, C., ed. Asian economic development. New York: Praeger, 1965. Pp. x, 242. $7.
Contributions by U Tun Wai, Gamani Corea, V. K. R. V. Rao, Lim Chong Yah, Nurul Islam, Puey Ungphakorn.

PINTO, A. Chile: una economía difícil. Mexico, D. F.: Fondo de Cultura Económica, 1964. Pp. 184. 16 pesos.

RIVKIN, M. D. Area development for national growth—the Turkish precedent. New York, Washington, and London: Praeger, 1965. Pp. 225. $15.

ROBINSON, E. A. G., ed. Problems in economic development—proceedings of a conference held by the International Economic Association. New York: St. Martin's Press, 1965. Pp. xxi, 626.

ROMANO, R. Prezzi, salari e servizi a Napoli nel secolo XVIII (1734-1806). Milan: Banca Commerciale Italiana, 1965. Pp. 162.

ROMANS, J. T. Capital exports and growth among U.S. Regions. New England research ser. no. 1. Middletown, Conn.: Wesleyan Univ. Press, 1965. Pp. xv, 230. $9.50.

SACHS, W. S. AND HOOGENBOOM, A. The enterprising Colonials—society on the eve of the Revolution. Chicago: Argonaut, 1965. Pp. xi, 236. $8.50.

SCHEEL, W. Aktuelle Probleme der Entwicklungspolitik. Keiler Vorträge no. 42. Kiel: Inst. für Weltwirtschaft, Univ. of Kiel, 1965. Pp. 16.

STALEY, E. AND MORSE, R. Modern small industry for developing countries. New York: McGraw-Hill, 1965. Pp. x, 435. $9.

SZERESZEWSKI, R. Structural changes in the economy of Ghana 1891-1911. London: Weidenfeld & Nicolson, 1965. Pp. 161. 36s.

TUCHTFELDT, E. Wachstumsprobleme der schweizerischen Volkswirtschaft. Kieler Vorträge no. 40. Kiel: Inst. für Weltwirtschaft, Univ. of Kiel, 1965. Pp. 23.

VELIZ, C., ed. Obstacles to change in Latin America. New York, Toronto and London: Oxford Univ. Press under the auspices of the Royal Inst. of Internat. Affairs, 1965. Pp. viii, 263. $6.75.

WARD, B. The decade of development—a study in frustration? A lecture delivered in London under the auspices of the Overseas Development Institute on May 3, 1965. London: Overseas Develop. Inst., 1965. Pp. 23. 3s 6d.

WILSON, G. W., GORDON, S., AND JUDEK, S. Canada: an appraisal of its needs and resources. New York: Twentieth Century Fund; Toronto: Univ. of Toronto Press, 1965. Pp. lxx, 453. $7.50.

WOOD, W. D. AND THOMAN, R. S., eds. Areas of economic stress in Canada: Proceedings of a conference, Queen's University, at Kingston, Ontario. Kingston: Indus. Relations Centre, Queen's Univ., 1965. Pp. ix, 221. $6.50; paper, $5.

WU, YUAN-LI. The economy of Communist China—an introduction. New York: Praeger, 1965. Pp. vii, 225. $6; paper, $2.25.

YOUNG, A. N. China's wartime finance & inflation 1937-1945. Harvard East Asian ser. no. 20. Cambridge: Harvard Univ. Press, 1965. Pp. xvii, 421. $10.

Comision Nacional de Valores: memoria anual 1964. Mexico, D. F.: Comision Nacional de Valores, *s.d.* Pp. 187.

Economic and social development program for Spain 1964-1967. Prepared by the Commissioner for Economic and Social Development Planning, Presidency of the Government of Spain. Baltimore: Johns Hopkins Press, 1965. Pp. xiii, 333. $15; £6 in U.K.
Originally pub. in Spain as: Plan de desarrollo económico y social para el periodo 1964-1967.

Economic Development Institute 1965-66. Washington, D.C.: Internat. Bank for Reconstruction and Development, 1965. Pp. 28.

Economic report on 1964. H. M. Treasury. London: H. M. Stat. Off., 1965. Pp. 32. Paper, 70c.

Essays on planning and economic development. Center of Research on Underdeveloped Economies research papers Vol. 2. Warsaw: Polish Scientific Pub., 1965. Pp. 182.

The new Brazil—prospects for stability and profits. New York: Business International, 1965. Pp. 60. Paper, $40.

Norges økonomi etter krigen (The Norwegian postwar economy.) Samfunnsøkon. stud. no. 12. Oslo: Central Bur. of Statistics of Norway, 1965. Pp. 437.
Statistical tables and Table of Contents are translated into English.

Statistical Methods; Econometrics; Social Accounting

BURN, D., SEAL, J. R. AND RATCLIFF, A. R. N. Lessons from central forecasting—three essays on the techniques and fallibility of statistical measurement and projection in steel, doctors and social insurance. London: Inst. of Econ. Affairs Ltd., 1965. Pp. 62. 7s 6d.

HART, P. E., MILLS, G. AND WHITAKER, J. K. Econometric analysis for national economic planning. Proceedings of the sixteenth symposium of the Colston Research Society held in the University of Bristol April 6th-9th, 1964. London: Butterworths, 1964. Pp. xii, 320. $14.50.

ISHIKAWA, S. National income and capital formation in Mainland China—an examination of official statistics. Tokyo: Inst. of Asian Econ. Affairs, 1965. Pp. xvii, 206.

MIERNYK, W. H. The elements of input-output analysis. New York: Random House, 1965. Pp. xi, 156. $2.95.
First published in 1957.

MUELLER, B. A statistical handbook of the North Atlantic Area. New York: Twentieth Century Fund, 1965. Pp. 239. $6.

STUVEL, G. Systems of social accounts. New York: Oxford Univ. Press, 1965. Pp. xiv, 270. $6.40.

THEIL, H., BOOT, J. C. G. AND KLOEK, T. Operations research and quantitative economics: an elementary introduction. Trans. from the Dutch Voorspellen en Beslissen. New York: McGraw-Hill, 1965. Pp. xiv, 258. $8.95.

TINTER, G. Econometrics. New York: Wiley, 1965. Pp. xiii, 370. Paper, $2.45.

Employment and earnings statistics for states and areas 1939-64. BLS bull. no. 1370-2. Washington: Supt. Docs., 1965. Pp. xliii, 652. $4.

Exploring 1970—some numerical results. A programme for Growth no. 6. Cambridge: M.I.T. Press for Dept. of Applied Econ., Univ. of Cambridge, 1965. Pp. xi, 107. $3.

Economic Systems; Planning and Reform; Cooperation

COLLETTE, J.-M. Politique des investissements et calcul économique—l'expérience soviétique. Paris: Ed. Cujas, 1965. Pp. x, 360.

CROSSMAN, R. H. S. The politics of socialism. New York: Atheneum, 1965. Pp. xi, 252. $5.95.

Business Fluctuations

GORDON, R. A. AND KLEIN, L. R., eds. Readings in business cycles. Homewood, Ill.: Irwin for American Economic Association, 1965. Pp. x, 731. $8.50.

PERKINS, J. O. N. Anti-cyclical policy in Australia 1960-1964. New York: Cambridge Univ. Press; Melbourne: Melbourne Univ. Press, 1965. Pp. v, 58. 75c; 7s 6d.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

CASTELLINO, O. Gli intermediari finanziari e la politica della moneta e del credito—l'esperienza italiana (1947-1963). Turin: G. Giappichelli, 1965. Pp. 173. L. 2200.

CROTEAU, J. T. La union de credito y su economia. Mexico: Ed. Roble, 1965. Pp. 263.

ERDMAN, H. E. AND LARSEN, E. Revolving finance in agricultural cooperatives. Madison, Wisc.: Mimir Pub., 1965. Pp. xi, 117. $3.50.

FARER, T. J., ed., Financing African development. Cambridge: M.I.T. Press, 1965. Pp. vii, 245. $6.

GURLEY, J. G. AND SHAW, E. S. La moneta in una teoria del finanziamento. Milan: Cassa di Risparmio delle Provincie Lombarde, 1965. Pp. xxx, 362.

HAHN, L. A. Rückblick und Ausblick. Vortrag, gehalten anlasslich seines 75. Geburtstags am 12, October 1964 in Frankfurt am Main. Erstmalig erschienen als Privatdruck der Deutschen Effecten-und Wechsel-Bank, Frankfurt a.M. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. 32. DM 4.

HARVEY, W. H. Coin's financial school. (Edited by R. Hofstadter.) Cambridge: Belknap Press of Harvard Univ. Press, 1963. Pp. vi, 254. $1.75.

KATONA, G. Private pensions and individual saving. Monogr. no. 40. [Ann Arbor]: Survey Research Center, Inst. for Soc. Research, Univ. of Michigan, 1965. Pp. xi, 114. $1.50.

KING, F. H. H. Money and monetary policy in China 1845-1895. Cambridge: Harvard Univ. Press 1965. Pp. ix, 330.

LEVY, M. E. Cycles in government securities, Pt. 2: Determinants of changes in ownership. New York: Nat. Indus. Conf. Board, 1965. Pp. 263. Paper, $4; $20, non-associates.

MACESICH, G. Commercial banking and regional development in the United States, 1950-1960. Florida State Univ. stud. no. 45. Tallahassee: Florida State Univ., 1965. Pp. xvii, 160. $5.50.

NADDA, N. L. Capital market in India. Patna: Bharati Bhawan, 1965. Pp. iv, 460; x, 8. Rs 20.

Nichols, D. M. Trading in federal funds—findings of a three-year survey. Washington: Board of Governors of the Fed. Reserve System, 1965. Pp. 116. Paper, $1.

Reichmann, M. T. La demanda de dinero—un intento de cuantificación. Santiago: Univ. de Chile Inst. de Econ., 1965. Pp. vii, 76.

Smith, W. L. and Teigen, R. L., eds. Readings in money, national income and stabilization policy. Homewood. Ill.: Irwin, 1965. Pp. x, 545. Paper, $5.50.

Tamagna, F. Central banking in Latin America. Mexico, D. F.: Centro de Estud. Monetarios Latinoamericanos (CEMLA), 1965. Pp. xii, 285.

Tormann, W. The investment institutions. Frankfurt a.M: Fritz Knapp Verlag, 1965. Pp. 83.

Wofford, B. M. Annual yield on investments in discounted paper and certain installment contracts. Research rept. no. 4. N. P.: College of Bus. Adminis., Bur. of Business and Econ. Research, Univ. of Nevada, 1965. Pp. 38. $2.50.

Los bancos y el mercado de valores en 1964. Mexico: Comision Nacional de Valores, 1965. Pp. 76.

The banking structure in evolution: a response to public demand. Preprint from 102nd Annual Report 1964, Administrator of National Banks. Washington: U.S. Treasury Dept., Off. of Comptroller of the Currency, 1965. Pp. 28.

Informe del grupo de estudio sobre la creacion de activos de reserva. Mexico, D. F.: Centro de Estud. Monetarios Latinoamericanos, (CEMLA), 1965. Pp. 139.

1965 fact book. Chicago: U. S. Savings and Loan League, 1965. Pp. 144.

Supplement to Banking and Monetary Statistics. Sec. 16 (new), Consumer Credit. Washington: Board of Governors, Federal Reserve System, 1965. Pp. 103. 65c.

Public Finance; Fiscal Policy

Gaa, C. J. Federal income taxation. 2 vols. Vol. 1: Fundamental questions, problems, and cases. Vol. 2: solutions. New York: McGraw-Hill, 1965. Pp. xix, 491; xix, 742. $9.75 each.

Haller, H. Zur Problematik eines rationalen Steuersystems. Kieler Vorträge no. 41. Kiel: Inst. für Weltwirtschaft, Univ. of Kiel, 1965. Pp. 27.

Heller, J. and Kauffman, K. M. Incentivos fiscales para el desarrollo industrial. Mexico, D. F.: Centro de Estud. Monetarios Latinoamericanos, (CEMLA), 1965. Pp. xiv, 321.

Hicks, U. K. Development finance—planning and control. New York: Oxford Univ. Press, 1965. Pp. 187. $3.50.

Howard, N. R., ed. The basic papers of George M. Humphrey as secretary of the Treasury 1953-1957. Cleveland: Western Reserve Historical Soc., [1965]. Pp. xxix, 644. $12.50.

Ketchum, M. D. and Strunk, N., eds. Conference on savings and residential financing. 1965 proceedings May 6 and 7, 1965, Chicago, Illinois. Chicago: U.S. Savings and Loan League, 1965. Pp. 192.

Maxwell, J. A. Financing state and local governments. Washington: Brookings Inst., 1965. Pp. xvii, 276. $5.

Ott, D. J. and Ott, A. F. Federal budget policy. Washington: Brookings Inst., 1965. Pp. xiv, 150. $5.

Phelps, E. S. Fiscal neutrality toward economic growth—analysis of a taxation principle. New York: McGraw-Hill, 1965. Pp. ix, 113. $6.95.

Rädler, A. J. Guides to European taxation. Vol. 2: Corporate taxation in the Common Market. Amsterdam: Internat. Bur. of Fiscal Documents, 1964. Var. pp., loose-leaf.

Shoup, C. S. The tax system of Brazil. A report to the Getúlio Vargas Foundation. [Rio de Janeiro]: Fundação Getúlio Vargas, Comissao de Reforma do Ministério da Fazenda, 1965. Pp. xvi, 85.

International Bureau of Fiscal Documentation annual report 1964. Amsterdam: Internat. Bur. Fiscal Doc., 1965. Pp. 37. Single copies free.

Management of public debt in India. New Delhi: Nat. Council of Applied Econ. Research, 1965. Pp. xi, 157. $5.

Maryland tax study. College Park: Bur. of Business and Econ. Research, College of Business and Public Administration, Univ. of Maryland, 1965. Pp. xxii, 366. $5; paper, $3.50.

Statistical yearbook of the Tel-Aviv—Yafo municipality 1964. Tel-Aviv: Dept. of Research & Stat., Municipal Corp. of Tel-Aviv—Yafo, 1965. Var. pp.

Statistics of income—1963, preliminary U. S. business tax returns—sole proprietorships, partnerships. U.S. Treasury Dept., Internal Revenue Ser. pub. no. 453 (9-65). Washington: Supt. Docs., 1965. Pp. 24. 20c.

Statistics of income 1962, U.S. business tax returns. Prepared under the direction of the Commissioner of Internal Revenue by the Statistics Division. Pub. no. 438 (8-65), Washington, D. C.: Supt. Docs, 1965. Pp. vi, 406. Paper, $2.25.

International Economics

Baerresen, D. W., Carnoy, M., and Grunwald, J. Latin American trade patterns. Washington: Brookings Inst., 1965. Pp. xix, 329. $6.

Boarman, P. M. The world's money—gold and the problem of international liquidity. Lewisburg, Pa.: Bucknell Univ. 1965. Pp. 25. Copies may be obtained by writing to the author; Grad. Inst. Internat. Studies, 132 rue de Lausanne, Geneva, Switzerland.

Elkan, P. G. Freer trade with Australia: why and how. Discussion paper no. 8. Wellington: New Zealand Inst. of Econ. Research, Inc., 1965. Pp. 25.

Galbraith, V. L. World trade in transition. Washington: Public Affairs Press, 1965. Pp. vi., 104. Paper, $2.50.

Haberler, G. Money in the international economy: a study in balance-of-payments adjustment, international liquidity, and exchange rates. Cambridge: Harvard Univ. Press, 1965. Pp. 47.

Hagras, K. M. United Nations conference on trade and development: a case study in U. N. diplomacy. New York, Washington and London: Praeger, 1965. Pp. xiii, 171. $15.

Hunsberger, W. S. Japan and the United States in world trade. New York and Evanston: Harper & Row, 1964. Pp. xvii, 492. $6.

Kruse, A. Aussenwirtschaft: die internationalen wirtschaftsbeziehungen. Berlin: Dunker & Humblot, 1965. Pp. xiv, 737. DM 68.

Machlup, F. and others. Planes de reforma del sistema monetario internacional. Mexico, D. F.: Centro de Estud. Monetarios Latinoamericanos (CEMLA), 1965. Pp. 257.

Marchal, A. L'Intégration territoriale. Paris: Presses Univ. de France, 1965. Pp. 128.

Morgan, D. J. British private investment in East Africa. Report of a survey and a conference. London: Overseas Develop. Inst., 1965. Pp. 63. Paper, 7s 6d.

Reid, E. The future of the World Bank—an essay. Washington: Internat. Bank for Reconstruction and Develop., 1965. Pp. v, 71.

Sen, S. India's bilateral payments and trade agreements 1947-48 to 1963-64. Calcutta: Bookland Private Ltd., 1965. Pp. xx, 336. Rs 35.

Strackbein, O. R. American enterprise and foreign trade. Washington: Public Affairs Press, 1965. Pp. iv, 193. $3.75.

Ward, R. International finance. Englewood Cliffs, N. J.: Prentice Hall, 1965. Pp. x, 213. $6.75.

Zolotas, X. Alternative systems for international monetary reform—a comparative appraisal. Bank of Greece papers and lectures no. 18. Athens: Bank of Greece, 1965. Pp. 46.

Comércio externo—comércio por mercadorias e países. Vol. 1: 1962. Lourenço Marques, Portugal: Direcção Provincial dos Servicos de Estatistica Geral [1965]. Pp. 506.

Fifth annual report 1964-1965 of the European Free Trade Association. Geneva: European Free Trade Assn., 1965. Pp. 43.

Guidelines for improving the international monetary system. Report of the Subcommittee on International Exchange and Payments to the Joint Economic Committee, 89th Cong., 1st sess. Washington: Supt. Docs., 1965. Pp. vi, 22. 15c.

Guidelines for international monetary reform. Hearings before the Subcommittee on International Exchange and Payments of the Joint Economic Committee, 89th Cong., 1st sess., July 27, 28, and 29, 1965. Washington: Supt. Docs., 1965. Pp. 191. 50c.

International Finance Corporation annual report 1964-1965. Washington: Internat. Finance Corp., 1965. Pp. 39.

International trade 1964. Geneva: GATT secretariat, 1965. Available in English, French, and Spanish. Sw. Fr. 8.50; US $2.

Simposio integracion economica. Santiago. Univ. de Chile Inst. de Econ., 1965. Pp. x, 95.

World Bank and IDA annual report 1964-65. Washington: World Bank and IDA, 1965. Pp. 84.

Business Finance; Investment and Security Markets; Insurance

Bevis, H. W. Corporate financial reporting in a competitive economy. New York: Macmillan; London: Collier-Macmillan, 1965. Pp. 212. $6.

Porterfield, J. T. S. Investment decisions and capital costs. Englewood Cliffs, N.J.: Prentice-Hall, 1965. Pp. viii, 152. Paper, $2.50.

Sobel, R. The big board—a history of the New York stock market. New York: Free Press 1965. Pp. xiii, 395. $7.95.

Business Organization; Managerial Economics; Marketing; Accounting

Farmer, R. N. and Richman, B. M. Comparative management and economic progress. Homewood, Ill.: Irwin, 1965. Pp. xiii, 436. $7.95.

March, J. G., ed. Handbook of organizations. Chicago: Rand McNally, 1965. Pp. xiv, 1247.

Neff, A. Der Haushalt-Grossgeräte-Markt von morgen. Dusseldorf: Econ-Verlag, GmbH, 1965. Pp. 200.

Odiorne, G. S. Management by objectives: a system of managerial leadership. New York: Pitman, 1965. Pp. ix, 204. $5.95.

Platt, P. L., Roche, J. G. and others. The MBA ["Master of Business Administration"] in American corporate life: a study of assimilation problems and procedures. New York: Hobbs, Dorman & Co., Inc., 1963. p. 87. $7.50.

Tybout, R. A., ed. Economics of research and development. Columbus: Ohio State Univ. Press, 1965. Pp. xx, 458. $7.50.

Wagner, E. Untersuchungen über die Kostenabhängigkeit der Geldnachfrage. Ein dynamisches Programmierungsmodell für die Kassenhaltung. Berlin: Duncker & Humblot, 1965. Pp. 79. Paper, DM 24.80.

Wernette, J. P., ed. Executive philosophy and practice. Ann Arbor: Grad. Sch. of Bus. Admin., Univ. of Michigan, 1965. Pp. 206. $4.

Computer technology—concepts for management. Papers presented at a symposium conducted by Industrial Relations Counselors, Inc. held at Greenwich, Connecticut May 7-8, 1964. Indus. Rel. monogr. no. 25. New York: Indus. Rel. Counselors, Inc., 1965. Pp. 120. Paper, $3.

Intercollegiate bibliography: cases in technological change and automation. Boston: Intercollegiate Case Clearing House, Harvard Grad. Sch. of Bus. Admin., 1965. Pp. 36. Paper, free samples to instructors; 35c, others.

Industrial Organization; Government and Business; Industry Studies

BOCK, E. A., ed. Government regulation of business—a casebook. Prepared for the Inter-University Case Program, Inc. Englewood Cliffs, N.J.: Prentice-Hall, 1965. Pp. xvi, 448. $9.95.

DROZE, W. H. High dams and slack waters—TVA rebuilds a river. Baton Rouge: Louisiana State Univ. Press, 1965. Pp. vii, 174. $7.50.

ILLERT, S. Der Preisvergleich mit empfohlenen Endverbraucherpreisen—eine Untersuchung der rechtlichen Grenzen preisvergleichender Werbung. Cologne: Carl Heymanns Verlag KG, 1965. Pp. 159.

KEFAUVER, E. In a few hands: monopoly power in America. Baltimore: Penguin Books, 1965. Pp. 245. Paper, $1.25.

KOCK, S. E. Företagsledning och Motivation. (Management and motivation—an inquiry into the correlation between certain personality variables among managers in the Finnish knitwear trade and the development and policy of the firms in question.) (With a discussion and summary in English.) Helsinki: Affärsekonomisk Förlagsförening, 1965. Pp. 284.

KUENNE, R. E. The attack submarine—a study in strategy. New Haven and London: Yale Univ. Press, 1965. Pp. x, 215. $6.

PARRIS, H. Government and the railways in nineteenth-century Britain. Toronto: Univ. of Toronto Press; London: Routledge & Kegan Paul, 1965. Pp. xii, 243. $5.75; 35s.

RAE, J. B. The American automobile: a brief history. Chicago and London: Univ. of Chicago Press, 1965. Pp. xiv, 265. $5.95.

THAYER, F. C., JR. Air transport policy and national security— a political, economic, and military analysis. Chapel Hill: Univ. of North Carolina Press, 1965. xxiii, 352. $8.

VAN DER LAAN, H. L. The Sierra Leone diamonds—an economic study covering the years 1952-1961. New York: Oxford Univ. Press for Fourah Bay College, Univ. College of Sierra Leone, 1965. Pp. xi, 234. $4.

VERRE, E., L'Entreprise industrielle en Union Soviétique. Adminis. des Entreprises Etudes et Docs. no. 1. Paris: Ed. Sirey, 1965. Pp. 266.

Documents on disarmament 1964. U.S. Arms Control and Disarmament Agency pub. 27. Washington: Supt. Docs., 1965. Pp. xv, 597. $1.50.

PARM system manual vol. 3, sec. L. Agriculture and food processing. NREC tech. rept. no. 54. Prepared by Philip M. Ritz and Lou P. Koenig, based in part on early work by Maaza Lemma. Washington: Nat. Planning Assn. for Math. and Computation Lab., Nat. Resource Evaluation Center, 1965. Var. pp.

PARM system manual vol. 3, sec. M. Manufacturing and mining. NREC tech. rept. no. 55. Prepared by Lou P. Koenig and Tong-Gyu Lim. Washington: Nat. Planning Assn. for Math. and Computation Lab., Nat. Resource Evaluation Center, 1965. Var. pp.

PARM system manual, Vol. 3, sec. N. Noncommodity activities, NREC tech. rept. no. 56. Washington: Nat. Planning Assn. for Mathematics and Computation Laboratory, Nat. Resource Evaluation Center, 1965. Var. pp.

Report of the Committee on the Economic Impact of Defense and Disarmament. Washington: Supt. Docs., 1965. Pp. ix, 92. 40c.

Transportation economics. A Conference of the Universities-National Bureau Committee for Economic Research. New York: Nat. Bur. Econ. Research; distrib., Columbia Univ. Press, 1965. Pp. xvii, 464. $10.

Land Economics; Agricultural Economics; Economic Geography; Housing

BERGER, J. S. Profile of the Los Angeles metropolis: residential finance census chart book. Pamphlet no. 4. Los Angeles: Real Estate Research Program, Grad. School Bus. Admin., Univ. of California, 1964. 13 charts.

BERNSTEIN, M. D. The Mexican mining industry 1890-1950—a study of the interaction of politics, economics, and technology. Albany: State Univ. of New York; Antioch Press, distrib., 1964. Pp. xvi, 412. $10.

BIEHL, M. Die ernährungswirtschaftliche Nutzbarmachung des Brahmaputra-Wassers für Indien und Pakistan. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. 140. vi.

BORGSTROM, G. The hungry planet: the modern world at the edge of famine. New York: Macmillan, 1965. p. xx, 487, $7.95.

BREIMYER, H. F. Individual freedom and the economic organization of agriculture. Urbana and London: Univ. of Illinois Press, 1965. Pp. viii, 314. $6.50; 50s.

DE KUN, N. The mineral resources of Africa. New York: Elsevier Pub. Co., 1965. Pp. xxvi, 740. $40.

DUNCAN, H. D. Culture and democracy—the struggle for form in society and architecture in Chicago and the Middle West during the life and times of Louis H. Sullivan. Totowa, N.J.: Bedminster Press, 1965. Pp. xxii, 616. $12.50.

FITE, G. C. Farm to factory: a history of the consumers cooperative association. Columbia: Univ. of Missouri Press, 1965. Pp. viii, 288. $6.

FRIEDMANN, K. J. German grain policies and prices: 1925-1964. Reprint from *Food Research Inst. Stud.,* 1965. *5*(1). Stanford: Food Research Inst., Stanford Univ., 1965. Pp. 68. Paper, $2.

GILLIES, J. AND BERGER, J. S. Profile of the Los Angeles metropolis—its people and its homes. Part 4, Financing homeownership: the borrowers, the lenders, and the homes. Research rept. no. 7. Los Angeles: Real Estate Research Program, Grad. School Bus. Admin., Univ. of California, 1965. Pp. ix, 77. Paper, $1.50.

GREEN, J. L. Metropolitan economic republics—a case study in regional economic growth. Athens: Univ. of Georgia Press, 1965. Pp. x, 206. Paper, $4.

HAVEMAN, R. H. Water resource investment and the public interest; an analysis of federal expenditures in ten southern states. Nashville: Vanderbilt Univ. Press, 1965. Pp. xiii, 199. $6.

HELMBERGER, P. G. AND HOOS, S. Cooperative bargaining in agriculture—grower-processor markets for fruits and vegetables. Berkeley: Univ. of California, Div. Agric. Sciences, 1965. Pp. iii, 234. Paper, $2.25.

KUIPER, E. Water resources development: planning, engineering and economics. Washington: Butterworths, 1965. Pp xi, 483. $24.50.

MONKHOUSE, F. J. A dictionary of geography. Chicago: Aldine, 1965. Pp. v, 344. $7.95.

NARAIN, D. The impact of price movements on areas under selected crops in India: 1900-1939. New York: Cambridge Univ. Press, 1965. Pp. ix, 234. $8.50.

NEUTZE, G. M. Economic policy and the size of cities. Canberra: Australian Nat. Univ., 1965. Pp. xi, 136. Aus. 30s; $3.

SINGH, D. Land reforms in Rajasthan: a study of evasion, implementation and socio-economic effects of land reforms. Rajasthan, India: Students Agency, Birla Vidyavihar, Pilani, 1964. Pp. xvi, 452. Rs22, $6, 42s.

SMIDT, S. A test of the serial independence of price changes in soybean futures. Stanford: Food Research Inst., 19665. Pp. 20. $1.

WALKER, K. R. Planning in Chinese agriculture—socialisation and the private sector, 1956-1962. Chicago: Aldine, 1965. Pp. xvii, 109. $5.

A case study of urban expansion and annexation, Harris County, Texas. Agric. econ. rept. no. 86. Washington: Supt. Docs., 1965. Pp. iv, 50.

Los productos alimenticios de consumo en fresco y el abastecimiento a las grandes ciudades. Madrid: Federacion Sindical de Comercia, *s.d.* Pp. 99.

Seminar on farm production planning and programming. Seminar ser. no. 7. Bombay: Indian Soc. Agric. Econ., 1965. Pp. viii, 127. Rs 8.

The shorter Oxford economic atlas of the world. 3rd ed. Prepared by the Economist Intelligence Unit and the Cartographic Department of the Clarendon Press. New York: Oxford Univ. Press, 1965. Pp. 106 + maps. $3.50.

Labor Economics

ALEXANDER, R. J. Organized labor in Latin America. New York: Free Press; London: Collier-Macmillan, 1965. Pp. x, 274. $5.95.

BOWEN, W. G. AND HARBISON, F. H., eds. Unemployment in a prosperous economy. A report of the Princeton Manpower Symposium, May 13-14, 1965. Princeton: Indus. Rel. Sec.; Woodrow Wilson School of Public and Internat. Affairs; Princeton Univ. Conf., 1965. p. vii, 173. Paper, $3.

CONLEY, R. W. The economics of vocational rehabilitation. Baltimore: Johns Hopkins Press, 1965. Pp. xii, 177. $6.

DANKERT, C. E., MANN, F. C., NORTHRUP, H. R., eds. Hours of work. New York: Harper & Row, 1965. Pp. viii, 208. $3.50.

FARMER, G. Management rights and union bargaining power—an assessment of Supreme Court and NLRB decisions. Indus. Rel. monogr. no. 26. New York: Indus. Rel. Counselors, Inc., 1965. Pp. 31.

FOENANDER, O. DE R. Shop stewards and shop committees—a study in trade unionism and industrial relations in Australia. Melbourne: Melbourne Univ. Press; London and New York: Cambridge Univ. Press, 1965. Pp. xi, 137. $7.50.

GORDON, M. S. Retraining and labor market adjustment in Western Europe. Washington: Supt. Docs., 1965. Pp. xvi, 226. $1.75.

JAMES, R. C. AND JAMES, E. D. Hoffa and the teamsters—a study of union power. Princeton, N.J.: Van Nostrand, 1965. Pp. xviii, 430. $6.95.

KUCZYNSKI, J. Die Geschichte der Lage der Arbeiter unter dem Kapitalismus. 2 vols. Berlin: Akademie Verlag, 1965. Pp. vii, 532. DM 44.

MARTIN, R. M. Whitecollar unions in Australia. Sydney: Australian Inst. of Polit. Sci., 1965. Pp. 40. Paper, 4s.

MILLER, R. W., ZELLER, F. A., AND MILLER, G. W. The practice of local union leadership: a study of five local unions. Columbus: Ohio State Univ. Press., 1965. Pp. xiv, 282. $6.25.

MOORE, W. E. The impact of industry. Englewood Cliffs, N.J.: Prentice-Hall, 1965. Pp. vii, 117. $4.50; paper, $2.25.

PIRKER, T. Die Histadrut—Gewerkschaftsprobleme in Israel. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. 134. DM 14; Lw. DM 18.

RHODES, G. Public sector pensions. Toronto: Univ. of Toronto Press for the Royal Inst. of Public Admin., 1965. Pp. 320. $7.

ROBERTS, H. S. AND BRISSENDEN, P. F., eds. The challenge of industrial relations in the Pacific-Asian countries. Honolulu: East-West Center Press, 1965. Pp. viii, 259. $6.50.

SOMERS, G. G., ed. Proceedings of the 1965 spring meeting of the Industrial Relations Research Association. Reprinted from *Labor Law Journal*, Aug., 1965 issue. Chicago: Commerce Clearing House, Inc., 1965. Pp. 451-513.

Turner, H. A. Wage trends, wage policies, and collective bargaining: the problems for underdeveloped countries. Univ. of Cambridge Dept. of Applied Econ. occas. paper no. 6. Cambridge: Cambridge Univ. Press, 1965. Pp. 76. Paper, $2.

Wolfbein, S. L. Employment, unemployment, and public policy. New York: Random House, 1965. Pp. xi, 210. Paper, $2.25.

Hard-core unemployment and poverty in Los Angeles. Prepared by staff of the Institute of Industrial Relations, University of California, Los Angeles, under a contract with the Area Redevelopment Administration. Washington: Supt. Docs., 1965. Var. pp. $2.75.

Ocupación y desocupación—Gran Santiago, Marzo de 1965. Inst. Econ. pub. no. 71. Santiago: Inst. de Econ., Univ. de Chile, 1965. Pp. ix, 37.

Ocupación y desocupación—Gran Santiago, Junio de 1965. Inst. Econ. pub. no. 72. Santiago: Inst. de Econ., Univ. de Chile, 1965. Pp. ix, 38.

Population; Welfare Programs; Consumer Economics

Anderson, C. A. and Bowman, M. J., eds. Education and economic development. Chicago: Aldine, 1965. Pp. x, 436. $10.75.

Cerych, L. Problems of aid to education in developing countries. New York: Praeger for Atlantic Inst., 1965. Pp. xiii, 213. $7.50.

Colberg, M. R. Human capital in southern development 1939-1963. Chapel Hill: Univ. of North Carolina Press, 1965. Pp. xv, 136. $5.

Ferman, L. A., Kornbluh, J. L., and Haber, A., eds. Poverty in America: A book of readings. Ann Arbor: Univ. of Michigan, 1965. Pp. xxv, 532. $9.

Freeman, R. A. Crisis in college finance? Time for new solutions. Washington: Inst. for Soc. Sci. Research, 1965. Pp. x, 243, $5.

Glass, D. V. and Eversley, D. E. C. Population in history—essays in historical demography. Chicago: Aldine, 1965. Pp. ix, 692. $17.50.

Hughes, R. D. and Leidheiser, H., Jr., eds. Exploring Virginia's human resources. Charlottesville: Univ. Press of Virginia, 1965. Pp. xi, 185. $4.

Iochi, R. Measurement of consumer price changes by income classes. Inst. of Econ. Research, Hitotsubashi Univ., econ. research ser. no. 7. Tokyo: Kinokuniya Book-Store Co., 1964. Pp. xii, 68.

Lind, G. Statistics of land-grant colleges and universities—year ended June 30, 1963 (final report). Circ. no. 763. Washington: Supt. Docs., 1965. Pp. viii, 136. $1.

McKie, J. W., ed. Education and the southern economy—a volume of essays. Chapel Hill: Southern Econ. Journal, 1965. Pp. xii, 128. $5; paper, $3.

Newman, P. Malaria eradication and population growth with special reference to Ceylon and British Guiana. Bur. of Pub. Health econ. research ser. no. 10. Ann Arbor: School of Public Health, Univ. of Michigan, 1965. Pp. xi, 259.

Pethe, V. P. Demographic profiles of an urban population. Bombay: Popular Prakashan, 1964. Pp. xvi, 144. Rs 15.

Seligman, B. B., ed. Poverty as a public issue. New York: Free Press; London: Collier-Macmillan, 1965. Pp. xiii, 359. $5.95.

Wallace, S. E. Skid Row as a way of life. Totowa, N.J.: Bedminster Press, 1965. Pp. vii, 219. $5.50.

Wilensky, H. L. and Lebeaux, C. N. Industrial society and social welfare—the impact of industrialization on the supply and organization of social welfare services in the United States. New York: Free Press; London: Collier-Macmillan Ltd., 1965. Pp. lii, 397. Paper, $2.45.

First pub. 1958 by Russell Sage Foundation.

Basic characteristics of social security in Chile. N.P.: Univ. of Chile, Inst. of Admin. Insora, Dept. of Indus. Relations, 1965. Pp. 92. Mimeo.

Digest of educational statistics—1965 edition. Dept. of Health, Educ., and Welfare bull. 1965, no. 4. Washington: Supt. Docs., 1965. Pp. xiv, 182. 70c.

Ekteskap, fødsler of vandringer i Norge—1856-1960. (Marriages, births and migrations in Norway—1856-1960.) Samfunnsøkon. stud. no. 13. Oslo: Central Bur. of Statistics of Norway, 1965. Pp. 221.
Statistical tables and Table of Contents are translated into English.

Fertility measurement—a report of the United States National Committee on vital and health statistics. Ser. 4, no. 1. Washington: Supt. Docs., 1965. Pp. vi, 26. Paper, 25c.

Personal health expenses—distribution of persons by amount and type of expense. United States: July-December-1962. Ser. 10, no. 22. Washington: Supt. Docs., 1965. Pp. 42. Paper, 30c.

Resident and extension enrollment in institutions of higher education, fall 1963. Dept. of Health, Educ., and Welfare, circ. no. 776. Washington: Supt. Docs., 1965. Pp. v, 88.

Selected health characteristics by occupation. United States—July 1961-June 1963. Ser. 10, no. 21. Washington: Supt. Docs., 1965. Pp. 79. Paper, 50c.

Related Disciplines

Beardsley, R. K., ed. Studies in Japanese culture: I. Ann Arbor: Univ. of Michigan Press, 1965. Pp. vi, 192.

Berelson, R. and Janowitz, M. Reader in public opinion and communication 2nd. ed. New York: Free Press; London: Collier-Macmillan, 1966. Pp. ix, 788. $8.95.

Byrnes, F. C. Americans in technical assistance: a study of attitudes and responses to their role abroad. New York: Praeger in cooperation with Internat. Programs, Michigan State Univ., 1965. Pp. xii, 156. $12.50.

Cataldo, B. F. and others. Introduction to law and the legal process. New York: Wiley, 1965. Pp. xiii, 880. $8.95.

Erdey-Grúz, T. and Trencsényi-Waldapfel, I., eds. Science in Hungary. Budapest: Corvina Press, 1965. Pp. 316.

Gilman, W. Science: U.S.A. New York: Viking Press, 1965. Pp. xii, 499. $7.95.

Hoover, C. B. Memoirs of capitalism, communism, and nazism. Durham: Duke Univ. Press, 1965. Pp. x, 302. $8.50.

Locke, J. Two treatises of government. A critical edition with an introduction and apparatus criticus by Peter Laslett. New York and Toronto: New American Lib., 1965. Pp. xiv, 576. 95c.

Lyons, G. M., ed. America: purpose and power. Chicago: Quadrangle Books, 1965. Pp. 384. $7.95.

Matras, J. Social change in Israel. Chicago: Aldine, 1965. Pp. x, 211. $6.95.

McNeil, W. H. The rise of the West: a history of the human community. New York and Toronto: New American Lib; London: New English Lib., 1965. Pp. 896. Paper, $1.50.

Naess, A. Gandhi and the nuclear age. Totowa, N.J.: Bedminster Press, 1965. Pp. vii, 149. $5.

Nutting, A. The Arabs. A narrative history from Mohammed to the present. New York and Toronto: New American Lib., 1965. Pp. 416. 95c.

Plamenatz, J., ed. Readings from liberal writers—English and French. New York: Barnes & Noble, 1965. Pp. x, 228. Paper, $1.75.

Randall, F. B. Stalin's Russia: a historical reconsideration. New York: Free Press, 1965. Pp. 328. $6.95.

Reitman, W. R. Cognition and thought—an information-processing approach. New York, London and Sydney: Wiley, 1965. Pp. xiii, 312. $7.95.

Toch, H. The social psychology of social movements. Indianapolis: Bobbs-Merrill, 1965. Pp. xiv, 257. $6.50.

WASKOW, A. I. Keeping the world disarmed. Santa Barbara, Calif.: Center for the Study of Democratic Institutions, 1965. Pp. 88. Paper, single copies free.

WHITE, L. D. The Federalists—a study in administrative history 1789-1801. New York: Free Press, 1965. Pp. xii, 538. Paper, $2.95.
First pub. in 1948.

———. The Jacksonians—a study in administrative history 1829-1861. New York: Free Press, 1965. Pp. xii, 593. Paper, $3.50.
First pub. in 1954.

———. The Jeffersonians—a study in administrative history 1801-1829. New York: Free Press, 1965. Pp. xiv, 572. Paper, $3.50.
First pub. in 1951.

———. The Republican era—a study in administrative history 1869-1901. New York: Free Press, 1965. Pp. ix, 405. Paper, $2.95.
First pub. in 1958.

An advertiser's guide to scholarly periodicals 1965-1966. New York: American Univ. Press Services, 1965. Pp. 290. Paper, $10.

The Brookings Institution annual report-1965. Washington: The Brookings Institution, 1965. Pp. viii, 127.

A constitution for the world. Santa Barbara, Calif.: Center for the Study of Democratic Institutions, 1965. Pp. 111. Paper, single copies free.

Intercultural education: an information service of education and world affairs. New York: Educ. and World Affairs, 1965. Pp. vii, 75. Single copies free.

Public papers of the presidents of the United States, Nov. 22, 1963-June 30, 1964. Book I. Washington: Off. of the Fed. Register, Nat. Archives and Records Serv., General Serv. Admin., 1965. $6.75.

Public papers of the presidents of the United States, July 1-December 31, 1964. Book II. Washington: Off. of the Fed. Register, Nat. Archives and Records Serv., General Serv. Admin., 1965. $7.

Some aspects of contemporary Chinese society. New Haven, Conn.: Human Rel. Area Files Press; New York: Taplinger, distrib., 1964. Pp. 52. Paper, 95c.

Some aspects of contemporary Pakistani society. New Haven, Conn.: Human Rel. Area Files ress; New York: Taplinger, distrib., 1964. Pp. 69. Paper, 95c.

Some aspects of contemporary Russian society. New Haven, Conn.: Human Rel. Area Files Press; New York: Taplinger, distrib., 1965. Pp. 70. Paper, 95c.

PERIODICALS

General Economics; Methodology

AGNATI, A. Di un criterio per determinare la durata degli entelechiani che modificano le variabili economiche endogene. Giorn. d. Econ., July-Aug. 1965, pp. 568-92.

MOSZKOWSKA, N. Methodologischer Subjektivismus in der Nationalökonomie. Schmollers Jahrb., 1965, *85*(5), pp. 513-24.

STIGLER, G. J. Textual exegesis as a scientific problem. Economica, Nov. 1965, pp. 447-50.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

AYDALOT, P. Note sur les économies externes et quelques notions connexes. Rev. Econ., Nov. 1965, pp. 944-73.

BAUMOL, W. J. Informed judgment, rigorous theory and public policy. So. Econ. Jour., Oct. 1965, pp. 137-45.

BECKER, G. S. A theory of the allocation of time. Econ. Jour., Sept. 1965, pp. 493-517.

BECKMANN, M. J. Einkommensverteilung und Wachstum bei nichtneutralem technischen Fortschritt. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 80-89.

BERNDTSON, H. Kapital, arbetskraft och kunskapsnivå sasom drivfjädrar för ekonomisk växt. Ekon. Samfundets Tids., 1965(2), pp. 95-108.

BERNHOLZ, P. Nochmals: Konsumtivzins und Minderschätuzung künftiger Bedürfnisse. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 176-81.

BIXLEY, B. Fixed factor proportions in classical economics: a note. Can. Jour. Econ. Pol. Sci., Nov. 1965, pp. 570-75.

BORCH, K. Theories and principles of productivity measurement at different levels. Prod. Meas. Rev., Aug. 1965, pp. 5-15.

BOSE, S. Economic growth in a two-sector planning model. Econ. Internaz., Aug. 1965, pp. 431-37.

BRONFENBRENNER, M. Formalizing the Shimomura growth model. Econ. Develop. and Cult. Change, Oct. 1965, pp. 85-93.

CAIRE, G. Un précurseur négligé: William Petty, ou L'approche systématique du développement économique. Rev. Econ., Sept. 1965, pp. 734-76.

CARAVALE, G. Per uno schema integrato di sviluppo e fluttuazioni. Riv. di Pol. Econ., June 1965, pp. 867-931.

CASS, D. Optimum growth in an aggregative model of capital accumulation. Rev. Econ. Stud., July 1965, pp. 233-40.

COLM, G. National goals analysis and marginal utility economics. Finanzarchiv, July 1965, pp. 209-24.

CUSIMANO, G. Flussi economici e flussi finanziari nella contabilità nazionale. Annali Fac. di Econ. e Com., 1965, *19*(1) pp. 3-120.

DAVIS, O. A. AND WHINSTON, A. B. Some notes on equating private and social cost. So. Econ. Jour., Oct. 1965, pp. 113-26.

DEL PUNTA, V. Modelli di decisione e programmazione economica: un'analisi critica. Riv. di Pol. Econ., Aug.-Sept. 1965, pp. 1210-52.

DEPOLLO, A. Un tentativo di modifica del modello Harrod-Domar. Giorn. d. Econ., July-Aug. 1965, pp. 606-22.

D'IPPOLITO, G. Una versione semplificata del modello Von Neumann in un contesto marginalistico. Econ. Internaz., May 1965, pp. 213-25.

EDVARDSEN, K. Selection of investment projects. Econ. of Planning, 1965, *5*(1-2), pp. 10-28.

EHRLICHER, W. Ansätze zu einer makroökonomischen Theorie der Marktformen. Jahrb. f. Nationalök. und Stat., Oct. 1965, pp. 297-315.

ELSNER, K. Wachstumstheorie und dynamisches Programmieren. Jahrb. f. Nationalök. und Stat., June 1965, pp. 481-93.

ENCARNACIÓN, J. On instability in the sense of Harrod. Economica, Aug. 1965. pp. 330-37.

FEI, J. C. H. AND RANIS, G. Innovational intensity and factor bias in the theory of growth. Internat. Econ. Rev., May 1965, pp. 182-98.

FERGUSON, C. E. The theory of multidimensional utility analysis in relation to multiple-goal business behavior: a synthesis. So. Econ. Jour., Oct. 1965, pp. 169-75.

FETTER, F. W. Economic controversy in the British Reviews, 1802-1850. Economica, Nov. 1965, pp. 424-37.

FLECK, F. H. Die Messung des technischen Fortschritts innerhalb des gesamtwirtschaftlichen Wachstumsprozesses. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 24-79.

FRANK, C. R. Entry in a Cournot market. Rev. Econ. Stud., July 1965, pp. 245-50.

FROSTMAN, L. The concept of time centre in investment theory and its relation to the average period of investment. Swedish Jour. Econ., June 1965, pp. 125-50.

FURUNO, Y. The period of production in two-sector models of economic growth. Internat. Econ. Rev., May 1965, pp. 240-44.

GAILLARD, Y. AND THUILLIER, G. Sur la thésaurisation. Rev. Econ., Sept. 1965, pp. 796-808.

GAMBINO, A. La politica monetaria e creditizia negle scritti di Costantino Bresciani Turroni. Studi Econ., May-Aug. 1965, pp. 196-210.

GANDOLFO, G. Un tentativo di inserimento del principio dell' "adeguamento dello stock di capitale" in un modello aggregato. Riv. di Pol. Econ., Oct. 1965, pp. 1427-53.

GORDON, B. J. Say's law, effective demand, and the contemporary British periodicals, 1820-1850. Economica, Nov. 1965, pp. 438-46.

GORMAN, W. M. Production functions in which the elasticities of substitution stand in fixed proportion to each other. Rev. Econ. Stud., July 1965, pp. 217-24.

GRASSINI, F. A. Il progresso tecnico come fattore dello sviluppo industriale italiano nel dopoguerra. Risparmio, June 1965, pp. 1079-1113.

GRILICHES, Z. AND WALLACE, N. The determinants of investment revisited. Internat. Econ. Rev., Sept. 1965, pp. 311-29.

GUILLEBAUD, C. W. The Marshall-Macmillan correspondence over the net book system. Econ. Jour., Sept. 1965, pp. 518-38.

GÜSTEN, R. Bemerkungen zur Marxschen Theorie des technischen Fortschritts. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 109-21.

HART, P. Business saving and errors in variables. Rev. Econ. Stud., July 1965, pp. 225-32.

HELMSTÄDTER, E. Investitionsquote und Wachstumsrate bei Harrod-neutralem Fortschritt. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 90-108.

HIRSHLEIFER, J. Investment decision under uncertainty—choice-theoretic approaches. Quart. Jour. Econ., Nov. 1965, pp. 509-36.

HOLLANDER, S. On the interpretation of the just price. Kyklos, 1965, *18*(4), pp. 615-33.

HOUTHAKKER, H. S. A note on self-dual preferences. Econometrica, Oct. 1965, pp. 797-801.

HUTT, W. H. Keynesian revisions. So. Afr. Jour. Econ., June 1965, pp. 101-13.

ISCHBOLDIN, B. Sombart's psychogenetic conception of capitalism. Riv. Internaz. di Sci. Econ. e Com., July 1965; pp. 645-54.

JOSSA, B. En torno a la inestabilidad del equilibrio en el modelo de Harrod. De Economia, April-June, 1965, pp. 241-64.

KLÄPPHOLZ, K. AND MISHAM, E. J. Las identidades en los modelos económicos. De Economia, April-June, 1965, pp. 265-75.

KRELLE, W. Beeinflussbarkeit und Grenzen des Wirtschaftswachstums. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 3-27.

LA TOURETTE, J. E. Sources of variation in the capital-output ratio in the United States private business sector, 1909-1959, Kyklos, 1965 *18*(4), pp. 635-51.

LÉCAILLON, J. Théorie de la monnaie et théorie de la répartition. Riv. Internaz. di Sci. Econ. e Com., Oct. 1965, pp. 948-57.

LOWE, A. La teoria clásica del crecimiento económico. De Economia, April-June, 1965, pp. 279-300.

MAEDA, H. A note on the efficient expansion paths of a firm. Bull. Univ. Osaka Prefecture, 1965, Ser. D, *9*, pp. 85-89.

MATTHIESSEN, L. Depreciation allowances, capital growth and the effective tax burden. Swedish Jour. Econ., Sept. 1965, pp. 208-39.

MORISHIMA, M. On the two theorems of growth economics: A mathematical exercise. Econometrica, Oct. 1965, pp. 829-40.

MOSBAEK, E. J. Information on the impact of reductions in defense expenditures on the economy. Quart. Rev. Econ. & Bus., fall 1965, pp. 47-67.

MUNDLAK, Y. AND HOCH, I. Consequences of alternative specifications in estimation of Cobb-Douglas production functions. Econometrica, Oct. 1965, pp. 814-28.

NAPOLEONI, C. Sulla edizione inglese degli "Éléments" di Walras. Giorn. d. Econ., July-Aug. 1965, pp. 634-48.

NARASIMHAM, G. V. L. Measurement and mechanism of economic growth of a country with particular reference to India. Econ. Internaz., May 1965, pp. 265-79.

NÄSLUND, B. Mathematical programming under risk. Swedish Jour. Econ., Sept. 1965, pp. 240-55.

NEUBAUER, J. A. Investitionen und Wachstum. Schmollers Jahrb., 1965, *85*(5), pp. 557-73.

NORDQUIST, G. L. The breakup of the maximization principle. Quart. Rev. Econ. & Bus., fall 1965, pp. 33-46.

O'BRIEN, D. P. The transition in Torrens' monetary thought. Economica, Aug. 1965, pp. 269-301.

POWELL, A. Post-war consumption in Canada: a first look at the aggregates. Can. Jour. Econ. Pol. Sci., Nov. 1965, pp. 559-65.

RAO, V. Y. AND SENGUPTA, J. K. An econometric application of a generalized Cobb-Douglas production function. Arthaniti, July 1964, pp. 113-24.

REY, G. M. Una misura della capacità produttiva utilizzata nel settore industriale. L'industria, July-Sept. 1965, pp. 360-71.

RUNGE, H. Verursachte und zugerechnete Produktivität. Schmollers Jahrb., 1965, *85*(5), pp. 549-56.

SAMUELSON, P. A. Using full duality to show that simultaneously additive direct and indirect utilities implies elasticity of demand. Econometrica, Oct. 1965, pp. 781-96.

SARAYDAR, E. Zeuthen's theory of bargaining: A note. Econometrica, Oct. 1965, pp. 802-13.

SAU, R. K. Intertemporal efficiency of capital accumulation and the von Neumann ray. Quart. Jour. Econ., Nov. 1965, pp. 642-48.

SCARPAT, O. Il progresso tecnico nella teoria neo-classica dello sviluppo. Giorn. d. Econ., July-Aug. 1965, pp. 593-605.

SCHMIDT, G. La etica economica del Judaismo. Rev. de Econ. Pol., 1965, *39*, p. 19.

SCHÖNFELD, P. Bemerkungen zum Zurechnungsproblem in prozessanalytischen Produktions- und Gleichgewichtsmodellen. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 80-98.

SKINNER, A. S. Economics and the problem of method. Scot. Jour. Pol. Econ., Nov. 1965, pp. 267-80.

SPENGLER, J. J. Today's circumstances and yesterday's theories: Malthus on "services." Kyklos, 1965, *18*(4), pp. 601-14.

STROTZ, R. H. The Keynesian model with a generalized money illusion. Riv. Internaz. di Sci. Econ. e Com., Oct. 1965, pp. 992-1009.

STURZENEGGER, A. C. Reflexiones en torno a las dinámicas de Domar y Harrod. (With English summary.) Económica, May-Aug. 1965, pp. 21-44.

TAKAYAMA, A. On a two-sector model of economic growth with technological progress. Rev. Econ. Stud., July 1965, pp. 251-62.

TAYLOR, L. Methods of estimating demand: an appraisal. Soc. Econ. Stud., June 1965, pp. 157-69.

THORELLI, H. B. The political economy of the firm: basis for a new theory of competition? Schweiz. Zeitschr. f. Volkswirtschaft und Stat., Sept. 1965, pp. 248-62.

TOMITA, S. The determination of distributive shares in a two-sector model. Keio Econ. Stud., 1964, *2*, pp. 17-34.

WADA, S. A two-sector model of technical progress and economic growth. Bull. Univ. Osaka Prefecture, 1965, Ser. D, *9*, pp. 16-24.

WALRAS, L. Doctrines économiques. Rev. Econ., Sept. 1965, pp. 731-33.

WEDDIGEN, W. Wirtschaft und Nächstenliebe. Schmollers Jahrb., 1965, *85*(4), pp. 385-401.

WEINRICH, J. Workable competition and the L-shaped cost curve. Riv. Internaz. di Sci. Econ. e Com., July 1965, pp. 667-91.

WELLS, P. Output and the demand for capital in the short run. So. Econ. Jour., Oct. 1965, pp. 146-52.

WINTER, S. G., JR. Some properties of the closed linear model of production. Internat. Econ. Rev., May 1965, pp. 199-210.

WU, D. M. An empirical analysis of household durable goods expenditure. Econometrica, Oct. 1965, pp. 761-80.

Spending and saving in urban and rural areas. Mo. Lab. Rev., Oct. 1965, pp. 1169-76.

Economic History; Economic Development; National Economies

ADELMAN, I. AND MORRIS, C. T. Factor analysis of the interrelationship between social and political variables and per capita Gross National Product. Quart. Jour. Econ., Nov. 1965, pp. 555-78.

ANSELMI, G., FRIGERI, V., SECCHI, R., AND SILVA, F. Ricerche di cinematica storica. Il movimento dei prezzi in Francia dal 1712 al 1725. Giorn. d. Econ., May-June 1965, pp. 438-63.

ARNDT, H. W. Australia—developed, developing or midway? Econ. Record, Sept. 1965, pp. 318-40.

BAUER, P. T. The new orthodoxy of economic development. Riv. Internaz. di Sci. Econ. e Com., Sept. 1965, pp. 829-47.

BIRD, R. The need for regional policy in a Common Market. Scot. Jour. Pol. Econ., Nov. 1965, pp. 225-42.

BORRINI G., ROSSI, A. M., AND COLOMBI, A. Ricerche di cinematica storica. Il movimento dei prezzi in Francia dal 1595 al 1635. Giorn. d. Econ., May-June 1965, pp. 468-90.

BOTTOMLEY, A. The fate of the artisan in developing economies. Soc. Econ. Stud., June 1965, pp. 194-203.

BOURGUINAT, H. La régionalisation des pays sous-développés. Une rationalité de substitution. Cahiers de L'I.S.E.A., March 1965, pp. 49-110.

BRUTON, H. J. On the role of import substitution in development planning. Philippine Econ. Jour., 1965, *4*(1), pp. 14-28.

DAL PANE, L. Les transformations des structures économiques de l'Italie unifiée (suite). Rev. Econ., Sept. 1965, pp. 689-707.

DAVIS, L. E. The investment market, 1870-1914: the evolution of a national market. Jour. Econ. Hist., Sept. 1965, pp. 355-99.

DAVIS, T. E. Changing conceptions of the development problem: the Chilean example. Econ. Develop. and Cult. Change, Oct. 1965, pp. 21-32.

DI NARDI, G. L'apporto dell'azione regionale allo sviluppo economico generale: Compatibilità degli obiettivi e dei programmi. Studi Econ., May-Aug. 1965, pp. 157-74.

ENCARNACIÓN, J. Two sector models of economic growth and development. Philippine Econ. Jour., 1965, *4*(1), pp. 1-13.

ERNST, M. Overstatement of industrial growth in Poland. Quart. Jour. Econ., Nov. 1965, pp. 623-41.

ETIENNE, G. La population et le développement économique du Pakistan. Population, Sept.-Oct. 1965, pp. 851-64.

FRANCIA, I. M. Inversión de capitales y establecimiento de nuevas industrias en Portugal. De Economia, April-June, 1965, pp. 343-54.

FRANCIA, I.M. AND DE PALMA, F. M. Informe sobre las posibilidades de inversión industrial en Grecia. De Economia, April-June, 1965, pp. 355-74.

FRIGERIO, M., BOCCARDI, C., SECCHI, C., AND FIORI, R. Ricerche di cinematica storica. Il movimento dei prezzi in Austria dal 1795 al 1855. Giorn. d. Econ., July-Aug. 1965, pp. 672-91.

FRISCH, R. Planning for the United Arab Republic. Econ. of Planning, 1965, *5*(1-2), pp. 29-42.

GUTH, W. Zum Rückzahlungsproblem der Entwicklungsländer. Ansatzpunkte für eine realistische Entwicklungspolitik. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 158-68.

HALLARÅKER, H. Sovjet-Uniones økonomiske utvikling. Stat. Tids., June 1965, pp. 99-116.

HANSEN, N. M. Regional planning in a mixed economy. So. Econ. Jour., Oct. 1965, pp. 176-90.

HANSON, P. Soviet living standards. Bull. Oxford Univ. Inst. Econ. and Stat., Aug. 1965, pp. 201-27.

HAZLEWOOD, A. The territorial incidence of the East African Common Services. Bull. Oxford Univ., Inst. Econ. and Stat., Aug. 1965, pp. 161-76.

HINRICHS, H. H. Determinants of government revenue shares among less-developed countries. Econ. Jour., Sept. 1965, pp 546-56.

HORWOOD, O. P. F. Economic balance, dualism and growth, with applications for the South African economy. So. Afr. Jour. Econ., Sept. 1965, pp. 171-92.

HUCHAPPA, G. T. Planning and monetary policy in India. Asian Econ. Rev., Aug. 1965, pp. 423-32.

HUNTER, H. Transport in Soviet and Chinese development. Econ. Develop. and Cult. Change, Oct. 1965, pp. 71-84.

IIDA, T. A non-neoclassical analysis of resource allocation in the dual economy. Econ. Jour., Sept. 1965, pp. 557-71.

JOHNSON, H. G. United States policy and the problems of developing countries. Jour. Bus. Univ. Chicago., Oct. 1965, pp. 337-43.

JÖRBERG, L. Structural change and economic growth: Sweden in the 19th century. Econ. and Hist., 1965, *8*, pp. 3-46.

KANE, E. J. Aggregating state data by Federal Reserve district: district per capita incomes, 1950-1960. Jour. Reg. Sci., summer 1963, pp. 69-76.

KELLEY, A. C. International migration and economic growth, Australia: 1865-1935. Jour. Econ. Hist., Sept. 1965, pp. 333-54.

KENDRICK, J. W. AND JAYCOX, C. M. The concept and estimation of gross state product. So. Econ. Jour., Oct. 1965, pp. 153-68.

KENNEDY, V. D. India: tendermindedness vs. tough problems. Indus. Rel., Oct. 1965, pp. 1-22.

Kenwood, A. G. Railway investment in Britain, 1825-1875. Economica, Aug. 1965, pp. 313-22.

Leclercq, H. and West, R. L. Economic research and development in Tropical Africa. Soc. Research, autumn 1965, pp. 299-320.

Lenotte, I. L'inflation dans les pays sous-développés. Recherches Econ. de Louvain, Sept. 1965, pp. 485-501.

Lewin, M. The immediate background of Soviet collectivization. Sov. Stud., Oct. 1965, pp. 162-97.

Lindblom, C. E. Has India an economic future? For. Affairs, Jan. 1966, pp. 239-52.

Marchal, A. El Plan de desarrollo español y las condiciones estructurales de la unificación europea. Transl. by C. M. Viñambres. (Orig. French text follows.) De Economia, April-June, 1965, pp. 221-40.

Markham, J. W. The market economy in historical perspective. Riv. Internaz. di Sci. Econ. e Com., July 1965, pp. 655-66.

Massad, C. Algunas oportunidades de investigación económica en América Latina. Trimestre Econ., Oct.-Dec. 1965, pp. 661-85.

Matuszewski, T. I. Some remarks on an econometric model of a provincial economy. Can. Jour. Econ. Pol. Sci., Nov. 1965, pp. 552-58.

Mazzocchi, G. Dualisme et disparités régionales. Rev. Econ., Sept., 1965, pp. 708-30.

McLoughlin, P. F. Teoria del "surplus labor" e realtà dello sviluppo: alcuni fatti concreti. Riv. di Pol. Econ., Oct. 1965, pp. 1454-68.

Minocha, A. C. Capital formation and the pattern of savings in developing economies. Asian Econ. Rev., Aug. 1965, pp. 433-43.

Mizoguchi, T. Statistical analysis of price system in Mainland China. Develop. Econ., Sept. 1965, pp. 305-22.

Müller, A. L. The economic position of the Asians in Africa. So. Afr. Jour. Econ., June 1965, pp. 114-30.

Nerlove, M. Two models of the British economy: a fragment of a critical survey. Internat. Econ. Rev., May 1965, pp. 127-81.

Newlyn, W. T. Gains and losses in the East African Common Market. Yorkshire Bull. Econ. Soc. Research, Nov. 1965, pp. 130-38.

Nove, A. Soviet economic progress. Lloyds Bank Rev., Oct. 1965, pp. 15-33.

Paelinck, J. La théorie du développement régional polarisé. Cahiers de L'I.S.E.A., March 1965, pp. 5-47.

Pal, M. N. A method of regional analysis of economic development with special reference to South India. Jour. Reg. Sci., summer 1963, pp. 41-58.

Pautard, J. La croissance de l'agriculture française depuis le début du XIX^e siècle et ses disparités régionales. Cahiers de L'I.S.E.A., April 1965, pp. 73-104.

Peroni, A. Le leggi dell'economia di sviluppo. Mondo Aperto, June-Aug., 1965, pp. 161-95.

Peterson, W. C. Transfer expenditures, taxes, and income distribution in France. Quart. Rev. Econ. & Bus., fall 1965, pp. 5-21.

Ranki, G. L'industria manifatturiera ungherese dal 1900 al 1938, suo sviluppo e suo posto in Europa. Annali, 1965, 4, pp. 198-208.

Rosenstein-Rodan, P. N. Industrialización de una región subdesarrollada. De Economia, April-June, 1965, pp. 389-95.

Rossi, A., Cappiello, M. T., Conserva, A., and Padoa-Schioppa, T. Ricerche di cinematica storica. Il movimento dei prezzi in Austria dal 1700 al 1740. Giorn. d. Econ., July-Aug. 1965, pp. 653-71.

Schapper, H. P. Economics and economists for economic and social advancement. Australian Jour. Agric. Econ., June 1965, pp. 1-16.

Singer, H. W. External aid: for plans or projects? Econ. Jour., Sept. 1965, pp. 539-45.

STRÜMPEL, B. Consumption aspirations: incentives for economic change. Soc. Econ. Stud., June 1965, pp. 183-93.

TAMAGNA, F. M. L'economia statunitense: politiche per uno sviluppo equilibrato. Risparmio, June 1965, pp. 1037-78.

URQUIDI, V. L. Nuevas consideraciones sobre la investigacion económica en América Latina. Trimestre Econ., Oct.-Dec. 1965, pp. 687-710.

VAN FENSTERMAKER, J. The statistics of American commercial banking, 1782-1818. Jour. Econ. Hist., Sept. 1965, pp. 400-13.

VAN PEETERSSEN, A. Essai d'établissement d'une fonction de consommation pour la Belgique, pour la période de 1961 à 1975. Cahiers Econ. de Bruxelles, 1965, *27*(3), pp. 437-46.

VELLA, G. F. I fatti dell'economia siciliana e la scienza economica. Annali Fac. di Econ. e Com., 1965, *19*(1), pp. 121-69.

VERLINDEN, C. Modernità e medioevalismo nell'economia e nella società coloniale americana. Annali, 1965, 4, pp. 1-60.

WOLF, C., JR. The political effects of economic programs: some indications from Latin America. Econ. Develop. and Cult. Change, Oct. 1965, pp. 1-20.

ZANELETTI, R. La teoria degli stadi di sviluppo e i sistemi economici eterogenei. Econ. Internaz., May 1965, pp. 235-62.

L'economie belge en 240 séries. Cahiers Econ. de Bruxelles, 1965, *27*(3), pp. 447-48.

Statistical Methods; Econometrics; Social Accounting

ARCELLI, M. Ordine e dipendenza causale nelle strutture economiche lineari. L'industria, July-Sept. 1965, pp. 335-59.

BACHARACH, M. Estimating nonnegative matrices from marginal data. Internat. Econ. Rev., Sept. 1965, pp. 294-310.

BLUMENTHAL, T. A test of the Klein-Shinkai econometric model of Japan. Internat. Econ. Rev., May 1965, pp. 211-28.

BONHOEFFER, F. O. AND MILICZEK, H. Zur regressionsanalytischen Untersuchung von ökonomischen Variablen mit Hilfe elektronischer Datenverarbeitungsanlagen. (With English summary.) Ifo-Stud., 1964, *10*(1/2), pp. 61-85.

BORCH, K. Theories and principles of productivity measurement at different levels. Prod. Meas. Rev., Aug. 1965, pp. 5-15.

FALISE, M. Essai d'application à une situation concrète déterminée de quelques instruments d'analyse en matière de répartition des revenus. L'évolution belge de 1953 à 1962. Rev. Econ., Sept. 1965, pp. 777-95.

FENSKE, R. W. An analysis of the meaning of productivity. Prod. Meas. Rev., Aug. 1965, pp. 16-22.

FISHER, F. M. The choice of instrumental variables in the estimation of economy-wide econometric models. Internat. Econ. Rev., Sept. 1965, pp. 245-74.

GUERRIERI, G. Talune proprietà caratteristiche delle distribuzioni esponenziali. Giorn. d. Econ., May-June 1965, pp. 427-37.

MILLER, R. E. Alternative optima, degeneracy and imputed values in linear programs. Jour. Reg. Sci., summer 1963, pp. 21-39.

NIITAMO, O. E. Taloudellisesta päätöksenteosta ja ohjelmoinnista epävarmuuden vallitessa. (With English summary—On decisions and programming under uncertainty.) Liiketaloudellinen Aikakauskirja, 1965 (3), pp. 246-81.

ROTHSCHILD, K. W. Theme and variations—remarks on the Kaldorian distribution formula. Kyklos, 1965, *18*(4), pp. 652-69.

SURÁNYI-UNGER, T. Sozialethische Aspekte der Einkommensverteilung in West und Ost (Italian summary). Riv. Internaz. di Sci. Econ. e Com., Nov. 1965, pp. 1036-89.

USHER, D. "Equalising differences" in income and the interpretation of national income statistics, Economica, Aug. 1965, pp. 253-68.

VIAN, F. Disposizione a blocchi con ripetizione ortogonale dei trattamenti in successivi periodi per la misura di effetti residui. Giorn. d. Econ., May-June 1965, pp. 400-426.

WINCKLER, W. Anwendung des x^2-Tests auf endliche Gesamtheiten. (With English summary.) Ifo-Stud., 1964, *10* (1/2), pp. 87-104.

Economic Systems; Planning and Reform; Cooperation

BALASSA, B. Whither French planning? Quart. Jour. Econ. Nov. 1965, pp. 537-54.

BRONFENBRENNER, M. *Das Kapital* for the modern man. Sci. & Soc., fall 1965, pp. 419-38.

DAL PANE, L. Vecchie polemiche sul liberismo e sul socialismo. Giorn. d. Econ., July-Aug. 1965, pp. 550-67.

DJORDJEVIC, J. Le système de planification en Yougoslavie. Rev. de Sci. Fin., July-Sept. 1965, pp. 418-45.

FONATANELLA, G. I trasporti terrestri nel "Progetto di programma" per il quinquennio 1965-69. Riv. di Pol. Econ., June 1965, pp. 933-58.

FRUMENTO, A. Metodi e fini dei vari tipi di programmazione economica. Giorn. d. Econ., May-June 1965, pp. 349-99.

GOLDMAN, J. Short- and long-term variations in the growth rate and the model of functioning of a socialist economy. Czech. Econ. Papers 5, 1965, pp. 35-46.

HENSEL, K. P. Establishing the economic order as a scientific problem. German Econ. Rev., 1965, *3* (3), pp. 181-92.

SIK, O. Problems of the new system of planned management. Czech. Econ. Papers 5, 1965, pp. 7-33.

VITO, F. Decentralization in a collectivist planned economy. Econ. Internaz., May 1965, pp. 197-209.

WICKMAN, K. Svensk långtidsplanering—metoder och problem. Ekon. Samfundets Tids., 1965(2), pp. 73-85.

ZAUBERMAN, A. On the objective function for the Soviet economy. Economica, Aug. 1965, pp. 323-29.

ZIELINSKI, J. G. Are there laws of planning? Econ. of planning, 1965, *5*(1-2), pp. 43-52.

Business Fluctuations

AIROV, J. The construction of interregional business cycle models. Jour. Reg. Sci., summer 1963, pp. 1-20.

BIRD, R. C., DESAI, M. J., ENZLER, J. J., AND TAUBMAN, P. J. "Kuznets cycles" in growth rates: the meaning. Internat. Econ. Rev., May 1965, pp. 229-39.

DUPRIEZ, L. H. De diagnostic de la conjoncture économique de la Belgique. Recherches Econ. de Louvain, Nov. 1965, pp. 591-610.

FELS, R. The U. S. downturn of 1948. Am. Econ. Rev., Dec. 1965, pp. 1059-76.

GARFIELD, F. R. Cycles and cyclical imbalances in a changing world. Fed. Res. Bull., Nov. 1965, pp. 1518-32.

GIUSSO, L. Politica della congiuntura e politica di sviluppo. Riv. di Pol. Econ., Aug.-Sept. 1965, pp. 1253-85.

JÖHR, W. A. Gegenwartsfragen der Konjunkturtheorie. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 50-69.

———. Spiel zur Simulation von Konjunkturprozessen. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 99-132.

SEMPERLOTTI, G. L'indagine sulle prospettive a medio termine dell'industria italiana. Riv. di Pol. Econ., Aug.-Sept. 1965, pp. 1286-1307.

SENGUPTA, J. K. Income stabilization policy: an elementary empirical analysis. Econ. Internaz., Aug. 1965, pp. 413-27.

Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

ALI, A. AND HITTI, S. Monetary experience of an oil economy. Fin. and Develop., Dec. 1965, pp. 223-29.

BERMAN, L. S. The flow of funds in the U. K. (with discussion). Jour. Royal Stat. Soc., 1965, Ser. A, *128*(3), pp. 321-60.

BROWN, C. V. A theory of interest rates or asset prices? Scot. Jour. Pol. Econ., Nov. 1965, pp. 297-308.

CROUCH, R. L. The genesis of bank deposits: New English version. Bull. Oxford Univ. Inst. Econ. and Stat., Aug. 1965 pp. 185-99.

DICKMAN, A. B. The South African money market—progress and problems since 1960. So. Afr. Jour. Econ., Sept. 1965, pp. 213-36.

GRAMLEY, L. E. AND CHASE, S. B., JR. Time deposits in monetary analysis. Fed. Res. Bull., Oct. 1965, pp. 1380-1406.

HOLLAND, T. E. A note on the "traditional theory" of the term structure of interest rates and rates on three- and six-month treasury bills. Internat. Econ. Rev., Sept. 1965, pp. 330-36.

JOHNSON, H. L. Margin requirements and stock market credit. Bus. Govt. Rev., Univ. Missouri, Nov.-Dec. 1965, pp. 5-10.

KUWATA, K. Creation of loanable funds and monetary policy. Bull. Univ. Osaka Prefecture, 1965, Ser. D., *9*, pp. 68-84.

MAO, J. C. T. AND MADEO, L. Portfolio selection with computers. Engin. Econ., fall 1965, pp. 10-16.

MICHAELSEN, J. B. The term structure of interest rates and yields on government securities. Jour. Fin., Sept. 1965, pp. 444-63.

MORGAN, E. V. Una politica monetaria per la stabilità dello sviluppo. L'industria, April-June 1965, pp. 185-206.

———. Una politica monetaria per la stabilità dello sviluppo (II). L'industria, July-Sept. 1965, pp. 391-412.

PORTER, R. C. Narrow security markets and monetary policy: lessons from Pakistan. Econ. Develop. and Cult. Change, Oct. 1965, pp. 48-60.

ROSENBLATT, S. M. The Federal Reserve System after fifty years. Quart. Rev. Econ. & Bus., fall 1965, pp. 69-85.

ROSSI, L. A proposito di valore della moneta e livello dei prezzi. (With English summary.) Riv. Internaz. di Sci. Econ. e Com., Oct. 1965, pp. 1010-18.

SEN, S. N. The penalty discount rate. Arthaniti, July 1964, pp. 99-112.

SHEARER, R. The foreign currency business of Canadian chartered banks. Can. Jour. Econ. Pol. Sci., Aug. 1965, pp. 328-57.

SHIMAMURA, T. Analysen des Konjunktur-Tests der Bank von Japan. (With English summary.) Ifo-Stud., 1964, *10* (1/2), pp. 19-32.

TOBIN, J. Money and economic growth. Econometrica, Oct. 1965, pp. 671-84.

WHALEN, E. L. A cross-section study of business demand for cash. Jour. Fin., Sept. 1965, pp. 423-43.

ZANELETTI, R. Un tentativo di generalizzazione della teoria dell'inflazione. L'industria, April-June 1965, pp. 216-36.

Il sistema bancario tunisino e il finanziamento dell'agricoltura. Servizio Studi della Banque Nationale Agricole di Tunisia. Risparmio, Aug. 1965, pp. 1498-1521.

Interest rates in capital markets. Fed. Res. Bull., August 1965, pp. 1053-65.

Public Finance; Fiscal Policy

Böhler, E. Das Anschlussprogramm zu den schweizerischen Konjunkturbeschlüssen. Wirtschaft und Recht, *2* (1965). pp. 65-70.

Brochier, H. Les problèmes de l'impôt indirect dans les pays en voie de développement. Rev. de Sci. Fin., July-Sept. 1965, pp. 369-85.

Buchanan, J. M. and Forte, F. Scelte fiscali attraverso il tempo: Una argomentazione a favore delle imposte indiritte. Studi Econ., May-Aug. 1965, pp. 175-95.

Cornwall, J. The structure of fiscal models. Quart. Jour. Econ., Nov. 1965, pp. 608-22.

Davin, E. Économie régionale et finances publiques. Rev. de Sci. Fin., July-Sept. 1965, pp. 386-417.

Diamond, P. A. National debt in a neoclassical growth model. Am. Econ. Rev., Dec. 1965, pp. 1126-50.

Gandenberger, O. Das Maximum des Fiskalertrages bei der speziellen Verbrauchsteuer im Konkurrenzfall. Finanzarchiv, July 1965, pp. 225-32.

Letsch, H. Probleme der Bundesfinanzpolitik. Eine Diskussionsskizze für die Standorts- und Kursbestimmung im Bereich der Bundesfinanzen. Wirtschaft und Recht, *2* (1965), pp. 87-137.

Merrett, A. J. The capital gains tax. Lloyds Bank Rev., Oct. 1965, pp. 1-14.

Nel, L. W. A. Some aspects of the incidence and structure of the central government's taxation and its significance as a stabilising measure: an empirical study. Fin. & Trade Rev., March-June 1965, pp. 167-84.

Nieto, A. L. Variantes de la ratio capital-producto en el anàlisis de proyectos. Rev. de Econ. Pol., Jan.-April 1965, pp. 63-81.

Saper, J. and Sweeney, T. The fiscal problems of less developed countries. Fin. and Develop., Dec. 1965, pp. 230-35.

Schmidt, K. Zu einigen Theorien über die relative Ausdehnung der öffentlichen Ausgaben. Finanzarchiv, July 1965, pp. 193-208.

Taubman, P. The effects of ad valorem and specific taxes on prices. Quart. Jour. Econ., Nov. 1965, pp. 649-56.

Venzke, H. Die gesamtwirtschaftlichen Auswirkungen der Umsatzbesteuerung aus kreislauftheoretischer Sicht. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 133-60.

Voyatzis, V. Die griechischen Unabhängigkeitsanleihen. Ein Beitrag zur Geschichte der Staatsfinanzierung im 19. Jahrhundert. Schmollers Jahrb., 1965, *85*(4), pp. 403-18.

Wittmann, W. Wachstumsaspekte des schweizerischen Steuersystems. Zeitschr. f. Nationalök., 1965, *25*(1-2), pp. 161-75.

Yamamoto, M. The balanced budget theorem modified. Bull. Univ. Osaka Prefecture, 1965, Ser. D, *9*, pp. 90-99.

Fiscal policy and debt management. Fed. Res. Bull., Nov. 165, pp. 1507-17.

International Economics

Bardhan, P. K. Optimum accumulation and international trade. Rev. Econ. Stud., July 1965, pp. 241-44.

Chipman, J. A survey of the theory of international trade: Part 2, the neo-classical theory. Econometrica, Oct. 1965, pp. 685-760.

Coppock, D. J. The alleged case against devaluation. Man. School Econ. Soc. Stud., Sept. 1965, pp. 285-312.

Fei, J. C. H. and Paauw, D. S. Foreign assistance and self-help: A reappraisal of development finance. Rev. Econ. Stat., Aug. 1965, pp. 251-67.

Flore, V. D. L'importanza dei trasporti marittimi nello sviluppo del commercio estero italiano con particolare riguardo ai servizi di linea. Mondo Aperto, June-Aug. 1965, pp. 195-227.

FORD, J. L. On the structure of, and gains from, international trade. Kyklos, 1965, *18*(4), pp. 670-84.

GEHRELS, F. The evaluation of trade from national and world viewpoints. Riv. Internaz, di Sci. Econ. e Com., July 1965, pp. 629-44.

HUPPERT, W. Systematik der Integration. Zur Theorie der wirtschaftlichen Integration und zur Problematik der EWG. Schmollers Jahrb., 1965, *85*(4), pp. 449-75.

JONES, R. W. Duality in international trade: a geometrical note. Can. Jour. Econ. Pol. Sci. Aug. 1965, pp. 390-93.

KENEN, P. B. AND YUDIN, E. B. The demand for international reserves. Rev. Econ Stat., Aug. 1965, pp. 242-50.

KENNEDY, C. The case for equities as a part of official external assets. Soc. and Econ. Stud. (Jamaica), Sept. 1965, pp. 245-51.

MABUCHI, T. A comment on Harrod's increasing cost in his "law of comparative costs." Bull. Univ. Osaka Prefecture, 1965, Ser. D, *9*, pp. 55-67.

MAY, R. S. Direct overseas investment in Nigeria, 1953-63. Scot. Jour. Pol. Econ., Nov. 1965, pp. 243-66.

OPPENHEIMER, P. M. Is Britain's worsening trade gap due to bad management of the business cycle? Bull. Oxford Univ. Inst. Econ. and Stat., Aug. 1965, pp. 177-83.

PATRIARCA, G. Sulla costituzionalità dei diritti antidumping. Riv. di Pol. Econ., June 1965, pp. 959-82.

PEARCE, I. F. AND ROWAN, D. C. A framework for research into the real effects of international capital movements. Riv. Internaz. di Sci. Econ. e Com., Oct. 1965, pp. 958-91.

POWRIE, T. L. On buying cheap and selling dear: another note. Can. Jour. Econ. Pol. Sci., Nov. 1965, pp. 566-69.

SALIN, P. La controverse des changes flexibles et le problème de l'équilibre simultané interne et externe (premiére partie). Econ. Internaz., Aug. 1965, pp. 371-412.

SCHMID, E. Perspektiven einer Regelung der wirtschaftlichen Beziehungen der Schweiz mit der EWG. Wirtschaft und Recht, *3* (1965), pp.162-73.

SOLIGO, R. AND STERN, J. J. Tariff protection, import substitution and investment efficiency. Pakistan Develop. Rev., summer 1965, pp. 249-70.

TRIAS FARGAS, R. Der export im Spanischen Wirtschafts-Entwicklungsplan. Jahrb. f. Nationalök. und Stat., June 1965, pp. 550-72.

TRIFFIN, R. International monetary arrangements, capital markets and economic integration in Latin America. Jour. Common Market Stud., Oct. 1965, pp. 70-104.

YAMANOUCHI, K. Changes in markets in Chinese foreign trade and their background. Develop. Econ., Sept. 1965, pp. 286-304.

Business Finance; Investment and Security Markets; Insurance

BABA, M. AND MATSUGI, T. Umsatzerwartungen und Investitionspläne. Eine Analyse Japanischer Erwwartungsdaten. (With English summary.) Ifo-Stud., 1964, *10*(1/2), pp. 1-17.

BARRÉRE, A. Les déterminants réels de l'incitation à investir en longue période. Riv. Internaz. di Sci. Econ. e Com., Oct. 1965, pp. 925-47.

DUVALL, R. M. AND AUSTIN, D. V. Predicting the results of proxy contests. Jour. Fin., Sept. 1965, pp. 464-71.

EDGE, S. K. Shareholders' reaction to rights issues. Man. School Econ. Soc. Stud., Sept. 1965, pp. 263-84.

FRIEND, I. AND VICKERS, D. Portfolio selection and investment performance. Jour. Fin., Sept. 1965, pp. 391-415.

LIPFERT, H. Theorems on optimum capitalization. German Econ. Rev., 1965, *3*(3), pp. 193-204.

SHARPE, W. F. Risk-aversion in the stock market: some empirical evidence. Jour. Fin., Sept. 1965, pp. 416-22.

WING, G. A. Capital budgeting, circa 1915. Jour. Fin., Sept. 1965, pp. 472-79.

Business Organization; Managerial Economics; Marketing; Accounting

ALBACH, H. Der Einfluss von Forschung und Entwicklung auf das Unternehmenswachstum. Liiketaloudellinen Aikakauskirja, 1965 (2), pp. 111-140.

BUNICH, P. Profit and the material stimulation of high production indices. Prob. Econ., May 1965, pp. 8-18.

DANIELSSON, A. On theories of behavior in organizations. Swedish Jour. Econ., Sept. 1965, pp. 173-207.

FABRIZI, C. La diffusione dei supermercati in Italia. Riv. di Pol. Econ., Oct. 1965, pp. 1469-89.

GOULD, J. R. AND PRESTON, L. E. Resale price maintenance and retail outlets. Economica, Aug. 1965, pp. 302-12.

JAHNUKAINEN, I. Yrityspeli opetus- ja tutkimusmenetelmänä. (With English summary—The business game as a method for teaching and research.) Liiketaloudellinen Aikakauskirja, 1965, (3), pp. 294-327.

JEYNES, P. H. The significance of reinvestment rate. Engin. Econ., fall 1965, pp. 1-9.

KELLERER, H. Mittlere Verweildauer und Umschlagshäufigkeit—zwei Kennzahlen für die Erneuerungsintensität von Bestandsmassen. Schweiz. Zeitschr. f. Volkswirtschaft und Stat., Sept. 1965, pp. 301-11.

PERROUX, F. Pour une theorie rajeunie de l'entreprise. Giorn. d. Econ., May-June 1965, pp. 333-48.

VINCENT, L.-A. De la mesure de la productivité aux problèmes généraux de gestion économique. Rev. Econ., Nov. 1965, pp. 879-924.

Industrial Organization; Government and Business; Industry Studies

ADELMAN, M. A. Monopoly and concentration: comparisons in time and space. Riv. Internaz. di Sci. Econ. e Com., Aug. 1965, pp. 725-50.

BRECHLING, F. The relationship between output and employment in British manufacturing industries. Rev. Econ. Stud., July 1965, pp. 187-216.

EDWARDS, C.D. Control of the single firm: its place in antitrust policy. Law and Contemp. Prob., summer 1965, pp. 465-87.

FERGUSON, J. M. Tying arrangements and reciprocity: an economic analysis. Law and Contemp. Prob., summer 1965, pp. 552-80.

FESSEY, M. C. Financial statistics of United Kingdom companies. Annales Sci. Econ. Appliquées, Aug. 1965, pp. 305-34.

FONTANELLA, G. Il trasporto terrestre nell'U.R.S.S. Giorn. d. Econ., July-Aug. 1965, pp. 623-33.

FUCHS, V. R. The growing importance of the service industries. Jour. Bus. Univ. Chicago, Oct. 1965, pp. 344-73.

GRAY, H. M. Technological determinism as a defense for monopoly. Quart. Rev. Econ. & Bus., fall 1965, pp. 23-31.

HOPE, E. En analyse av konsentrasjonen innen norsk bankvesen. Stat. Tids., June 1965, pp. 47-98.

KÜNG, E. Wettbewerb bei Überbeschäftigung und technischer Revolution. Schweiz. Zeitschr. f. Volkswirtschaft und Stat., Sept. 1965, pp. 233-47.

LEONT'EV, A. P., MUKASIAN, S. P., AND KANYGIN, I. M. Balances and proportions. Prob. Econ., May 1965, pp. 3-7.

LOMBARDINI, S. Les oligopoles en Italie. Rev. Econ. Nov. 1965, pp. 849-78.

LUNGHINI, G. La struttura del sistema economico italiano nel 1959. L'industria, April-June 1965, pp. 237-58.

MARKHAM, J. W. The new antitrust policy and the individual business firm. Law and Contemp. Prob., summer 1965, pp. 607-20.

McGEE, J. S. Some economic issues in Robinson-Patman land. Law and Contemp. Prob., summer, 1965, pp. 530-51.

METWALLY, M. M. A comparison between representative size of plant in manufacturing industries in industrialised and less-industrialised countries. Yorkshire Bull. Econ. Soc. Research, Nov. 1965, pp. 139-55.

PETTENATI, P. Oligopolio e accumulazione. Studi Econ., May-Aug. 1965, pp. 211-36.

PRESTON, L. E. Restrictive distribution arrangements: economic analysis and public policy standards. Law and Contemp. Prob., summer 1965, pp. 506-29.

RENZETTI, G. Indirizzi per una riforma dell'Azienda Ferroviaria Italiana. Giorn. d. Econ., July-Aug. 1965, pp. 519-49.

SCHERER, F. M. Firm size, market structure, opportunity, and the output of patented inventions. Am. Econ. Rev., Dec. 1965, pp. 1097-1125.

STOCKER, P. Wettbewerbspolitische Erfahrungen europäischer Staaten. Schweiz. Zeitschr. f. Volkswirtschaft und Stat., Sept. 1965, pp. 263-300.

TELSER, L. G. Abusive trade practices: an economic analysis. Law and Contemp. Prob., summer 1965, pp. 488-505.

TOWNSEND, H. Exclusive dealing in petrol: some comments. Economica, Nov. 1965, pp. 410-23.

WILLIAMSON, O. E. A dynamic theory of interfirm behavior. Quart. Jour. Econ., Nov. 1965, pp. 579-607.

Technical progress and its social consequences in the French textile industry. Internat. Lab. Rev., July 1965, pp. 51-62.

Land Economics; Agricultural Economics; Economic Geography; Housing

ASSANTE, F. Calopezzati. Propietà fondiaria e classi rurali in un comune della Calabria (1740-1886). Annali, 1965, 4, pp. 61-197.

AYDALOT, P. Etude sur les processus de polarisation et sur les réactions des industries anciennes à la lumière de l'expérience de Lacq. Cahiers de L'I.S.E.A., March 1965, pp. 111-64.

BANDIDI, M. Co-operatives and agrarian reform. Internat. Lab. Rev., August 1965, pp. 115-27.

BERTRAND, A. L. The impact of changes in agriculture on political life in the North American region. Cahiers de L'I.S.E.A., April 1965, pp. 119-31.

COURTHÉOUX, J.-P. Groupes de pression face à l'innovation économique: la canalisation de la Moselle. Rev. Econ., Nov. 1965, pp. 925-43.

DOUMONT, J. Le débat agricole des Six; ses conséquences économiques, financières, politiques. Cahiers Econ. de Bruxelles, 1965, *27*(3), pp. 419-36.

FLORES, E. The economics of land reform. Internat. Lab. Rev., July 1965, pp. 21-34.

HOFFMAN, E. S. AND HUME, J. R. Farm and non-farm income in Australia. Quart. Rev. Agric. Econ., July 1965, pp. 121-35.

KARCZ, J. F. The new Soviet agricultural programme. Sov. Stud., Oct. 1965, pp. 129-61.

KLEINHANS, J. Gli elementi di costo del credito agrario. Risparmio, June 1965, pp. 1114-32.

KLEMM, H. Eine Aufgliederung des Beitrags der Landwirtschaft zum Bruttoinlandsprodukt nach Quartalen. (With English summary.) Ifo-Stud., 1964, *10*(1/2), pp. 33-60.

KRUSE-RODENACKER, A. Die Landwirtschaft als binnen- und aussenwirtschaftliches Problem der Industriestaaten. Schmollers Jahrb., 1965, *85*(4), pp. 419-48.

RUTTAN, V. W. Growth stage theories and agricultural development policy. Australian Jour. Agric. Econ., June 1965, pp. 17-32.

SALLEZ, A. AND SCHLEGEL, J. Les commandes gouvernementales de sous-traitance et le développement régional. Cahiers de L'I.S.E.A., March 1965, pp. 165-93.

SELLA, L. Struttura, economia e finanziamento della agricoltura nel Kenya. Risparmio, Aug. 1965, pp. 1522-33.

TAGLIACARNE, G. I baricentri demografici ed economici del Mec e la posizione periferica dell'Italia. Riv. di Pol. Econ., Aug-Sept. 1965, pp. 1191-1209.

UMEMURA, M. Agriculture and labour supply in Japan in the Meiji era. Develop. Econ., Sept. 1965, pp. 269-85.

VÖCHTING, F. Gesellschaft und Wirtschaft einer süditalienischen Kleinstadt. Jahrb. f. Nationalök. und Stat., June 1965, pp. 494-524.

Labor Economics

ÅBERG, Y. The relationship between wage and employment changes in individual firms. Swedish Jour. Econ., June 1965, pp. 101-24.

BLAKEY, K. A. The scope for manpower analysis in planning production in certain African countries. Internat. Lab. Rev., Nov. 1965, pp. 380-96.

BLAŠKOVIČ, O. Some problems of education for choice of occupation in Czechoslovakia. Internat. Lab. Rev., August 1965, pp. 106-14.

BLOCH, H. D. Discrimination against the Negro in employment in New York, 1920-1963. Am. Jour. Econ. Soc., Oct. 1965, pp. 361-81.

BRIGGS, V. M., JR. The mutual aid pact of the airline industry. Indus. Lab. Rel. Rev., Oct. 1965, pp. 3-20.

CASH, W. C. A critique of manpower planning and educational change in Africa. Econ. Develop. and Cult. Change, Oct. 1965, pp. 33-47.

COOK, A. H. The international labor organization and Japanese politics. Indus. Lab. Rel. Rev., Oct. 1965, pp. 41-57.

DOROSHIN, I. Labor estimates and perfection of the proportions of social reproduction. Prob. Econ., May 1965, pp. 19-26.

DOUTY, H. M. "Fair comparison": the case of the United States white-collar civil service. Economica, Nov. 1965, pp. 375-92.

FLANAGAN, R. J. Disguised unemployment and the structural hypothesis. Indus. Rel., Oct. 1965, pp. 23-36.

FORSTER, C. Australian unemployment, 1900-1940. Econ. Record, Sept. 1965, pp. 426-50.

GILMAN, H. J. Economic discrimination and unemployment. Am. Econ. Rev., Dec. 1965, pp. 1077-96.

KAUN, D. E. The fair labour standards act: an evaluation. So. Afr. Jour. Econ., June 1965, pp. 131-45.

———. Wage adjustments in the Appalachian states. So. Econ. Jour., Oct. 1965, pp. 127-36.

PFISTER, B. Die Problematik des Faktors Arbeit in Entwicklungsländern und Entwicklungspolitik. Jahrb. f. Nationalök. und Stat., Aug. 1965, pp. 169-85.

RAIMON, R. L. Changes in productivity and the skill-mix. Internat. Lab. Rev., Oct. 1965, pp. 314-24.

REES, A. An incomes policy for the United States? Jour. Bus. Univ. Chicago, Oct. 1965, pp. 374-78.

SELIGMAN, B. B. On work, alienation, and leisure. Am. Jour. Econ. Soc., Oct. 1965, pp. 337-59.

SIEBEL, W. Die Umschichtung der Arbeits- und Verwaltungsleistungen unter den Bedingungen der Automation. Schmollers Jahrb. 1965, *85*(3), pp. 287-301.

SLOANE, A. A. Collective bargaining in trucking: prelude to a national contract. Indus. Lab. Rel. Rev., Oct. 1965, pp. 21-40.

SOKOLOWSKA, M. Some reflections on the different attitudes of men and women towards work. Internat. Lab. Rev., July 1965, pp. 35-50.

STEENKAMP, W. F. J. The problem of wage regulation. So. Afr. Jour. Econ., June 1965, pp. 87-100.

YAMAMURA, K. Wage structure and economic growth in postwar Japan. Indus. Lab. Rel. Rev., Oct. 1965, pp. 58-69.

Manpower shortages and active manpower policies in Europe in 1964. Internat. Lab. Rev., July 1965, pp. 1-20.

Population; Welfare Programs; Consumer Economics

ADAMS, F. G. AND GREEN, E. W. Explaining and predicting aggregative consumer attitudes. Internat. Econ. Rev., Sept. 1965, pp. 275-93.

BLAUG, M. The rate of return on investment in education in Great Britain. Man. School Econ. Soc. Stud., Sept. 1965, pp. 205-62.

BRADFIELD, S. Some occupational aspects of migration. Econ. Develop. and Cult. Change, Oct. 1965, pp. 61-70.

CHUBACHI, M. Structure and function of the worker's family living. Keio Econ. Stud., 1964, *2*, pp. 1-16.

DAVIS, K. The population impact on children in the world's agrarian countries. Pop. Rev., Jan. and July 1965, pp. 2-31.

DENIEL, R. AND HENRY, L. La population d'un village du Nord de la France, Sainghin-en-Mélantois, de 1665 à 1851. Population, July-Aug. 1965, pp. 563-602.

DRACHOUSSOFF, V. Economie rurale et problèmes humains du développement. Cahiers de L'I.S.E.A., April 1965, pp. 53-72.

FELDSTEIN, M. S. Hospital bed scarcity: an analysis of the effects of inter-regional differences. Economica, Nov. 1965, pp. 393-409.

GIL, B. AND OMABOE, E. N. Population censuses and national sample surveys in developing countries. Internat. Lab. Rev., Sept. 1965, pp. 169-83.

HURAULT, J. La population des Indiens de Guyane française (deuxième article). Population, Sept.-Oct. 1965, pp. 801-28.

MALASSIS, L. Education et développement rural. Cahiers de L'I.S.E.A., April 1965, pp. 27-51.

MORTARA, G. Incremento demografico e progresso economico nel pensiero e nell'azione dei governi. L'industria, April-June 1965, pp. 207-15.

———. Limitazione delle nascite e progresso economico. L'industria, July-Sept. 1965, pp. 372-90.

STONE, L. O. Population redistribution and economic development in Puerto Rico, 1950-1960. Soc. and Econ. Stud. (Jamaica), Sept. 1965, pp. 264-71.

SUTTER, J. Etude à l'échelle démographique de la fréquence et des effets d'une mutation: l'atteinte des incisives latérales supérieures. Population, Sept.-Oct. 1965, pp. 785-800.

SUTTER, J. AND GOUX, J. M. Les équivalents létaux et la mesure démographique de la mortalité. Population, Sept.-Oct. 1965, pp. 829-50.

VIMONT, C. AND BAUDOT, J. Les titulaires d'un diplôme d'enseignement technique ou professionnel dans la population active en 1962. Population, Sept-Oct. 1965, pp. 763-84.

Related Disciplines

CÉPÈDE, M. Aspects sociologiques des changements institutionnels dans les sociétés rurales en développement. Cahiers de L'I.S.E.A., April 1965, pp. 7-26.

FRICKE, R. Die geistigen Voraussetzungen der Industrialisierung im 19. und 20. Jahrhundert. Schmollers Jahrb. 1965, *85*(3), pp. 257-72.

GALANTINO, F. Studio statistico delle curve di diffusione dell'Alb. I[131] in soggetti normali, aterosclerotici e diabetici degenti nella clinica medica dell'Università di Palermo. Annali Fac. di Econ. e Com., 1965, *19*(1), pp. 171-239.

NOTES

Important Notice to Members

The American Economic Association is again asking members of the economics profession to assist in the collection of data for the National Register of Scientific and Technical Personnel, 1966. We were most happy with the cooperation we received from our members in 1964. The information you provided enabled us to begin a detailed analysis of the economics profession ("The Structure of Economists' Employment and Salaries, 1964", *American Economic Review*, Supplement, December 1965). We hope that you will cooperate with us again this year by completing and returning at your earliest convenience the questionnaire you receive this month.

Harold F. Williamson
Secretary

New NDEA Program for Institutes

The Higher Education Act of 1965 has added economics as one of the 12 disciplines now eligible for support for institutes for advanced study and teaching under Title 11 of the National Defense Education Act. The Office of Education, following the recommendation of a committee of economists and educators called to advise it, has invited proposals from colleges and universities for institutes in the teaching of economics for the summer of 1967 or for the academic year 1966-67.

There is considerable flexibility as to the exact type of institute, but it must be aimed at helping to develop the economic understanding and teaching ability for economics of teachers in the secondary or primary schools; institutes for college teachers are not eligible for consideration at this time. If suitable applications are received, the Office of Education will consider supporting a substantial number of such workshops for the period indicated above. The legislation provides full financial support for the workshops, including stipends for the high school or primary school teachers participating.

Complete information on the program, including steps to be taken in submitting proposals, can be obtained by writing to Dr. Donald Bigelow, Officer in Charge, Division of Educational Personnel Training, Bureau of Elementary and Secondary Education, U.S. Department of Education, Washington, D.C.

This new program is in addition to the program of the National Science Foundation, with somewhat similar aims, which has been in operation for the past two years.

Announcements

The AEA and the Institute of International Education announce the ninth session of the Economics Institute, June 30 to August 31, 1966, at the University of Colorado, Boulder, Colorado. The Institute provides a summer training program for selected foreign students who have been admitted to regular graduate study in economics at universities and colleges in the United States.

Application forms and scholarship information can be obtained from the Institute of International Education, 809 United Nations Plaza, New York, N.Y. 10017 or from the Director of The Economics Institute, Professor Wyn F. Owen, University of Colorado, Boulder, Colorado 80304.

The Survey Research Center of the University of Michigan will once again hold a Summer Institute in Survey Research Techniques. The 1966 Institute will be presented in two four-week sessions, the first from June 27 to July 21 and the second from July 25 to August 18. These two sessions may be taken independently or successively. For further information please write to the Survey Research Center, University of Michigan, P.O. Box 1248, Ann Arbor, Michigan 48106.

The Graduate School of Business and Public Administration of Cornell University has announced a program in international development studies designed for persons interested in administrative careers and advisory services abroad. Under the Master's degree program a person may combine basic courses in administration and public affairs with subjects on the political, social, and economic aspects of development offered throughout the University and in the School. The doctoral candidate may also elect administration and development as a field. Students may participate in any of the four area programs and may include language instruction in any of the 25 languages offered at Cornell. For further information about the program and financial assistance, the applicant should write to Professor Douglas E. Ashford, Malott Hall, Cornell University. Several publications on international studies at Cornell and on the international development program of the School are available on request.

The National Institute of Mental Health provides program grants to departments of economics for pre- and postdoctoral training of students with specific mental health research competencies. Training programs include up to four years of predoctoral training, with stipends, dependency allowances, and tuition support for students; up to two years of postdoctoral training; as well as relevant faculty and other training support. Programs should stress general excellence of disciplinary training and should delimit a clear mental health training research program. Programs are initially underwritten for a maximum of seven years, and, shown merit, may be renewed. Additional information may be obtained from Nathaniel H. Siegel, Social Sciences Section, Training and Manpower Resources Branch, National Institute of Mental Health, Bethesda, Maryland.

The Department of Medical Care Organization of the School of Public Health, University of Michigan, announces the availability of fellowships for a program of study leading to the Ph.D. degree in medical care organization. The program is intended to prepare students for careers in teaching, research, and policy formulation in the sociological, economic, and administrative aspects of medical care organization. Students with a bachelor's or an advanced degree in the social sciences or health fields are invited to apply. The program of study is designed to be completed in three years and to provide competence in the following areas: medical care, relevant aspects of sociology and economics, and research methods.

Fellowship stipends begin at $2400 for the first year with increments for succeeding years. Stipends are supplemented by an additional $500 for each dependent and full tuition.

For application or further information, write to Benjamin J. Darsky, Chairman, Doctoral Program in Medical Care Organization, School of Public Health, University of Michigan, Ann Arbor, Michigan 48104.

Deaths

Arthur Z. Arnold, professor of economics, New York University, December 1965.

Andrew Jackson Dadisman, professor emeritus of economics, College of Commerce, West Virginia University, November 8, 1965.

James R. Elliott, Jr., Northern Illinois University, De Kalb, December 1, 1965.

John Thom Holdsworth, professor emeritus and dean emeritus, University of Miami. Florida, October 22, 1965.

Murray G. Lee, American Bankers Association, New York, July 3, 1965.

Harold G. Moulton, President Emeritus, The Brookings Institution, Washington, December 14, 1965.

Weldon Powell, Haskins and Sells, New York, October 23, 1965.

David S. Roswell, New Rochelle, New York.

Rudolph L. Treuenfels, Brooklyn, New York, October 22, 1965.

Retirements

Theodore N. Beckman, professor, department of business organization, Ohio State University, June 1965.

Ralph C. Davis, professor, department of business organization, Ohio State University, September 1965.

Visiting Foreign Scholars

Case A. Cannegieter, University of Western Australia: visiting professor, department of economics, University of Texas, spring 1966.

Will Grader, Nederlands Opleidings Instituut voor het Buitenland: visiting professor, department of finance and business environment, School of Business Administration, University of Oregon.

H. P. Hain, University of Melbourne: visiting professor of accounting, University of Alabama, fall 1965.

Donald M. Lamberton, University of New South Wales: visiting associate professor, business administration and economics, Graduate School of Business, University of Pittsburgh, winter 1966.

Shirley W. Lerner, University of Manchester: visiting professor, School of Labor and Industrial Relations, Michigan State University, winter quarter 1966.

B. H. Schatzmann, University of Neuchatel, Switzerland: visiting associate professor of international marketing, University of Alabama, fall 1965.

C. N. Vakil, University of Bombay and Reserve Bank of India: visiting professor of international business, Graduate School of Business, Indiana University, first semester 1965-66.

Promotions

Irving Abramowitz: professor, department of business organization, Ohio State University.

David L. Bickelhaupt: professor, department of business organization, Ohio State University.

Ernest Bloch: professor, department of banking and finance, School of Commerce, New York University.

Julian H. Bradsher: professor, department of economics, Oklahoma State University.

Lawrence A. Danton: associate professor of economics, Municipal University of Omaha.

Joseph S. DeSalvo: assistant professor of economics, Virginia Military Institute.

Ronald S. Foster: associate professor, department of business organization, Ohio State University.

Harry M. Johnson: associate professor of insurance, finance department, University of Connecticut.

Herbert Kiesling: assistant professor, department of economics, Indiana University.

Benjamin Klebaner: professor, department of economics, City College of New York.

Joseph J. Klos: professor, department of economics, Oklahoma State University.

Vincent De P. Mathews: assistant professor, department of economics, Catholic University of America.

Jean Namias: professor of economics, St. John's University.

David M. Nowlan: assistant professor, department of political economy, University of Toronto.

Roy J. Sampson: professor, School of Business Administration, University of Oregon.

Ned Shilling: associate professor of business statistics, Pennsylvania State University.

Rafael A. Sison: principal economic analyst, Baltimore City Department of Planning, Research and Analysis Section.

E. J. Steele: professor, department of economics, Municipal University of Omaha.

Donald L. Thompson: associate professor, School of Business Administration, University of Oregon.

Elliot Zupnick: professor, department of economics, City College of New York.

Administrative Appointments

Lewis C. Bell, University of Mississippi: director, Tax Research Center, and coordinator of research in business and government, Western Kentucky State College.

Henry W. Briefs: chairman, department of economics, Georgetown University.

Richard R. Carroll: assistant professor and chairman, department of economics, King College, Tennessee.

Edwin B. Cox: assistant dean, College of Business Administration, Boston University.

Rollin O. Dunsdon: chairman, department of economics and business administration, University of the Pacific.

Dascomb R. Forbush: chairman, department of economics, Clarkson College of Technology.

Donald F. Forster: executive assistant to the president, University of Toronto.

C. Edward Galbreath: acting dean, College of General Studies, George Washington University.

Abraham L. Gitlow: acting dean, School of Commerce, New York University.

Rudolph W. Hardy: executive director, New England Economic Research Foundation.

Richard B. Harshbarger: head, department of economics and business, Manchester College.

Charles J. Hitch: vice president, business and finance, University of California, Berkeley.

James H. Hund: dean, School of Business Administration, Emory University.

William N. Kinnard, Jr.: director, Center for Real Estate and Urban Economic Studies, University of Connecticut.

Benjamin Klebaner: assistant dean, College of Liberal Arts and Science, City College of New York.

Richard H. Leftwich: professor and head, department of economics, Oklahoma State University.

Robert Lekachman: chairman, department of economics, State University of New York, Stony Brook.

Richard A. Lester: director, Woodrow Wilson School of Public and International Affairs Graduate Program, Princeton University.

Wesley H. Long: acting director, Center for Economic Research, department of economics, Bowdoin College.

Hugh I. Macdonald, University of Toronto: chief economist, Department of Economics and Development of the Government of Ontario.

Ronald R. Olsen: chairman, department of economics, University of Kansas.

Murray E. Polakoff: chairman, department of economics, School of Commerce, New York University.

Kenneth D. Ramsing: assistant dean, division of undergraduate studies in business administration, University of Oregon.

Edward K. Smith, Boston College: Deputy Assistant Secretary for Economic Policy, U.S. Department of Commerce, Washington.

E. J. Steele: chairman, department of economics, Municipal University of Omaha.

Donald L. Thompson: assistant dean, division of graduate studies in business administration, University of Oregon.

Stan West: director, department of research and statistics, New York Stock Exchange.

Jerome H. Zoffer: assistant dean, academic affairs, associate dean for administration, Graduate School of Business, University of Pittsburgh.

Erratum

The appointment of David LeSourd was listed incorrectly in the December 1965 number

of the *Review*. Professor LeSourd was appointed chairman, department of commerce and economics, University of Vermont.

Appointments

John Q. Adams, III: assistant professor, department of economics, University of Maryland.

H. K. Allen, University of Illinois: visiting professor, department of economics, Southern Illinois University, academic year 1965-66.

Peter L. Arcus: research associate, department of economics, Iowa State University.

Fred D. Arditti, Massachusetts Institute of Technology: staff member, economics department, RAND Corporation.

Roland Artle, University of California, Berkeley: August Röhss Professor of Economics, University of Gothenburg, Sweden.

Francis M. Bator: Deputy Special Assistant to the President for National Security Affairs.

Roger D. Blackwell, Northwestern University: assistant professor, department of business organization, Ohio State University.

Karl Brunner, University of California, Los Angeles: Everett D. Reese Professor of Economics and Banking, department of economics, Ohio State University, fall 1966.

Thomas Burke, Boston College: instructor in economics, Georgetown University.

Kenneth T. Cann: resident lecturer, department of economics, Indiana University, Southeastern Campus.

John M. Cassels: professor, department of economics, University of Colorado, fall 1966.

Perry Chang, Northern Illinois University: associate professor of economics, Municipal University of Omaha.

Ewan Clague, U.S. Department of Labor: distinguished visiting professor, School of Labor and Industrial Relations, Michigan State University, spring quarter 1966.

Darwin B. Close, University of Pennsylvania: assistant professor, department of business organization, Ohio State University.

David W. Cole, Indiana University: assistant professor, department of business organization, Ohio State University.

Donald R. Connell, Iowa State University: assistant professor of economics, Municipal University of Omaha.

Raymond Coward, Louisiana State University: lecturer in business law, University of Alabama.

Edwin B. Cox: professor of statistics, College of Business Administration, Boston University.

Nelson H. Cruikshank, Social Security Department, AFL-CIO: visiting professor, School of Labor and Industrial Relations, Michigan State University, spring quarter 1966.

Douglas C. Dacey, Institute for Defense Analysis, Washington: associate professor, department of economics, University of Texas, spring 1966.

Jim E. Davis, Long Beach State College: program economist, U.S. AID Mission, Morocco.

Francis R. Eels: associate research professor of economics, Institute of Business, Economics and Government Research, University of Alaska.

Betty G. Fishman: lecturer in economics and research associate, Regional Research Institute and College of Commerce, West Virginia University.

Roger N. Folsom: instructor, department of economics, U.S. Naval Postgraduate School.

Robert J. Freeman, University of Arkansas: lecturer in accounting, University of Alabama.

Philip Friedly: assistant professor of planning, division of community and regional planning, Faculty of Graduate Studies, University of British Columbia.

Richard C. Gerhan, Federal Reserve Bank of Cleveland: associate professor, department of economics, Baldwin-Wallace College.

Kenneth Gordon, University of Chicago: instructor in economics, Georgetown University.

George R. Green: assistant professor, department of economics, University of Maryland.

Kanji Haitani, Ohio State University: assistant professor of economics, Southern Illinois University.

Harry H. Hall: research associate, department of economics, Iowa State University.

Stanley T. Hardy, Michigan State University: assistant professor, department of business organization, Ohio State University.

George H. Hempel, Washington University: research fellow, National Bureau of Economic Research.

Mona S. Hersh: assistant professor of economics, Texas Woman's University.

Daniel Howland: associate professor, department of business organization, Ohio State University.

Ichirou Inukai: instructor, department of economics, University of Alaska.

Wells H. Keddie: research director and assistant professor of labor studies, department of labor education, Pennyslvania State University.

Stuart E. Kirby: professor of economics, College of Advanced Technology, University of Aston, Birmingham, England.

David T. Kollat, Indiana University: assistant professor, department of business organization, Ohio State University.

Helen C. Kramer: instructor, department of economics, Brooklyn College.

L. Emil Kreider, Ohio State University: instructor in economics, Southern Illinois University.

Joseph S. La Cascia, University of Florida: assistant professor of economics, Municipal University of Omaha.

Neal B. Long: assistant professor, department of economics, Indiana University, South Bend.

Duncan McDougall, Carleton University, Ottawa: professor, department of economics, University of Kansas, fall 1966.

David L. McKee: resident lecturer, Indiana University, Fort Wayne.

Hillis S. McKenzie: associate professor, department of economics, University of Alaska.

Peter F. M. McLoughlin, World Bank (I.B.R.D.): associate professor of economics, department of economics, University of Santa Clara.

Thomas D. Moore, Tennessee Polytechnic Institute: assistant professor of economics, University of Alabama.

C. J. Nicholas: industry economist, U.S. Department of Agriculture, Hyattsville, Maryland.

Robert E. Osborn, Jr., University of Connecticut: College of Business, Western Kentucky University.

William H. Parks, Michigan State University: instructor, School of Business Administration, University of Oregon.

James W. Petersen, University of Vermont: associate head, logistics department, RAND Corporation.

J. Marion Posey, University of Arkansas: assistant professor of accounting, University of Alabama.

Reed M. Powell, University of California, Los Angeles: professor, department of business organization, Ohio State University.

Frederic L. Pryor: assistant professor, department of economics, Swarthmore College.

James P. Quirk, Purdue University: professor, department of economics, University of Kansas, fall 1966.

Wilbur A. Rapp, Northwestern University: assistant professor, department of business organization, Ohio State University.

H. Dieter Renning, Illinois State University: assistant professor of economics, Kenyon College.

Hugh Rose, University of Toronto: department of economics, University of Rochester.

John B. Ross, Federal Reserve Bank of Dallas: research economist, Bank of America National Trust and Savings Association, San Francisco.

Richard W. Ruppert, University of California, Santa Barbara: assistant professor, department of economics, University of Kansas, fall 1966.

Rubin Saposnik, State University of New York, Buffalo: professor, department of economics, University of Kansas, fall 1966.

T. Paul Schultz, Massachusetts Institute of Technology: staff member, economics department, RAND Corporation.

Leslie L. D. Shaffer, University of Arizona: associate professor, management, School of Business Administration, University of Oregon.

Kenneth L. Shellhammer: instructor in economics and research associate, Regional Research Institute and College of Commerce, West Virginia University.

John W. Snow: assistant professor, department of economics, University of Maryland.

Kenneth T. Strand, OECD: associate professor, department of economics and commerce, Simon Fraser University, December 1966.

Harold K. Strom, University of California, Los Angeles: associate professor of transportation, School of Business Administration, University of Oregon.

Paul Swadener, Indiana University: assistant professor of insurance, School of Business Administration, University of Oregon.

Sydney Turoff, State University of New York, Buffalo: assistant professor of finance, University of Connecticut.

Arlon R. Tussing: assistant professor, department of economics, University of Alaska.

Balder von Hohenbalken: visiting associate professor of economics, department of economics, Iowa State University.

Robert J. Wolfson, RAND Corporation: principal scientist, System Development Corporation, Santa Monica.

G. Donald Wood, University of Pennsylvania: instructor in economics, Georgetown University.

Kenneth L. Wyman: assistant professor, department of political economy, University of Toronto.

Leaves for Special Appointments

Clark C. Bloom, Ford Foundation, Beirut: visiting professor, department of economics, Southern Illinois University, fall term 1965-66.

Lon C. Cesal, Iowa State University: Iowa-Peru project on agrarian reform and economic development under contract with AID, 1966-68.

Frank S. Conklin: Iowa State University: National School of Agriculture, Chapingo, Mexico and University of Nuevo Leon, Monterrey, Mexico, November 1965 for three years.

H. A. J. Green, University of Toronto: Simon Visiting Professor, University of Manchester, England, 1965-66.

Arnold C. Harberger, University of Chicago: International Bank for Reconstruction and Development, autumn 1965.

Douglas G. Hartle, University of Toronto: Royal Commission on Canada's Taxation.

James L. Heskett, Ohio State University: visiting associate professor, Harvard Graduate School of Business Administration.

Benjamin Higgins, University of Texas: senior specialist, East-West Center, University of Hawaii, spring and summer 1966.

Randall A. Hoffman: visiting associate professor, Iowa State University and University of Nuevo Leon, Monterrey, Mexico, October 1965 for three years.

Francis Kutish, Iowa State University: U.S. Department of Agriculture, Washington, November 1965-July 1966.

George W. Ladd, Iowa State University: visiting professor, University of California, Davis, spring 1966.

Ta-Chung Liu, Cornell University: visiting professor of economics, Brandeis University, 1966-67.

Stephen E. McGaughey, Iowa State University: Iowa-Peru project on agrarian reform and economic development under contract with AID, 1966-68.

Philip R. Phillips, New York University: University of Lagos, Nigeria, 1965-66.

Lee E. Preston, University of California, Berkeley: visiting professor, Chinese University of Hong Kong, spring 1966.

Geoffrey S. Shepherd, Iowa State University: Iowa-Peru project on agrarian reform and economic development under contract with AID, September 1965 for two years.

Larry Sjaatad, University of Chicago: University of Minnesota project, AID, University of Los Andes, Bogota, Colombia, winter quarter 1966.

Richard H. Slavins, West Virginia University: Deputy Commissioner of Commerce, Department of Commerce, State of West Virginia.

Wesley J. Yordon, University of Colorado: visiting lecturer in economics, Universidad Autonoma de Guadalajara, Mexico, academic year 1965-66.

Resignations

John Browning, Rutgers—The State University.

James F. Gilsenan, School of Commerce, New York University.

Richard L. Lesher, Ohio State University.

William E. Schlender, Ohio State University.

Barnard Seligman, School of Commerce, New York University.

Atsushi Suzuki, Indiana University.

William L. Velman, Ohio State University.

Miscellaneous

John H. Prime has relinquished deanship and has been named distinguished professor of finance, School of Commerce, New York University to begin after sabbatical leave, 1965-66.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois 60201.

Vacancies

Medical economist: This is a research position to study economic aspects of hospital and related services in relation to the overall medical and general economy of the state. Requires total of 5 years of graduate training and research experience in economics, including at least 2 years of training or experience in medical or hospital economics. Salary is $10,898 to $12,110, plus a wide range of progressive fringe benefits. Send résumé to: Robert P. Mayer, Director of Personnel, N.Y.S. Health Department, 84 Holland Avenue, Albany, New York. 12208

Regional economist: Responsibilities include identification of potential regional research programs, formulation of research proposals for potential sponsors, direction of urban and regional research including those doing desk and field research. The Battelle-Columbus Economics Research Department needs a staff member who has the ability to assess assets and liabilities of local area economics—someone who has working knowledge of literature related to economics and regional analysis. We require an M.S. in economics, economic geography, regional science or a related field. In addition, two years of applicable experience in these fields is required. Salary is commensurate with education and experience. Forward résumé to: L. G. Hill, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio. 43201

Manpower economist: The Battelle Memorial Institute currently has a requirement for a Ph.D. in economic theory, labor economics, or regional economics to supervise projects dealing with projected labor force requirements in areas and nations (some international work involved). He will participate as research staff member of regional economic studies with contributions focusing on the demographic characteristics of regional populations including employment and education. He will also participate in projects dealing with the formulation of educational and training programs at local, state, and national levels. The ability to communicate with research team members drawn from the disciplines of sociology, political science, educational administration, and various process technologies is essential. Added responsibilities include formulation of research proposals and study outline implementation of research design and preparation of oral and written reports. We require experience in regional economic studies, analysis of labor force structure, or general economic forecasting. Salary is commensurate with education and experience. Please forward résumé to: L. G. Hill, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio. 43201

Quantitative economics, business economics: Openings exist at junior and senior levels in statistics and/or operations research and in business economics. Write to: Chairman, Department of Business Economics, College of Business Administration, University of Hawaii, Honolulu, Hawaii. 96822

EDWARD HASTINGS CHAMBERLIN

Distinguished Fellow

1965

It is not given to many scientists to reach into the minds of all their fellows and to influence the work of a whole generation, but the author of *The Theory of Monopolistic Competition* did so. The nature of products, the character of intercommodity competition, the accommodation of selling costs in price theory, and the theory of oligopoly have all undergone major change because of Chamberlin's great work.



E.H. Chamberlin

The American Economic Review

Volume LVI | JUNE 1966 | Number 3

SOME ESTIMATES OF THE COST OF CAPITAL TO THE ELECTRIC UTILITY INDUSTRY, 1954–57

By MERTON H. MILLER AND FRANCO MODIGLIANI*

In its simplest form, the central normative proposition of the micro theory of capital is that the firm should adjust its capital stock until the marginal rate of return on further investment (or disinvestment) is equal to the cost of capital. Under conditions of perfect certainty—which is the assumption on which most of classical theory has been developed—the concept of the cost of capital presents no particular difficulty. It is simply the market rate of interest. Since all securities must have the same yield in equilibrium under certainty there is only one such rate per period, and it is, in principle, a directly observable magnitude. Under real-world conditions, however, we are confronted not with one, but with a bewildering variety of securities, with very different kinds and priorities of claims to portions of the (uncertain) future earnings of the firm. Since these securities will also, in general, have different anticipated yields, it is by no means clear which yield or combination of yields is the relevant cost of capital for rational investment planning. Nor, because it is based on anticipations, is the cost of capital any longer a directly observable magnitude. It must, rather, somehow be inferred from what *is* observable, namely, the market prices of the various kinds of claims represented by the different securities.

Although most (but not all) recent studies of investment behavior have shown some awareness of these difficulties, a common approach in empirical work has been simply to ignore the problem and to use

* [The authors are, respectively, professor of finance and economics at the Graduate School of Business, University of Chicago and professor of economics and industrial management at Massachusetts Institute of Technology.]

In the course of the several years during which this paper has been in preparation we have been fortunate in having the assistance of many institutions and individuals. In particular, we wish to express our thanks to (1) The Ford Foundation which has subsidized the project in a variety of ways ranging from Ford Faculty Research Fellowships to the authors to research grants to both schools for the project; (2) the Research Committees of the Graduate School of Business of the University of Chicago, Sloan School of Management, Massachusetts Institute of Technology, and the Graduate School of Industrial Administration, Carnegie Institute of Technology for providing us with additional time for research and for bearing much of the (heavy) cost of computer programming and calculation; (3) our student research assistants, including Clark Armstrong, Stephen Furman, Peter Gustafson, Gardner Heydrich, Anthony Johnson, Myron Scholes, Robert Sharp, Theodore Van Wormer, and Robert Williamson; (4) our many colleagues past and present, with whom we have discussed the various problems of substance and methodology in the paper, including William Alberts, Eugene Fama, Zvi Griliches, Arnold Harberger, Dale Jorgenson, Edwin Kuh, Albert Madansky, Henri Theil, H. Martin Weingartner, Arnold Zellner, and especially Jacques Drèze and Lester Telser. To the extent that there are errors remaining, this must be attributed not to their failure to point them out, but only to our failure to listen.

without comment or explicit justification some standard index of current, nominal yields on high-grade corporate bonds (or even government bonds) as a measure of the cost of capital. Other writers use both a current bond yield series to approximate the cost of debt capital and a current profit series to measure the "availability" and hence, presumably, also the "cost" of equity capital. Still others have tried indexes of share prices, current dividend yields, or current earnings yields alone or in various averages with bond yields along lines suggested in the standard texts on corporation finance. How much error is involved in the use of such measures is still unknown, though even a cursory survey of the underlying theory suggests many grounds for apprehension on this score. But we cannot be sure. Too little work has yet been done to permit even a rough calibration of these series as proxies for the cost of capital, let alone to provide acceptable alternative series.

It is the purpose of this paper to take at least some first steps toward closing this gap in our information about the factors affecting investment. We shall attempt here to develop effective methods for inferring the cost of capital relevant for optimal investment decisions from data on the market values of securities. As a concrete application of these techniques—which we hope will prove of interest not only to economists concerned with understanding investment behavior, but also to the many corporate officials we know to be currently engaged in similar investigations—we shall present some actual estimates of the cost of capital for a sample of large electric utilities for the years 1954, 1956, and 1957.

The sample consists of 63 separate firms representing all of the (consolidated) systems classified as of 1950 as Class A by the Federal Power Commission;[1] plus those of the smaller Class B systems whose assets devoted to electricity generation were at least $15 million in 1950. The cutoff at $15 million was, of course, an arbitrary one and represented what we hoped would be a workable compromise between our desire to have as large a sample as possible and yet to avoid having to find valuations for many small companies whose securities are not widely held or actively traded. Consolidated systems rather than individual companies were used to avoid the many problems (both of accounting and of valuation) posed by intercompany holdings of securities. A list of the included companies is appended as Appendix A.

As a testing ground for techniques of estimating the cost of capital the large utilities offer a number of important advantages. They permit us to have both a large sample and one in which the component firms are remarkably homogeneous in terms of product, technology, and

[1] Except for the Hartford Electric Light Company whose accounts could not be adequately reconstructed and made continuous over the entire sample period.

market conditions. In addition, as a by-product of regulation, substantially uniform accounting conventions (down to even such small, but often annoying, details as choice of fiscal year) are followed throughout the industry. In the years studied, moreover, earnings have been highly stable with few of the wide swings in year-to-year reported earnings due to strikes, cyclical or competitive shifts in demand, write-offs of assets, mergers, and the like that often render the published earnings figures virtually meaningless in more sprawling and less sheltered sectors.

As for the sample years, we are embarrassed to have to admit that 1957 was chosen because it was the latest full year available when we began this study. The year 1954, we felt, would be far enough back to show up any significant trend in the cost of capital, but not so far back as to create problems of continuity for the companies in the sample. Since there were some months of business recession in both these years we included the boom year 1956 in the sample in the hope that it might provide some evidence on cyclical swings in the cost of capital if such were sizable or important.

The paper itself will be presented in four main sections. We begin in Section I by providing an operational definition of the cost of capital and developing therefrom the link between cost of capital and market valuation (largely along lines set forth in our previous papers [14] [15] and [17]). Section II describes the econometric model and techniques to be used in estimating the cost of capital. The estimates themselves are presented along with some tests of the specification in Section III. Section IV provides an economic interpretation of the results and a comparison, insofar as that is possible, with other, more conventional estimates.

I. *Valuation and the Cost of Capital*

As used throughout, the term cost of capital, C, will be taken to mean that minimum prospective rate of yield that a proposed investment in real assets must offer to be just worthwhile undertaking from the standpoint of the current owners of the firm. Under conditions of perfect capital markets (and even in some special cases where systematic imperfections are present) there is a one-for-one correspondence between "worthwhileness" in the above sense and the current market value of the owners' interest. If the management of the firm takes as its working criterion for investment (and other) decisions "maximize the market value of the shares held by the current owners of the firm," then it can be shown (see, e.g., Hirshleifer [8]) that this policy is also equivalent to maximizing the economic welfare or utility of the owners. Thus, valuation and the cost of capital are intimately related.

\. *The Simple Certainty Model*

The precise relation between them is most easily seen in the context ıf a simple certainty model in which all real assets are assumed to ield uniform, sure income streams in perpetuity and in which the mar- :et rate of interest, r, is given and constant over time. If, in addition, we ıssume perfect capital markets, rational investor behavior, and no tax lifferentials on different sources of income, then it can readily be shown hat the equilibrium current market value, V, of any firm (i.e., the sum ıf the market values of all securities or other claims to its future earn- ngs) is given by

$$V = \frac{1}{r} X, \tag{1}$$

vhere X is the (uniform) income per period generated in perpetuity by he assets presently held. The term $1/r$ in (1)—the reciprocal of the nterest rate—is commonly referred to as the market "capitalization ate" for sure streams since it represents the factor the market applies o a unit income flow in converting it to a capital stock.

For an expansion of real assets to be worth undertaking from the tandpoint of the current owners of such a firm, the investment must ead to an increase in the market value of their holdings. If we let lA = the purchase cost of the assets acquired, dS^o = the change in value ıf the holdings of the original owners, and dS^n = the market value of the ıdditional securities issued to finance the investment, then differentiat- ng (1) with respect to A yields:

$$\frac{dV}{dA} = \frac{dS^o}{dA} + \frac{dS^n}{dA} = \frac{dS^o}{dA} + 1 = \frac{dX}{dA}\frac{1}{r}. \tag{2}$$

t follows that the cost of capital, C, must be the reciprocal of the mar- :et capitalization rate for earnings since from (2), $dS^o/dA \geq 0$ if, and ınly if, $dX/dA \geq r$, i.e., if, and only if, the rate of return on the new nvestment is equal to or greater than the market rate of interest.[2]

}. *Extension to the Case of Uncertainty*

When we turn from a world of certainty to one of uncertainty, the ıroblem of defining the cost of capital in operational terms becomes a nuch more formidable one for which no completely general solution is 'et available. We have at least been able to show, however (see [15]),

[2] We have here stated (and shall continue to state) the conditions for optimality of invest- ıent decisions in terms of the rate of return or internal yield on investment. Although it is ell known that there may be cases in which such a rate of return cannot be adequately or nambiguously defined (see, e.g., Hirshleifer [8]), such cases are largely ruled out by our dditional simplifying assumptions.

that if we retain the assumptions of perpetual streams, rational investor behavior, perfect markets, no taxes (and no "growth" in a sense to be more precisely defined later), then an analog of the certainty valuation formula (1) does carry through to the case of uncertainty. In particular, if we restrict attention to what we have called a "risk equivalent class" of firms, then the equilibrium market valuation of any firm in such a class can be expressed as

$$V = \frac{1}{\rho_k} \overline{X} \quad \text{for all firms in class } k \tag{3}$$

where V is the sum of the market values of all the firm's securities; $\overline{X}$ is the *expected* level of average annual earnings generated by the assets it currently holds; and where $1/\rho_k$ can be interpreted as the market's capitalization rate for the expected value of uncertain, pure equity earnings streams of the type characteristic of class k.[3] Hence, by a straightforward extension of the reasoning in the previous section, the cost of capital for a proposed expansion in scale by any firm in the class is simply ρ_k.[4] The precise value of ρ_k will, of course, be different from

[3] At the level of pure theory a "risk-equivalent class" can be defined in precise terms as a collection of firms such that the elements of the (uncertain) future earnings stream of each firm is proportional to (and hence perfectly correlated with) those of every other member of the class. (See [15].) For empirical purposes, however, the best that can usually be done is to work with reasonably homogeneous industries and hope that the differences among firms are small, random and not strongly correlated with any of the explanatory variables. As noted above, the utility industry probably has the best prospects for meeting these tests and that is one of the reasons why we chose it. It is perhaps also worth noting in this connection that a recent study of comovements among security prices by King [10a] does find strong (but, of course, far from perfect) correlation of price movements within most of the two-digit industries studied including the electric utility industry.

[4] The fact that under uncertainty we have to allow, in principle, for financing by many different types of securities creates no very serious difficulty. To illustrate, let dS^n = the value of any new shares floated and dD = the value of any additional debt floated. Then from (3)

$$\frac{dV}{dA} = \frac{dS^o}{dA} + \frac{dS^n}{dA} + \frac{dD}{dA} = \frac{d\overline{X}}{dA}\frac{1}{\rho_k},$$

and it is easily seen that

$$\frac{dS^o}{dA} \geq 0 \quad \text{requires} \quad \frac{d\overline{X}}{dA} \geq \rho_k$$

regardless of whether the new investment is financed entirely by stock

$$\left(\frac{dS^n}{dA} = 1 \quad \text{and} \quad \frac{dD}{dA} = 0\right)$$

or by bonds

$$\left(\frac{dD}{dA} = 1 \quad \text{and} \quad \frac{dS^n}{dA} = 0\right)$$

or by any combination of the two. Note that for simplicity we here and throughout ignore the possible second-order repercussions of the financing method used on the market value of any already outstanding debt.

class to class, presumably increasing with the market's uncertainty as to the level of future long-run earnings in the class (and reflecting also the nature and extent of the covariation with the returns in other classes). Though the various ρ's themselves would not be directly observable they could, in principle, be inferred from the market valuations, e.g., by regressing the observed V on estimates of $\bar{X}$ over a cross section of firms within a class.[5]

An important implication of (3) is that the market value of a firm depends only on its real earning power and on the market capitalization rate for pure equity streams of its class, and not at all upon the particular mix of security types that characterize its financial structure. This independence of value and financial structure is basically a reflection of the assumption of perfect capital markets—an assumption implying, among other things, that for comparable collateral, the supply curve of borrowed funds for individuals is the same as that for corporations. Hence if corporations making heavy use of borrowed funds should sell, say, at a premium relative to unlevered corporations in the same class, rational investors could always obtain a more efficient portfolio by selling the "overvalued" levered shares, purchasing the "undervalued" unlevered shares, and restoring the previous degree of leverage by borrowing against the shares on personal account. And conversely if levered shares should sell at a discount, in which case the "arbitrage" operation involves selling the unlevered shares, buying the levered shares, and unlevering them by also buying a pro rata share of the firm's debts.[6]

With reference to the cost of capital, the independence of market value and financial policy implies, of course, that the cost of capital relevant for investment decisions is also independent of how the investment is to be financed, even though the particular securities considered may, and in general will, have very different expected yields. This seeming paradox disappears as soon as it is recognized that the

[5] Although ρ_k is a constant over the class, it need not, of course, be regarded as a constant over time. Variations in ρ_k over time will create no serious additional problems with the interpretation of (3) as a perpetuity formula so long as such changes in ρ_k as occur are in the nature of "unanticipated" changes (as would be the case, for example, if the process governing the behavior of $\Delta\rho_k$ over time were essentially a random walk with zero mean). Note also that the assumption that all assets generate perpetual streams is less restrictive than might appear at first sight. The perpetuity valuation *formulas* may still be good approximations at the level of the firm as a whole even where the component assets have finite lives, provided that the firm is close to a steady state in which replacements balance depreciation and provided $\bar{X}$ is interpreted as net earnings after depreciation (but before interest).

[6] A fuller account of the arbitrage mechanism and proof of the independence proposition is given in our [15]. It is perhaps worth noting that the independence proposition can be proved under assumptions much weaker than those necessary to develop equation (3). In particular, neither the perpetuity assumption nor the concept of a risk equivalent class is essential (see, e.g., the discussion in our [14, pp. 429–30] and also Hirshleifer [9]).

independence property also requires that the common shares in levered corporations have higher expected yields than those of less levered corporations in the same class—a differential which can be thought of as compensation for the greater "riskiness" attaching to levered shares. Thus, the apparent gain in terms of the cost of capital coming from the ability of a firm to finance an investment with "cheap" debt capital is offset (and with rational behavior in a perfect capital market exactly offset) by the correspondingly higher cost of equity capital.[7]

The reason for this effect can perhaps be seen somewhat more readily by deriving from (3) an explicit expression for the value of the shares in terms of net profits and financial structure. Making use of the identities $V \equiv S+D+P$ (where $S=$ the market value of the common stock, $D=$ the market value of the firm's debts, and $P=$ the market value of the preferred stock) and $\overline{X} \equiv \bar{\pi}+\overline{R}+\overline{Pdv}$ (where $\bar{\pi}=$ expected net profits to the common shareholders, $\overline{R}=$ expected interest payments and $\overline{Pdv}$ $=$ expected preferred dividends) it is easily seen that (3) implies

$$S = \frac{1}{\rho_k}\bar{\pi} - \left(D - \frac{\overline{R}}{\rho_k}\right) - \left(P - \frac{\overline{Pdv}}{\rho_k}\right). \tag{4}$$

The market value of the debt, D, represents the capitalized value of the expected interest stream $\overline{R}$; and since the interest component of total expected earnings is less "risky" than the stream of earnings itself (at least over the range of leverage that safety-oriented creditors normally permit), it will tend to be capitalized more favorably than the rate $1/\rho_k$. Hence $(D-(\overline{R}/\rho_k))$ and $(P-(\overline{Pdv}/\rho_k))$ are positive and may be thought of as in the nature of risk discounts to be subtracted from capitalized net profits in arriving at the value of the shares.

Note finally that (4) could serve equally well, in principle, with (3) as a cross-sectional valuation equation from which to infer $1/\rho_k$. Clearly, however, (3) is more efficient (since it requires estimating only a single coefficient) and accordingly will be used here as the basis for estimation.

C. *The Effect of Corporate Income Taxes*

When we extend the analysis to allow for the existence of corporate income taxes and the deductibility of interest payments, the most im-

[7] References here and elsewhere in the paper to differences in "risk" in relation to differences in expected yields as among securities are, of course, entirely heuristic and intended solely to provide a rationalization of the theoretical results in a way that accords with ordinary, common sense notions about relative valuations. The results themselves, however, are in no way dependent on any special assumptions with respect to risk or "risk aversion"; and readers who find our rationalization in terms of these categories unsatisfactory are free to substitute their own.

portant change is that market value and financial structure are no longer completely independent. To see what is involved let us again denote by $1/\rho_k$ the capitalization rate in a given class for pure equity streams available to investors (i.e., streams of expected net profits after taxes in unlevered firms); by $\bar{X}$, a firm's expected total earnings, now to be taken as earnings before taxes as well as interest; and let τ be the (constant) marginal and average rate of corporate income taxation. Then the market value of an unlevered firm can be expressed as:

$$V_u = S_u = \frac{\bar{X}(1-\tau)}{\rho_k}, \tag{5}$$

where $\bar{X}(1-\tau)$ is the unlevered firm's earnings after taxes. The value of a levered firm with D of debt or other securities whose payments are tax deductible can be shown to be:

$$V \equiv S + D + P = \frac{\bar{X}(1-\tau)}{\rho_k} + \tau D. \tag{6}$$

Note that in (6), the expression $\bar{X}(1-\tau)$ no longer represents the firm's earnings after taxes or any other standard accounting concept when D is not zero and hence when $\bar{X}$ includes some tax-deductible interest. To avoid confusion, therefore, we shall hereafter refer to $\bar{X}(1-\tau)$ as "tax adjusted" earnings, using the symbol $\bar{X}^\tau$ for earnings after taxes in the ordinary accounting sense (i.e., for the sum of expected net profits after taxes, preferred dividends and interest payments as they actually come onto the market for sale to the various security purchasers).[8]

As to the meaning of (6), it says, in effect, that the government pays a subsidy to firms using certain sources of capital which under current law would include bonds, notes, and other firm contractual obligations of indebtedness but not preferred stocks or common stocks.[9] The addition to the present worth of the firm occasioned by these tax savings is the corporate tax rate times the market value of the debt—the latter

[8] The relation between the various concepts can easily be established by observing that taxes paid will be $\tau(\bar{X}-\bar{R})$ so that $\bar{X}=\bar{X}^\tau+\tau(\bar{X}-\bar{R})$ and hence $\bar{X}(1-\tau)=\bar{X}^\tau-\tau\bar{R}$. In earlier versions of this paper, we made extensive use of valuation formulas based on $\bar{X}^\tau$ rather than $\bar{X}(1-\tau)$ largely because of what we felt was the greater intuitive appeal and familiarity of the former concept. This advantage, however, we now believe is not sufficient to outweigh the loss in the efficiency of estimation relative to the somewhat simpler valuation relations based on the tax-adjusted concept. Furthermore, the concept of tax-adjusted earnings, though not known by that name, is actually a fairly standard concept in the literature on capital budgeting. (See, e.g., [2].)

[9] There is one exception to the rule that dividends on preferred stocks are not tax-deductible, and that, unfortunately, occurs in the electric utility industry. The deduction, however, is only a partial one and applies only to a limited set of issues (those outstanding as of 1942 or issued subsequently in redemption of such issues). Hence, the error involved in ignoring this complication (as we shall) is not likely to be important for present purposes.

being, of course, the present worth, as judged by the market, of the future stream of tax-deductible payments.[10]

With respect to the value of the common shares, the effect of the tax subsidy for interest is to change (4) to

$$S = \frac{1}{\rho_k}\bar{\pi}^\tau - (1-\tau)\left(D - \frac{\bar{R}}{\rho_k}\right) - \left(P - \frac{\overline{Pdv}}{\rho_k}\right), \tag{7}$$

which can be derived from (6) by noting that $\bar{\pi}^\tau = (1-\tau)(\bar{X}-\bar{R}) - \overline{Pdv}$ = net profits to the common stockholders after taxes. That the subtraction from S due to debt financing is now reduced to the fraction $(1-\tau)$ of its former size reflects the fact that the government's claim on the firm's earnings is essentially similar to that of a stockholder (see n. 10). Hence while the government can claim τ per cent of the profits, it must also bear τ per cent of the "risk," including the risk introduced by leverage. In the case of nondeductible preferred stock, no such profit-sharing takes place so that the subtraction due to this form of leverage retains its previous value.

Since the deductibility of interest payments thus makes the value of the firm a function of its financial policy, it must also make the required yield or cost of capital a function of financial policy. To see the precise nature of this dependence, let dS^o, as before, stand for the change in the market value of the shares held by the current owners of the firm; dS^n = the value of any new common shares issued; dP = the value of any new preferred stock issued; and dD = the value of any new tax-deductible debt issued, with $dS^n + dP + dD = dA$. Then from (6) we have:

$$\frac{dV}{dA} = \frac{dS^o}{dA} + \frac{dS^n}{dA} + \frac{dP}{dA} + \frac{dD}{dA} = \frac{dS^o}{dA} + 1 = \frac{d\bar{X}(1-\tau)}{dA}\frac{1}{\rho_k} + \tau\frac{dD}{dA} \tag{8}$$

[10] Since the proof is developed in detail in [7], we shall not reproduce it here. The reader may, however, easily convince himself of the correctness of (6) by means of the following parable. Suppose that we were back in the no-tax world of formulas (3) and (4) and that all stocks were owned by two families—the G or Government family owning precisely τ per cent of the shares in every corporation and the P or Private family owning the remaining $(1-\tau)$ per cent plus all the bonds. (For simplicity, ignore preferred stock.) Then the value of the combined stockholdings of both families in a levered firm is:

$$S_L^P + S_L^G = S_L = V_L - D_L^P = \frac{\bar{X}}{\rho_k} - D_L^P.$$

The value of the purely Private family holdings of stocks and bonds in the firm must then be:

$$V_L^P = S_L^P + D_L^P = (1-\tau)S_L + D_L^P = \frac{\bar{X}(1-\tau)}{\rho_k} - (1-\tau)D_L^P + D_L^P = \frac{\bar{X}(1-\tau)}{\rho_k} + \tau D_L^P.$$

For some additional discussion of this way of looking at the derivation see [18].

from which it follows that the cost of capital or required yield on a tax-adjusted basis is

$$C = \rho_k\left(1 - \tau\frac{dD}{dA}\right) \tag{9}$$

since $dS^o/dA \geq 0$ if, and only if, $d\bar{X}(1-\tau)/dA$ is equal to or greater than the right-hand side of (9).[11]

In connection with (9) the two extreme cases of financing methods are of particular interest. For an investment financed entirely by equity capital—and remember that in this context equity capital includes non-deductible preferred stock—dD/dA will equal zero. Hence the required tax-adjusted yield or "marginal cost of equity capital" is ρ_k. For an investment financed entirely by debt or other sources of capital whose payments are tax-deductible, dD/dA is unity, implying that the "marginal cost of debt capital" is $\rho_k(1-\tau)$.

The term "marginal cost" has been placed in quotes to emphasize that while these extreme cases serve to illuminate the meaning of (9), neither is directly relevant for actual decision-making at the level of the firm. In companies large enough to have ready access to the capital markets, as would certainly be true of those in our sample, investment and financing decisions (including decisions to retire outstanding securities) are made continuously and largely independently over time. Since particular investment projects thus are not, and in general cannot, be linked to particular sources of financing, the relevant cost of capital to the firm must be thought of as essentially an average of the above costs of debt and equity capital with weights determined by the long-run average proportions of each in the firm's program of future financing. If we denote this "target" proportion of debt as L, then the weighted average cost of capital can be expressed as $C = C(L) = \rho(1-\tau)L + (1-L)\rho$, or, more compactly as $C = C(L) = \rho_k(1-\tau L)$, where the notation $C(L)$ will be used when we want to emphasize that the cost of capital is a *function* of the target debt ratio L.[12]

[11] Alternatively, the required yield could be stated in terms of *before-tax* earnings, $\bar{X}$ as $\rho_k(1-\tau(dD/dA))/(1-\tau)$. There would, of course, be no reason to prefer one concept of earnings over the other as long as the perpetuity interpretation of the earnings stream is strictly maintained. The tax-adjusted yield, however, can be shown to be the relevant one for decision-making when the analysis is extended to admit assets with finite lives and, accordingly, will be used here throughout. (Cf. notes 5 and 8 above and also the discussion in our [17, n. 16].)

[12] The determination of the optimal value of L for the firm involves many difficult issues for which no completely well-worked-out theoretical analysis is yet available. Suffice it to say here only that it does not follow that because debt is subsidized we must expect to find all firms in any class with the same degree of leverage and that at the maximum permitted by lenders. The subsidy to debt is at least partially offset by other costs incurred (and not adequately comprehended in simple static models of valuation of the kind here considered) as well as by possible interactions between debt and earnings. The utility industry provides a

Notice, finally, that while the definition of the cost of capital has become a good deal more complex as a result of the introduction of corporate income taxes, the problem of estimation remains essentially the same. It still involves only the estimation of a single capitalization rate—in this case $1/\rho_k$, the capitalization rate for unlevered, pure equity streams in the class. The difference between the cost of equity and debt capital introduces no new difficulties because the cost of debt capital does not depend on the market rate of interest on bonds, but only on the above capitalization rate and the tax rate. Hence $1/\rho_k$ remains a sufficient parameter both for economists seeking to explain rational investment behavior and for firms planning their investment programs on the basis of given financial policies.[13]

D. *Growth and Valuation*[14]

Up to this point, we have been focusing attention entirely on the role of current earning power and financial policy as determinants of the value of the firm. There are, of course, very many other factors that influence real-world valuations and some that may well be large enough and systematic enough to warrant incorporating them directly into the model rather than impounding them in the general disturbance term. Of these, one of the most important is "growth potential," in the sense of opportunities the firm may have to invest in real assets in the future at rates of return greater than a "normal" rate of return (i.e., greater than the cost of capital)

Clearly, translating such a concept into operational terms is a task of formidable proportions and one for which no universally applicable solution can be expected. For industries such as the electric utilities,

particularly good example of the latter since corporate income taxes are deductible in computing earnings allowed on the rate base. To the extent that tax is thus passed on, the ultimate value of the tax subsidy on interest is correspondingly reduced (i.e., the total derivative of debt on value will be less than the partial of D on V *given* $\bar{X}$). Nor, finally, do firms always have a completely unrestricted choice as to their future debt policies, either because of restrictions incorporated in already outstanding debts or because, as in the case of the utilities, the regulatory authorities may impose upper (and in some cases allegedly even lower) bounds on debt levels.

[13] The independence of the cost of equity capital (and hence also of debt capital) from the interest rate is, of course, an independence only within a partial equilibrium framework. In a general equilibrium setting, there is necessarily a very direct connection between the interest rate (which may be regarded to a first approximation as the yield on assets generating sure streams) and the various ρ_k (which are the expected yields on assets generating streams of various degrees of uncertainty). But while the connection is direct (since they are mutually determined in the process of market clearing and jointly reflect such underlying factors as the level of wealth, the composition of the stock of real assets, attitudes toward risk and the like), there is no reason to believe that they must move closely together over time.

[14] N.B. Since all of the main earnings and cost of capital concepts have now been introduced we shall hereafter, in the interests of simplicity, drop all subscripts and superscripts on the variables where there is no danger of ambiguity.

however—where the growth in earnings has been (and will presumably continue to be) reasonably steady—rough, but tolerable, approximations to growth potential can probably be obtained by exploiting the so-called constant-growth model. In particular, suppose that a firm has the opportunity to invest annually an amount equal to 100 k per cent of its tax-adjusted earnings ($k \lesseqgtr 1$), on which investments it will earn a tax-adjusted rate of return of ρ^*, greater than $C=C(L)$, its average cost of capital. (These assumptions imply, among other things, that earnings will grow at the constant rate of $k\rho^*$ per year.) And suppose further that these especially profitable opportunities are expected to persist over the next T years, after which only normally profitable opportunities will be available. Then, by analogy to the solution we have derived for the certainty case (see [14, n. 15]), the current market value of the firm can be expressed as

$$V = \frac{1}{\rho}\overline{X}(1-\tau) + \tau D + k\overline{X}(1-\tau)\left[\frac{\rho^* - C}{C(1+C)}\right]T, \tag{10}$$

where the first two terms, as before, represent the capitalized value of the current tax-adjusted earning power plus the tax benefits on debt; and the last term is the contribution to value of the future growth potential.[15,16]

Despite the heroic simplifications invoked in its derivation, the above expression for growth potential is still by no means a simple one. It is the *product* of three separate elements: the profitability of the future opportunities as measured by the difference between ρ^* and $C(L)$; the size of these opportunities $k\overline{X}(1-\tau)$; and how long they are expected to last T. None of these component terms is directly observable, though some such as $k\overline{X}(1-\tau)$ and possibly ρ^* might be approximated by extra-

[15] Several considerations led to our use of the above finite growth model rather than the more popular infinite growth model (see Gordon [7] or Lintner [12]) which (taxes and leverage aside) leads to the familiar valuation formula

$$V = \frac{X(1-k)}{\rho - k\rho^*}.$$

The finite model does capture at least the essence of the S-shaped growth path which is encountered so frequently (and for good economic reasons) in empirical studies of firm and industry development. The infinite model, moreover, requires that k be strictly less than unity and $k\rho^*$ strictly less than ρ for a stable equilibrium value of the firm to exist at all; and, as a practical matter, for k and $k\rho^*$ to be substantially below these limits to avoid absurdly high valuations. In our sample, however, k is in the neighborhood of two in all three years and, we might add, that this is by no means an exceptional case. The value of k has been quite close to unity for the corporate sector as a whole in recent years.

[16] The expression for growth potential in (10) differs from that in [14, n. 15] by the inclusion of the term $(1+C)$ in the denominator. The new expression given here is the correct one, and we are grateful to H. M. Weingartner for calling our attention to the slip in the original derivation.

polating recent past experience. Accordingly, we shall, in this paper, take the simplest way out by focusing on the most tractable component $k\bar{X}(1-\tau)$, the level of investment opportunities, and impounding the others in its regression coefficient.[17]

As an empirical estimate of investment opportunities, we have used in the subsequent estimating equations the quantity $(1/5(A_t - A_{t-5})/A_{t-5}) \cdot A_t$. That is, we have used a linear 5-year average growth rate of total assets times current assets. This particular form of average, denoted for simplicity hereafter as $\overline{\Delta A}$, happens to yield consistently higher gross and net correlations with total value than other simple smoothings we have tried. But the differences are not large and the estimates of the other coefficients are not sensitive to the specific measure used.

E. *Dividend Policy, Valuation, and the Cost of Capital*

Under ideal conditions of perfect capital markets, rational investor behavior, and no tax discrimination as between sources of income, dividend policy would present no particular problem. In such a setting, we have shown [14] that, given a firm's investment policy, its dividend policy will have no effect whatever on the current market value of its shares or on its cost of capital; and that despite the impressions of some writers to the contrary (see, e.g., Lintner [11]), this conclusion is equally valid whether one is considering a world of certainty or uncertainty. Dividend policy serves to determine only the division of the stockholders' return as between current cash receipts and capital appreciation; and the division of the firm's equity financing as between retained earnings and external flotations.

The picture becomes considerably more complicated, however, as soon as we weaken the assumptions to allow for the present tax subsidy on capital gains and for the existence of substantial brokerage fees and

[17] Needless to say, the particular approach adopted here with respect to representing the growth variable is not to be regarded as of general applicability and was chosen, at least in part, with a number of very special properties of the electric utility industry in mind. First, growth in earnings and assets in the utility industry does tend to be fairly steady from year to year and there is, in particular, no problem of large spurts in growth due to mergers or acquisitions. Hence current and recent past investment levels do provide at least a meaningful starting point for estimates of future investment. Second, because of the regulated status of the industry there is reason to believe that what has been impounded in the coefficient, namely,

$$\left(\left[\frac{\rho^* - C}{C(1 + C)}\right]T\right),$$

is not only of the same sign, but at least roughly of the same order of magnitude for all companies. Certainly we do not have the wide dispersion in rates of return that would characterize, say, the railroad industry (where it is not unreasonable to suppose that for some lines ρ^* may even be less than $C(L)$) or the typical manufacturing industry whose component firms are much more exposed to competition from new products and new firms.

flotation costs. Under these conditions, a firm's dividend policy can, in general, be expected to have an effect on its market value, though the precise amount of the effect is impossible to determine a priori. Unlike the case of corporate debt, the tax subsidy to capital gains is not a constant but varies widely from investor to investor (with the subsidy actually being zero for the substantial part of the market represented by pension funds, foundations, etc., and possibly even negative for very small investors by virtue of the dividends-received credit and for corporate investors [including casualty insurance companies] by virtue of the partial exclusion of intercorporate dividends). Moreover, the tax disadvantages of high payout shares may be partly or wholly offset by other influences. Elderly or retired investors—a nontrivial segment of the market—who are decumulating (or at least not further accumulating) their wealth in the form of shares may find considerable savings in brokerage fees and other costs of portfolio adjustment in high payout as opposed to high capital gain securities. Even net accumulators may be induced to hold high payout shares despite their lower after-tax yields either for reasons of control (broadly understood to include shares held by management as an incident to employment) or for reasons of diversification.

Given this uncertainty as to the size, and to some extent even the direction of the dividend effect, the indicated course might seem to be simply to add a dividend term with unspecified coefficient to the structural equation (10) and let the sample determine its value. From such a valuation equation we could, of course, also go on to derive an extension of the cost of capital formula (9) running in terms of dividend policy as well as debt policy.[18]

The trouble with such an approach, however, is that if applied in straightforward fashion (as in Gordon [6] [7] or Durand [5]), the resulting estimate of the dividend coefficient will inevitably be strongly upward biased (and the key earnings coefficient correspondingly downward biased). Since the precise mechanism generating this bias will be discussed at length in a later section, we need not dwell on the matter further at this point beyond observing that the difficulty arises from the widespread practice of dividend stabilization. With current dividends based in large part on management's expectations of long-run future earnings, the dividend coefficient in the regression equation will

[18] Although the procedure for deriving the marginal and average costs of capital in the dividend case is analogous to that for the leverage case, the derivation is considerably more complicated. Further difficulties arise from the fact that in the presence of such major market imperfections, maximizing market value is no longer always equivalent to maximizing the economic welfare of the owners. Since these and related problems are largely peripheral to the main concerns here, further discussion of them will be deferred to separate papers.

reflect this substantial informational content about $\overline{X}(1-\tau)$ along with the true effect, if any, of dividends per se on valuation.[19]

Because of this confounding of the earnings and dividend coefficients, our approach here will be initially to omit the dividend variable entirely, and to focus on the problem of estimating the earnings coefficient (which is, of course, to be interpreted as the capitalization rate for earnings for companies following the sample average dividend policy). As we shall see, the procedures to be developed for obtaining consistent estimates of the key earnings coefficient will also permit us to go on to obtain at least order of magnitude estimates of the true dividend effect for the sample years.[20]

F. *Size and Valuation*

All the valuation equations so far considered have been written as linear homogeneous functions of the independent variables, implying among other things that a given proportionate change in the values of all the independent variables leads to an equal proportionate change in the market value of the firm. The results of previous valuation studies (see, e.g., Gordon [7]) suggest, however, that the true market capitalization rate for the expected earnings of large firms may possibly differ systematically from that of small firms in the same industry.

As was true of the growth effect, there are a number of possible ways of incorporating this size or scale effect into the model. By far the simplest—and hence the approach we shall here adopt—is merely to add a constant term to the valuation equation.[21] The resulting nonhomo-

[19] Another possible source of bias arises from the accelerated depreciation provisions of the Revenue Act of 1954. As a result of the increased deductions permitted, part or all of the dividends paid by some companies in our sample (and in other industries as well) are considered for tax purposes as exceeding accumulated earned surplus and hence as being paid out of "capital." Such returns of capital are not taxable to investors—the investor merely being required to reduce the basis (i.e., "cost") of the security for purposes of computing the capital gain or loss on sale. Since the higher the payout, the higher the probability that the dividend may qualify as tax-exempt, the net effect is likely to be an upward bias in the dividend coefficient.

[20] We hasten to add that omitting dividends is, again, not a procedure we would care to recommend for general application; and that like our solution for the growth problem, the approach was adopted, in part, with some of the special properties of the utility industry very definitely in mind. For one thing, payout ratios are far less variable from company to company in this industry so that our eventual estimate of the average capitalization rate for earnings will be a much more representative average than it would be for the general run of industry groupings. Moreover, the average payout ratio is quite high, so much so that utility stocks have long represented the example *par excellence* of "income stocks." Since the clientele attracted to such stocks is likely to be much less sensitive to the tax subsidy to capital gains, the size of the dividend effect (and hence of the specification error involved in omitting it) is also likely to be much smaller here than elsewhere.

[21] Our reasons for not wanting to introduce a continuous size variable, such as total assets, at this stage will become clear from the discussion of the errors of measurement problem in Section II, below.

geneous equation must then be interpreted as the linear approximation over the sample range to the underlying nonlinear relation, and the coefficient of the earnings variable as the (constant) marginal capitalization rate in the industry. The magnitude and direction of the scale effect would be indicated by the size and sign of the constant term. A negative constant term would mean that the average capitalization rate is less than the marginal and hence that the average capitalization rate tends to rise with increasing size of firm. A positive value for the constant term, on the other hand, would imply decreasing returns to scale in valuation.

G. *Summary*

Our analysis of the theory of valuation leads to the following structural equation:

$$(V - \tau D) = a_0 + a_1 \overline{X}(1 - \tau) + a_2 \overline{\Delta A} + U, \tag{11}$$

where a_1 is the marginal capitalization rate for pure equity streams in the class, and hence the key parameter for deriving the cost of capital; a_0 is an intercept term whose size and sign will measure any effects of scale on valuation; a_2 is a measure of the effects of growth potential on value; and U is a random-disturbance term. Notice that since the theory implies that the coefficient of the leverage variable D is equal to the marginal corporate tax rate τ, we have so constrained it in the above equation by incorporating it with the dependent variable. There remains the problem of specifying the nature and properties of the disturbance term and it is to this task that we now turn.

II. *The Statistical Model*

Least-squares estimates of the coefficients of (11) will be efficient and unbiased only if among other things (i) the variance of the disturbance term is a constant, independent of the size of the firm; and (ii) the disturbances are not correlated with the independent variables. Unfortunately, neither of these conditions can reasonably be expected to hold in our sample.

A. *Heteroscedasticity*

As for the variance of the disturbances, one would certainly suppose that the errors in a valuation equation (including errors in measuring $(V-\tau D)$) are of the multiplicative, rather than the additive variety. And indeed, check of the simple scatter of value on measured earnings suggests that the standard deviation of the error term is approximately proportional to the size of firm. Any attempt to fit (11) directly, therefore, would be highly inefficient and in our sample (where the largest

firm is on the order of 100 times the smallest) the results would be completely dominated by a handful of giant companies.

In the present context, there are at least two approaches worth considering as possible solutions for this problem of heteroscedasticity: (i) dividing (11) through by $(V-\tau D)$ and re-expressing the structural relation in so-called "yield" form; or (ii) weighting each observation in inverse proportion to the size of the firm and hence to the size of the standard deviation of the error.[22] The former (which was the approach we adopted in our first paper [15] and which was subsequently followed by Barges [1]), leads to the estimating equation:

$$\frac{\bar{X}(1-\tau)}{V-\tau D} = a_1' + a_0' \frac{1}{V-\tau D} + a_2' \frac{\overline{\Delta A}}{V-\tau D} + u', \tag{12}$$

where $a_1' = \rho =$ the reciprocal of the capitalization rate for pure equity streams (or, equivalently, the "marginal cost of equity capital");

$$a_0' = -a_0\rho;\ a_2' = -a_2\rho;\quad \text{and}\quad u' = -\rho\frac{U}{V-\tau D},$$

with Var (u') approximately a constant for all firms.[23]

While an approach of this kind has the virtue of simplicity, it suffers from the fact that the variable $(V-\tau D)$ enters into the denominator of the ratios on both sides of the equation. This is not only somewhat unesthetic—since we are, in effect, using V to explain V—but will lead to biased estimates to the extent that $(V-\tau D)$ contains stochastic

[22] A third possibility, of course, would be to use the logarithms rather than the natural values of the variables, but this would be inappropriate since the growth term, as we have here derived it, enters as an additive rather than a multiplicative or exponential term. It could be argued, of course, that given the heroic assumptions underlying that growth variable, the additional specification error introduced by the log transform is not likely to be of overwhelming concern and there is certainly some merit in this position. The trouble is, however, that our interest here is both in estimating the cost of capital and in testing certain hypotheses about valuation. For the latter purpose, as we shall see, additional variables must be incorporated into the equations and these very definitely enter in linear form. Since the other methods for dealing with heteroscedasticity are entirely adequate for that purpose and also carry through more naturally to the testing problem, we shall work with them exclusively in the sections to follow.

[23] An alternative form of yield equation, though somewhat less convenient, would be

$$\frac{\bar{X}(1-\tau)}{V} = a_1''\left(1-\tau\frac{D}{V}\right) + a_0''\frac{1}{V} + a_2''\frac{\overline{\Delta A}}{V} + u'', \tag{13}$$

where $\bar{X}(1-\tau)/V$ can be interpreted as the tax-adjusted earnings yield on total value (approximately the "average cost of capital" of the finance literature) and $a_1''=\rho$. Note that considerations of efficiency suggest using the quantity $(1-\tau D/V)$ as the leverage variable and suppressing the constant term rather than estimating separately a constant term and a coefficient for D/V as is usually done when (13) or variants of it are fitted to the data.

The economic meaning of yields such as $\bar{X}(1-\tau)/V$ and $\bar{X}(1-\tau)/(V-\tau D)$ and their relations to the cost of capital will be considered at length in Section IV, below.

elements independent of those in the numerator of the ratios. In the present case, this will mean that the coefficients of the growth and size variables will be too high (i.e., less negative) and that the estimate of the cost of capital (from the intercept term a_1') will be correspondingly too low. Since $(V-\tau D)$ certainly does have a stochastic component—impounded in the term U in (11)—and since we have, at this stage, no basis for judging how large the resulting bias really is, we obviously cannot afford to rely exclusively on estimating equations of this form.[24] Instead, we shall use them here essentially only for checking the reasonableness of the estimates obtained via the weighted regression approach.

As for implementing the weighted regression approach, the fact that the standard deviation of the error term in (11) is roughly proportional to size of firm means that the required weighting can be effected by the relatively simple expedient of deflating each of the variables by some scale variable such as the book value of total assets, denoted by A. Our reason for using total assets as a deflator rather than, say, total sales (as, e.g., in Neilsen [19]) is mainly that in the utility industry at least such deflated terms as V/A, D/A, or $\overline{X}(1-\tau)/A$ have natural and useful economic interpretations in their own right. The equation to be fitted, then, will be of the form

$$\text{(14)} \qquad \frac{V - \tau D}{A} = a_0 \frac{1}{A} + a_1 \frac{\overline{X}(1 - \tau)}{A} + a_2 \frac{\overline{\Delta A}}{A} + u,$$

with $u = U/A$ and Var (u) = a constant.

One question that immediately arises in connection with (14) is the status of the constant term. Recall that we are interpreting the basic valuation equation (11) in the original, undeflated variables as a linear approximation over the sample range, with its constant term a_0 serving as a measure of the effect of scale on valuation. To preserve this interpretation we must, therefore, regard the derived deflated regression (14) as homogeneous, that is, as being fitted with no constant term and with the coefficient of the variable $1/A$ now measuring the size effect. Failure to suppress the constant term in the deflated regression would imply that the book value of total assets appears along with expected future earnings as one of the explanatory variables in the original equation—a specification that makes little sense from the standpoint of the theory of valuation. We shall, of course, attempt to check the validity of the assumption that the regression in deflated form has a zero constant term, but there are complications involved, as will become clear from the discussion in the next section.

[24] The problem of bias in the yield-form tests is further considered in note 28, below.

B. *Errors of Measurement in Expected Earnings*

In using an equation such as (14) to estimate the cost of capital, the key variable is, of course, $\overline{X}(1-\tau)/A$, defined, it will be recalled, as the market's expectation of the long-run, future, tax-adjusted earning power of the assets currently held by the firm. Since it is an expectation, it is not directly observable or measurable and the best that can normally be done is somehow to approximate it from the firm's published accounting statements. This best, unfortunately, is likely to be none too good even in an industry such as the electric utility industry where there is substantial uniformity of accounting conventions among firms; where there are, at least in our sample period, no firms suffering net losses; and where large, year-to-year random fluctuations in reported earnings seem to be relatively rare.

The implications of these inevitable errors in the measurement of earnings for the problems at hand are perhaps most easily seen by expressing the underlying structure as the following *system* of equations where, to simplify the notation we let

$V^* = \dfrac{V - \tau D}{A}$; $X^* = \dfrac{\overline{X}(1 - \tau)}{A}$ = the "true" unobservable expected earnings;

X = deflated earnings as measured from the accounting statements; and Z_i, $i=1 \cdots m$, stand for all other relevant variables (including constants, where appropriate):

$$(15)\qquad \begin{aligned} &(1)\quad V^* = \alpha X^* + \sum_{i=1}^{m} \beta_i Z_i + u \\ &(2)\quad X = X^* + v \\ &(3)\quad X^* = \sum_{i=1}^{m} \gamma_i Z_i + w, \end{aligned}$$

where some γ_i and β_i may be zero; and with the error terms assumed to be independent of each other and to have mean zero and (constant) variances σ_u^2, σ_v^2, and σ_w^2, respectively. In words, the value of the firm depends on expected earnings and certain additional explanatory variables; measured earnings are merely an approximation to true expected earnings, the error of measurement being v; and lastly, some, at least, of the explanatory variables are also correlated with (and hence convey information about) the true, but unobservable X^*.[25]

[25] Note that we have assumed X to be an unbiased estimate X^*. We shall also assume that the error of measurement v is uncorrelated with any of the Z_i (u and w meeting this requirement by construction). Neither of these assumptions is essential for present purposes, though they simplify the presentation, and we shall consider later some of the implications of relaxing them.

Equations (15, 1–3) thus constitute a simultaneous system in which V^* and X are, in effect, endogenous variables, and the Z_i are exogenous variables. It follows then that if we attempt to fit by direct least squares the single equation,

$$V^* = aX + \sum_{i=1}^{m} b_i Z_i + u', \tag{16}$$

in which V^* is regressed on the Z_i and the endogenous, measured earnings X, the error term u' will not be independent of X, and the coefficients of (16) will be subject to the equivalent of the familiar simultaneous equations bias. More concretely, it can readily be shown (see, e.g., Chow [3, esp. pp. 94–98]) that in the limit for large samples the coefficient of X will be given by

$$a = \alpha \frac{\sigma_w^2}{\sigma_w^2 + \sigma_v^2},$$

which is less than the true value α, and the more so, the larger the variance of the error of measurement σ_v^2, and the better the included exogenous variables are as proxies for earnings (i.e., the smaller the value of σ_w^2).[26] As for the other variables, the coefficients will be given by

$$b_i = \beta_i + \gamma_i \alpha \frac{\sigma_v^2}{\sigma_w^2 + \sigma_v^2} = \beta_i + \gamma_i [\alpha - a]$$

and thus may be larger or smaller than their true values β_i, depending on the direction of correlation with X^* (i.e., on the sign of γ_i).[27,28]

[26] The above expression for the bias in the earnings coefficient was derived on the assumption that (16) was fitted *with* a constant term. If the equation were fitted without a constant term (and if, as we have assumed, there is no constant term in the true specification), then the apparent bias would be considerably smaller. The reason is, of course, that the bias, by flattening the slope of the regression, tends to produce a positive intercept even where none really belongs. Hence forcing the regression through the origin and eliminating the artificial intercept offsets some of the distortion. The offset is only partial, however, and forcing the regression through the origin cannot be regarded as a satisfactory substitute for the more elaborate methods for eliminating the bias to be introduced below.

[27] Note that even if some $\beta_i = 0$ (implying that the corresponding Z_i really has no effect on market value), its estimate b_i might still be positive if $\gamma_i > 0$, and that b_i might be quite large if γ_i is large and if the measurement error in X is substantial (so that a is considerably smaller than α). This is, of course, precisely the "information effect" or proxy variable bias we were concerned about in connection with the dividend variable (cf. above, Section II.E and see also the discussion in our [16]).

[28] In the light of the system (15) it is interesting to re-examine the problem of bias in the yield-form equations (cf. Sec. II.A). Using the notation above, but interpreting the variables as undeflated, the proposed yield equation becomes

C. *An Instrumental Variable Approach*

Recasting the original structure in the form (15) not only serves to clarify the nature of the biases introduced by errors of measurement, but also to suggest a remedy, namely an instrumental variable approach. For reasons of computational simplicity as well as ease of interpretation in the present context, we shall implement this approach by means of a two-stage procedure formally equivalent to the two-stage least-squares method of Theil [21]. See also [13]. Operationally, this means first regressing the endogenous variable X on all the instrumental variables Z_i, thereby obtaining estimates g_i of the coefficients γ_i in (15.3). From these estimates a new variable $\hat{X}$ is formed, defined as

$$\sum_{i=1}^{m} g_i Z_i,$$

and thus constituting an estimate of X^* from which, if our assumptions are correct, the error of measurement v will have been purged.[29] If $\hat{X}$

$$\frac{X}{V^*} = \frac{1}{\alpha} - \frac{1}{\alpha} \sum_{i=1}^{m} \beta_i \frac{Z_i}{V^*} + u'', \tag{17}$$

where

$$u'' = \frac{v - \dfrac{1}{\alpha} u}{V^*}.$$

Under our assumptions (a) that X is an unbiased estimate of X^*, and (b) that v is uncorrelated with the Z_i, the measurement error v leads to inefficiency, but not to bias since that error is independent of the explanatory variables. The trouble comes rather from the fact that the endogenous variable V^* is now included among the explanatory variables, thereby leading to a lack of independence between the error term u'' and the variables Z_i/V^*. Thus, the nature of the bias is different when the form of the equation is changed; but given the fact that we have two endogenous variables, bias in one form or another is bound to arise under any single-equation approach.

[29] That is, (15.2) and (15.3) imply

$$X = \sum_{i=1}^{m} \gamma_i Z_i + v + w$$

so that

$$\hat{X} = \sum_{i=1}^{m} \gamma_i Z_i = X - v - w = X^* + v - v - w = X^* - w,$$

the v term being eliminated. The additional error term $-w$ causes no problems (other than loss of efficiency) since by construction w is orthogonal to all the Z_i and hence to $\hat{X}$. In practice, of course, we do not know the γ_i exactly but only the regression estimates of them, g_i. Hence, in defining $\hat{X}$ as $\sum_{i=1}^{m} g_i Z_i$, there is another error term of the form $\sum_{i=1}^{m} (\gamma_i - \ \ _i) Z_i$, which will, however, tend to approach zero as the sample size increases.

is then used in the second stage as the earnings variable in (16) in place of X, (and if the conditions for identification are met) the resulting estimates a and the b_i can be shown to be consistent estimates of α and the β_i in the basic structural equation (15.1).

As for the specific exogenous or instrumental variables to be used we have already considered two, growth and size. In addition, if the coefficients in (16) are to be identified, we must find at least one more such variable that may properly be included in the first-stage regression of X on the Z_i and properly be excluded from the second-stage equation (16). We say at least one more because while even a single such variable will suffice for identification, additional variables, by actually overidentifying (16), will help to reduce the loss in the efficiency of estimation—that loss being the price we normally have to pay for gaining consistency by using an instrumental variable approa h. In concrete terms, what we need are, of course, variables that can be expected to be strongly correlated with (and hence to convey considerable information about) X^* and, at the same time, to be uncorrelated with the error of measurement in X.[30] Fortunately, in the present context, we have a number of likely candidates readily at hand.

The obvious first choice as an instrumental variable for long-run expected earnings is, of course, total assets. This will not provide a perfect measure since the rate base for regulation is not quite the same as total assets and since the allowable rate of return on the base varies somewhat from state to state. Given the fact of regulation, however, and the (partly consequent) substantial uniformity of accounting conventions in the industry, the amount of information conveyed by total assets is still substantial, and certainly far more so here than for virtually any other industry. Note, incidentally, that the fact that all our variables are deflated by total assets creates no particular problem. We can, in effect, incorporate total assets in the first-stage regression by the simple expedient of fitting it with a constant term.

Another presumably very powerful instrumental variable for the net profit component of total earnings would be current dividends paid. As has been stressed here at several points, the practice of dividend stabilization— and the utility industry is the *locus classicus* of such policies—means that dividends and dividend changes indirectly convey a considerable amount of information at least about management's expectations of long-run future profits. In fact, it will be recalled, it was precisely because they conveyed so much information that we were led to omit dividends entirely from the basic valuation equation in order to

[30] The second requirement is important, of course, because to the extent that v is correlated with the Z_i, part of the measurement error will remain in $\hat{X}$. In fact, if the correlation is very strong, $\hat{X}$ will be little different from and hence little better than X itself.

avoid hopeless confounding of the real and the purely informational effects. The instrumental variable approach thus permits us to break this impasse and to salvage at least some of the informational content of dividends.[31]

For the interest component of expected earnings, it might be thought that the reported figure would be sufficiently close to the true value so that no specific instrumental variable need be introduced on this account. A glance at the accounting statements, however, will show that even interest paid is not an unambiguous concept in the utility industry (nor are the accounting practices completely uniform from company to company, particularly in such matters as the treatment of interest on construction and the separate reporting of interest on short-term obligations). There is the further difficulty that some companies may have issued (or retired) bonds during the year with the result that the reported interest payments for the year represent less than a full-year's interest obligation on the debt outstanding at the end of the year (or more than the future level of obligations in the event of a retirement). Accordingly, an instrumental variable may well prove worthwhile for interest and preferred dividends with the natural choices being the market value of the debt and of the preferred stock. Inclusion of these capital structure variables has the further advantage of picking up any systematic correlation between leverage and the level of total earnings as might arise, for example, if regulatory commissions, as has been suggested, attempt to pass the tax savings from interest deductions forward to consumers by lowering rates.

Though the variables described above would seem adequately to meet the first requirement for good instrumental variables—namely that they be correlated singly and in combination with the unobservable market expectations of normal earnings—we can, of course, never be entirely sure that they also meet the second test—namely that they be uncorrelated with the error in measured earnings. Actually, this independence condition cannot be met literally and exactly. A transient upward fluctuation in current measured profits, for example, must show up either in temporarily higher dividends, or more likely in a level of total assets higher than it otherwise would have been. But while complete independence is not to be expected, we would doubt that such

[31] It may seem strange at first glance to treat dividends as an exogenous variable in an equation "explaining" X^* when the causation clearly runs from X^* to dividends as a matter of business policy. The fact that dividends may be an endogenous variable from the standpoint of some larger system creates no serious problem at this point, however, since the only use here made of the coefficients in the first stage is in building up an $\hat{X}$ from which the measurement errors in X have been purged. As long as this is accomplished by the first-stage variables taken as a set, the individual coefficients are of little moment. Eventually, of course, when we attempt to put bounds on the effects of dividends on value, we shall have to face up to the endogenous nature of dividends. See below, Section III.E.2.

correlation as does exist is so large as to dash all hopes for substantially improving the estimates by the instrumental variable procedure.

III. *The Results*

A. *Definitions of the Variables*

Before presenting and discussing the results in detail, some brief remarks may be in order with respect to the precise definitions of the variables. Measured, tax-adjusted earnings can be derived from published accounting statements either (a) by means of the formula $\overline{X}(1-\tau)$ where $\overline{X}$ is total before-tax earnings, and τ is the marginal corporate tax rate of 52 per cent in the sample period; or (b) from the formula $\overline{X}^{\tau}-\tau\overline{R}$, where $\overline{X}^{\tau}$ is after-tax earnings, and $\overline{R}$ is expected interest payments (cf. n. 8). If tax liabilities were exactly equal to 52 per cent of profits as reported to the shareholders, the two concepts would, of course, be identical. In practice, however, profits as defined for tax purposes do not in general coincide with those reported to the public and the consequent discrepancy between measures (a) and (b) is large enough to force a choice to be made.

It is difficult on purely a priori grounds to establish a decisive case for either alternative. Measure (b) has perhaps an advantage in that reported $\overline{X}^{\tau}$ is the concept of earnings in terms of which the "reasonable return" is defined by the regulatory commissions. In addition, measure (b) permits an exact derivation of regression equations describing the valuation of shares from those in terms of the value of the firm as a whole—a property that is of considerable value for constructing tests of conflicting hypotheses about the influence of leverage on valuation. Since it also happens to be the case that measure (b) is consistently and substantially the better of the two by the goodness-of-fit criterion, that measure will be used here throughout, and tax-adjusted earnings will thus be estimated as $\overline{X}^{\tau}-\tau\overline{R}$.[32]

In building up our measure of tax-adjusted earnings from the accounting statements we have taken from *Moody's Utilities* the current-year figures for net profits, preferred dividends, and interest paid as defined by the companies themselves. Initially, when we first recognized the seriousness of the errors of measurement problem we experimented with a number of alternative concepts of earnings (e.g., adding back to profits any allocations to a reserve for deferred taxes). None of these concepts, however, appeared to be better than the actually reported current earnings in the sense of yielding consistently higher gross and

[32] In terms of the numerical results, the main difference between the two measures is that measure (a) turns out to yield somewhat lower estimates of the cost of capital. In all other respects, however, such as the contributions of the other variables, the behavior of the systems over time and the results of various tests applied to check the specification, the two measures, insofar as we have tested them, give an essentially similar picture.

net correlations with V or of showing less evidence of attenuation in direct least-squares regressions. The same was true, moreover, for simple equally weighted two- and five-year averages of past reported earnings. The relatively poor performance of such averages is probably a reflection of the fact that in the utility industry, the purely "transitory," random components in earnings tend to be quite small relative to the "permanent" shocks (such as those induced by changes in regulation policies or in accounting conventions). Where such is the case, it might well be that some *unequally* weighted average with high weight on the current year probably exists that would do better than either current earnings alone or a simple average. We have, however, made no further attempts to find such an average because there is no real hope that even such an improved measure would be sufficiently error-free to permit dispensing with the instrumental variable procedure. At best there might be a small gain in efficiency, but the eventual second-stage estimate is unlikely to be very different. For simplicity, therefore, we chose to work with current-year earnings (though, once again, this is not a procedure we would recommend for other industries, particularly those subject to cyclical fluctuations in earnings).[33]

Another troublesome variable offering several possible choices of definition is the debt variable. Originally, we had thought we could focus exclusively on long-term debt, ignoring short-term liabilities most of which would presumably be noninterest bearing and which, in any event, we expected to be small and relatively uniform throughout the industry. The major definitional problem would then be that posed by convertible issues selling at a substantial premium. As it turned out, however, such issues were quite unimportant during our sample period; but short-term liabilities proved to be both highly variable and in some cases very large in relation to total liabilities. In part, at least, this variation seems to stem from the practice of many companies of financing construction with short-term bank loans that are later refunded after completion of the project. Since it was not always possible to

[33] The main danger in using current earnings arises from the possibility that because of, say, industry-wide strikes or cyclical disturbances, current earnings may be a systematically *biased* estimate of long-run earnings throughout the industry as a whole. That is, in terms of the system (15), the second equation would have to be replaced by $X=\lambda X^*+v$, $\lambda\neq 1$, implying in turn that the ultimate second-stage estimate of α will be a biased one—yielding α/λ rather than α. Where such industry-wide distortions are present, an average of past earnings will be a less biased measure of X^* and should be used in the first stage even in the unlikely event that it did not perform as well as current earnings in the direct least-squares regression of V on X. In our industry, it happens that the sample mean (deflated) current earnings are so close to the sample mean average earnings that nothing much would be gained in using the average. This is not to say, of course, that either current earnings or average earnings constitute an unbiased estimate of X^*. We suspect that there probably is some (slight) systematic understatement of earnings in both cases due to the more generous depreciation allowances permitted for tax purposes during the Korean War (and by the Revenue Act of 1954) and reflected in profits, either directly or indirectly, via the reserve for deferred taxes.

separate out such loans from other short-term liabilities, the safest course seemed to be to include all short-term liabilities as well as long-term liabilities in the debt variable. Any errors that this approach may involve are not likely to create serious difficulties for present purposes of estimating the cost of capital.[34]

Debt, preferred stock, common stock and their sum, total value of the firm, are taken at market values and are based on price quotations as of the end of each sample year. To iron out any purely short-term randomness in the price quotations, a simple average of December and January weekly closing prices was employed. The necessary price quotations were always available for common and preferred stock issues, but not always for long-term debt issues where an issue was privately placed and held. In such cases, we estimated the market value on the basis of the yields reported by *Moody's* for comparably rated issues of equivalent term. For the short-term liabilities, we had, of course, no choice but to use the book value of such liabilities. Our measure of V, therefore, must certainly be regarded as subject to error. But there is no reason to believe that these errors (plus the inevitable purely clerical errors) are of a sufficiently systematic nature to bias the estimates.

B. *The Single-Equation Least-Squares Estimates*

Turning now to the results themselves, we first present in Table 1 the estimates obtained from single-stage, least-squares regressions using reported current tax-adjusted earnings as the earnings variable.[35] If our analysis has been correct, these estimates should display at least the more obvious symptoms of attenuation bias; and a glance at the top panel of the table shows that such is indeed the case. Here the equation has been fitted with a constant term—a specification, it will be recalled, that is equivalent to adding total assets as an explanatory variable in the undeflated equation. These constants are not only significantly positive in two of the three years—on the order of three to four times their standard errors—but are quite large relative to the mean value of the dependent variable, which is approximately .9 in all three years. The effect is particularly marked in 1954, where the value for the constant of .274 would be far too high to be taken seriously as a measure of the true explanatory contribution of total assets to market value if earnings were measured without error.[36]

[34] Even though the debt variable may include random measurement error, the fact that it is incorporated into the dependent variable keeps such errors from introducing any systematic bias in the coefficients of the value equation. Some problems may arise, however, where tests of the specification are involved. See below, Section III.E.1.

[35] The means and standard deviations of the major variables along with the matrix of simple correlation coefficients are presented in Appendix B.

[36] Just why the measurement error might be higher in 1954 than in either of the two subsequent years is still not entirely clear to us. Possible explanations might be (a) that fundamental

TABLE 1—DIRECT LEAST-SQUARES ESTIMATES WITH MEASURED EARNINGS
Dependent Variable: $(V-\tau D)/A$

Year	Coefficients of				Mult. R	Adjusted Standard Error	Ratio of Adjusted Standard Error to Mean V/A
	Constant	Earnings $(\overline{X}^{\tau}-\tau\overline{R})/A$	Size $1/A\cdot 10^7$	Growth $\overline{\Delta A}/A$			
1957	.164 (.06)	15.7 (1.2)	−.278 (.08)	1.37 (.24)	.88	.057	.052
1956	.057 (.06)	15.6 (1.2)	−.122 (.08)	.846 (.23)	.88	.057	.051
1954	.274 (.06)	13.7 (1.3)	−.192 (.06)	.305 (.15)	.83	.054	.045
1957	—[a]	16.0 (.44)	−.277 (.08)	1.39 (.23)	.88	.057	.052
1956	—[a]	16.6 (.39)	−.111 (.07)	.926 (.21)	.87	.057	.051
1954	—[a]	19.2 (.43)	−.205 (.07)	.466 (.17)	.75	.063	.053

[a] Constrained to equal zero.

In the bottom panel of the table, we present the results obtained with the same variables but with constant term suppressed. As would be expected (cf. n. 26), the estimates of the capitalization rate are all higher (especially so in 1954). If our analysis is correct, however, they are still somewhat too low, so that still higher values should presumably be found in the second-stage regressions.[37]

C. *The First-Stage Regressions on the Instrumental Variables*

Table 2 shows the first-stage regressions of reported arnings on the instrumental variables. To help in interpretation, the results are presented with and without dividends included among the set of instrumental variables. As can be seen from the first panel, total assets appear to convey considerable information about earnings in this industry—the constant terms ranging from just under 4 to just over 7 times their standard errors. Given total assets, moreover, little additional information seems to be provided by the other variables as evidenced by the low values of the multiple correlation coefficient.

The group of regressions in the second panel, which will serve as the basis for the computed earnings variable in most of the subsequent second-stage regressions, involves the full set of instrumental variables including dividends. As expected, the dividend variable makes a very substantial contribution in all three years. In fact, the combination of dividends, debt, and preferred stock now proxies for the earnings stream so well that there seems to be relatively little left for total assets to contribute (especially in 1956 where total assets are completely swamped).

As for the actual numerical values of the coefficients, there is, of course, little point in attempting to interpret them separately. The relevant concern rather must be whether they seem to behave "sensibly"

---

revisions in the tax law were made in that year by the Revenue Act of 1954, particularly in the important area of depreciation allowances; (b) that earnings in 1954 still reflected to some extent transitory elements stemming from the Korean War and its immediate aftermath; and (c) that for the many companies in our sample that had only relatively recently emerged as separate entities following dissolutions under the Holding Company Act, reported earnings still reflected transients connected with the process of dissolution.

[37] For what it may be worth as an aid in judging goodness-of-fit, the table also shows a column labeled "Multiple R" for the no-constant regressions. The entry was computed according to the usual definition of R (i.e., $1-R^2$ = the ratio of the residual variance around the regression to the variance of the dependent variable) even though this may lead to nonsense results when the fit is extremely poor (since the residual variance around the constrained regression line may actually exceed the variance of the dependent variable). An alternative and probably more meaningful way of looking at goodness-of-fit with constant suppressed is to ask how well the regression enables us to "predict" the value of the firm. For this purpose we show the adjusted standard error—the adjustment consisting of summing the squared deviations not around the actual sample mean residual but around zero which is the presumed mean if the specification were really correct—and the adjusted error as a per cent of the sample mean value of V/A.

TABLE 2—FIRST-STAGE LEAST-SQUARES REGRESSIONS OF EARNINGS ON THE INSTRUMENTAL VARIABLES

Dependent Variable: $(\overline{X}^{\tau}-\tau\overline{R})/A$

Year	Coefficients of						Mult. R
	Constant	Size $1/A\cdot 10^7$	Growth $\overline{\Delta A}/A$	Debt D/A	Preferred Stock P/A	Dividends div/A	
1957	.067 (.01)	.0072 (.008)	.030 (.03)	−.041 (.02)	−.022 (.02)		.31
1956	.055 (.01)	−.0021 (.009)	−.009 (.03)	−.010 (.02)	−.011 (.02)		.13
1954	(.01) (.01)	(.0090) (.007)	−.001 (.02)	.010 (.02)	.004 (.01)		.19
1957	.014 (.01)	.0073 (.006)	.055 (.02)	−.066 (.01)	.029 (.01)	1.18 (.16)	.73
1956	−.001 (.01)	−.0009 (.006)	.022 (.02)	.025 (.02)	.040 (.01)	1.29 (.17)	.71
1954	.011 (.01)	.0039 (.005)	.011 (.02)	.019 (.01)	.024 (.01)	.864 (.14)	.64

taken as a whole and here we can derive at least some comfort from the relative stability of the relations over time. There is evidence of drift over time in some of the coefficients; but the key coefficients with one exception (the debt coefficient of 1957) are of the same sign, order of magnitude, and approximate level of significance in all three years.[38]

D. *The Second-Stage Estimates*

Table 3 presents the second-stage estimates with the earnings variable constructed from the full set of instrumental variables using the regression coefficients in panel II of Table 2. In the upper half of Table 3 the regressions have been fitted with a constant term so as to provide a check on the validity of our specification that the true constant term is zero. Comparison with the corresponding panel of Table 1 shows that the constant term does indeed drop sharply in the two years, 1957 and 1954, for which it was substantial in the direct least-squares regressions. One coefficient changes sign and all three constants are now much smaller than their standard errors.[39] We have therefore considerable support for our hypothesis that the true constant term is zero; and that the large positive constants of the direct least-squares regressions are simply a reflection of the proxying power of total assets when earnings are measured with error. Further confirmation of the errors model (as well as of our conjecture that the errors of measurement in earnings would not be strongly correlated with our instrumental variables) is

[38] Although the coefficients in the second set of regressions in Table 2 are relatively stable over time, considerable difference exists between the two sets in the values of the coefficients, particularly of debt and preferred stock. There is, however, nothing anomalous or surprising in these differences. With dividends excluded, the debt and preferred coefficients in the first set show the effects on total earnings of differences in debt and preferred *given total assets*, i.e., essentially the effects of differences in "capital structure." With dividends included, and the constant term close to zero, the debt and preferred coefficients in the second set are largely showing the effects of differences in debt and preferred *given net profits*, i.e., essentially the proxying power of debt and preferred for their corresponding pieces of the income stream.

[39] Since no "canned" two-stage least-squares computer program was available to us at the time the estimates were made, the coefficients appearing in Table 3 (and in all subsequent second-stage tables) were computed literally in two stages. That is, a new earnings variable was actually constructed from the first-stage regressions and the second-stage regression run with that variable included among the independent variables. The coefficients so obtained are, of course, exactly the same as those that would have been obtained from a 2SLS program, but that is not true for the standard errors of the coefficients (or the standard error of the regression). In terms of previous notation, the sampling variances in the literal second stage will be the product of the inverse of the moment matrix and the scalar Var $(u+\alpha w)$; whereas the appropriate scalar multiple for our specification should be Var $(u-\alpha v)$. An estimate of the latter is provided by the residual variance of the direct least-squares equation recomputed with the 2SLS coefficients. In those cases in which the constant term in the regression is suppressed the desired residual variance must, of course, be computed around the presumed mean of zero under the specification, rather than around the actual sample mean residual. We are grateful to Albert Madansky for some helpful discussion on these and certain related points.

TABLE 3.—SECOND-STAGE ESTIMATES WITH COMPUTED EARNINGS

Dependent Variable: $(V-\tau D)/A$

Year	Coefficients of				Mult. R	Adjusted Standard Error	Ratio of Adjusted Standard Error to Mean V/A
	Constant	Computed Earnings[b] $(\overline{X^{\tau}-\tau R})/A$	Size $1/A \cdot 10^7$	Growth $\overline{\Delta A}/A$			
1957	−.004 (.08)	16.2 (1.7)	−.280 (.08)	1.37 (.24)	.87	.057	.052
1956	.054 (.08)	15.6 (1.6)	−.122 (.07)	.847 (.23)	.87	.057	.051
1954	.072 (.10)	18.1 (2.2)	−.234 (.07)	.286 (.17)	.77	.060	.050
1957	—[a]	16.1 (.46)	−.280 (.08)	1.36 (.23)	.88	.057	.052
1956	—[a]	16.7 (.40)	−.114 (.07)	.896 (.21)	.87	.057	.051
1954	—[a]	19.7 (.45)	−.244 (.07)	.299 (.18)	.73	.063	.053

[a] Constrained to equal zero.
[b] From Panel II of Table 2.

provided by the behavior of the earnings coefficients, all of which are higher than their counterparts in Table 1—particularly so in 1954 where the coefficient increases by nearly 40 per cent.

The second part of Table 3 shows the second-stage regressions with constant term constrained to equal zero. Since the a priori value of the constant term in our model is zero and since the actual sample values, as we have seen, do not differ significantly from zero, these constrained estimates are the most efficient obtainable under our specification. Notice that the loss of efficiency from the use of instrumental variables turns out to be relatively small. In the second-stage regressions, the standard errors of the coefficients average only about 3 per cent higher than those of the direct least-squares regressions.[40] As it turns out, the earnings coefficients also show only a very slight increase over those in the corresponding part of Table 1. This is, of course, not entirely surprising. Since the bias due to measurement error takes the form of an attenuation of the coefficient of the "noisy" earnings variable, the mere act of suppressing the constant will, as noted earlier, automatically force that slope much closer to its "true" value. Only where the error of measurement is large, as it appears to be in 1954, or where one or more of the other explanatory variables in the equation are strongly correlated with the unobservable X^* are the effects of removing the bias likely to stand out sharply. In our present specification it happens that the other variables are not good proxies for earnings; but, as we saw in the case of the constant term and as we shall see even more strikingly in the next section, such proxies and consequent dangers of bias do enter as soon as we try to test the validity of that specification.[41]

E. *Some Further Tests of the Specification*

1. *Debt and valuation.* It will be recalled (see Sec. I. C) that under our model the value of the firm, growth aside, is equal to capitalized tax-adjusted earnings plus τ times the market value of the debt. Since the coefficient of the debt variable is thus specified to have the value τ independent of the other coefficients, we have so constrained it in all the regressions by subtracting τD from V in forming the dependent variable. For most finance specialists, however, this will be regarded as a gross misspecification. The traditional view in finance has long been that

[40] It may perhaps be worth mentioning that in terms of their ability to explain V—one measure of this ability being indicated by the relative standard errors shown in the last column of Table 3—the second-stage results here presented compare favorably with those obtained in other valuation studies such as, e.g., Gordon [7].

[41] As a further check on the earnings coefficient, and in particular to see whether our use of dividends as an instrumental variable might be introducing some distortions, we ran the second-stage regressions with $\hat{X}$ constructed from the first panel of Table 2. The earnings coefficients turned out to be almost identical to those in Table 3, but, of course, the standard errors were considerably higher. We also have run the entire set in undeflated form and again found no significant change in the coefficients.

because of the supposed nontax advantages of leverage the contribution of debt to value, given tax-adjusted earnings, will be significantly greater than τ; and that there will also be a substantial positive leverage contribution from preferred stock (whose coefficient, of course, we have in effect constrained to be zero by omitting any preferred stock variable in the value equations). A complete test of these conflicting views about the effects of leverage on valuation would be far too space-consuming for this paper. We shall, therefore (albeit somewhat regretfully), have to postpone any full-scale attempt to settle the leverage issue to a subsequent paper. Fortunately, however, there is no need for present purposes to have the most precise estimate possible of the leverage effect. At the moment, the concern is merely whether our specification is a sufficiently accurate first approximation for purposes of estimating the cost of capital; and this more narrow question can be answered by the relatively simple expedient of including debt and preferred stock among the independent variables and seeing whether they make any significant additional contribution.

The results of this test are presented in Table 4. The top panel shows the direct least-squares estimates using measured earnings. Clearly, if these results were to be taken seriously, our specification would have to be rejected. In one of the three sample years, 1954, the preferred stock coefficient is positive and one and one-half times its standard error, while the debt coefficient is positive and slightly greater than three times its standard error. Taking the three years as a whole, the average debt coefficient is about .13 and the average preferred stock coefficient about .07—values uncomfortably far from zero in the present context. Note also that including these capital structure variables completely reverses the time pattern of the key earnings coefficients with the values increasing steadily from 1954 instead of decreasing as in the second panel of Table 1 or as in Table 3.

Previous results and analysis, however, would lead us to suspect upward bias in these coefficients, particularly in 1954. If the measurement error in 1954 earnings is really as serious as it seemed to be in earlier tests, the proxying power of debt and preferred stock for their corresponding components of the true earnings stream could easily produce positive values as large or larger than the coefficients actually observed in the DLS equation. A glance at the two-stage results in the second panel of Table 4 strongly suggests that such proxying is very definitely at work in the DLS equations. Notice that all six coefficients fall in size—the fall being particularly dramatic in the case of the large debt coefficient of 1954. The average of the three debt coefficients is now only .01 and the average of the preferred stock coefficients a little over .025. The average standard error is many times as large and the coefficients themselves vary unsystematically in sign and magnitude over the

TABLE 4—VALUE AND CAPITAL STRUCTURE

Dependent Variable: $(V-\tau D)/A$

Year	Coefficients of[a]					Mult. R	Adjusted Standard Error	Ratio of Adjusted Standard Error to Mean V/A
	Earnings $(\overline{X}^{\tau}-\tau\overline{R})/A$	Size $1/A\cdot 10^7$	Growth $\overline{\Delta A}/A$	Debt D/A	Preferred Stock P/A			
	A. Direct Least-Squares Estimates							
1957	16.3 (.92)	−.274 (.08)	1.43 (.27)	−.031 (.10)	.002 (.15)	.88	.057	.052
1956	15.8 (.94)	−.133 (.08)	.800 (.25)	.098 (.11)	.026 (.14)	.88	.057	.051
1954	15.4 (1.2)	−.212 (.07)	.131 (.19)	.337 (.11)	.186 (.12)	.80	.060	.050
	B. Two-Stage Estimates[b]							
1957	16.7 (1.1)	−.278 (.08)	1.44 (.27)	−.063 (.11)	−.015 .15	.87	.058	.052
1956	15.9 (1.1)	−.132 (.08)	.806 (.26)	.085 (.13)	.021 (.14)	.87	.058	.052
1954	19.3 (1.7)	−.241 (.07)	.270 (.21)	.018 (.15)	.080 (.13)	.73	.063	.052

[a] Constant constrained to equal zero.

[b] Earnings constructed from Panel II of Table 2.

three years. We clearly, therefore, have considerable support for the proposition that we are seeing here only sampling fluctuations around a true value of zero; and that the coefficients of these variables may safely be constrained to zero in the interests of increasing the efficiency of the estimate of the key earnings coefficient.[42,43]

2. *Dividends and valuation.* The treatment of dividends so far in this paper has been predicated on the assumption that the true, as opposed to the purely informational, effect of dividends on market value is small enough in this industry to be safely ignored. By safely ignored we mean both that it would not be worth the bother to introduce payout terms in the expression for the cost of capital; and that there is no significant loss in the efficiency of estimation of the key earnings coefficient by omitting dividends entirely from the value equation. The two-stage, instrumental variable approach permits us to test these suppositions and in the process to shed some new light on the long-standing controversies in the field of finance over the role of dividends in valuation.

The dividend variable to be used in the tests will be of the form

$$\left(\frac{div}{A} - \lambda\left(\frac{\overline{X}^{\tau} - \tau\overline{R}}{A}\right)\right),$$

where λ is the sample average "payout" ratio in terms of $(\overline{X}^{\tau} - \tau\overline{R})$ for the year in question. This payout form for the dividend variable has the advantage of preserving the interpretation of the earnings coefficient as the capitalization rate for companies following an average dividend policy and thereby facilitating comparisons with previous results.[44]

[42] It should be noted, in this connection, that the debt and preferred stock coefficients are likely to be somewhat biased upward even in the second-stage regressions. The bias arises from the fact that we now have the debt and preferred variables on both sides of the equation so that any errors of measurement or of concept will be positively correlated.

[43] Once again, we checked to make sure that this result was not somehow an artifact stemming from the use of dividends as an instrumental variable by running the same test with $\hat{X}$ computed from the first panel of Table 2. The general picture was very similar to that in Table 4 with the coefficients of debt and preferred actually averaging even closer to zero. Essentially the same was also true for tests run in undeflated form.

[44] If div/A is used as the dividend variable, the earnings coefficient measures instead the effect of an increase in earnings, dividends held constant (i.e., of an increase in earnings all of which is retained). The coefficients of the two regressions

$$\frac{V - \tau D}{A} = a_1\left(\frac{\overline{X}^{\tau} - \tau\overline{R}}{A}\right) + a_2\left(\frac{div}{A}\right) + \cdots$$

and

$$\frac{V - \tau D}{A} = a_1'\left(\frac{\overline{X}^{\tau} - \tau\overline{R}}{A}\right) + a_2'\left(\frac{div}{A} - \lambda\frac{\overline{X}^{\tau} - \tau\overline{R}}{A}\right) + \cdots$$

are, of course, not independent and in fact it can easily be verified that $a_2' = a_2$, $a_1' = a_1 + \lambda a_2$, $\mathrm{Var}(a_2') = \mathrm{Var}(a_2)$, and $\mathrm{Var}(a_1') = \mathrm{Var}(a_1) + \lambda^2\,\mathrm{Var}(a_2) + 2\lambda\,\mathrm{Cov}\,(a_1, a_2)$.

Note also that the optimal value of λ to use would be the regression coefficient of dividends on earnings with all the other explanatory variables included in the regression. Since a constant

The results of the tests are presented in Table 5. The top panel shows the direct least-squares results with measured earnings as the earnings variable. Notice that the dividend coefficient is positive in all three years and very substantially so relative to its standard error and to the earnings coefficient in 1954. Findings of this kind (which are quite typical of past valuation studies) have been the main empirical support of the traditional view that high dividend payouts increase the value of the firm.[45] If our analysis is correct, however, these coefficients are biased upwards because of the additional information that dividends convey about expected future earnings beyond that contained in the imperfectly measured earnings variable.

The second-stage results in the bottom panel of Table 5 provide a test of this interpretation. Since dividends are included among the instrumental variables, the coefficient of the dividend variable in the second stage should provide an estimate of the contribution of dividends to valuation that is presumably less seriously contaminated by the mere informational effect. In this light the results are quite striking. In each of the three years the point estimate of the coefficient is now found to be negative, although of small magnitude and significance, except possibly in 1954. Thus, at least for this industry in these years the traditional view that the market valuation of shares tends to increase with the proportion of income paid out as dividends is completely unsupported.[46]

---

term is not included among these other variables, the regression coefficient can be counted on to be extremely close to the mean value of $div/\overline{X}^{\tau}-\tau\overline{R}$ and accordingly, for simplicity, we have used these means in the tests presented.

[45] Since the conventional tests in the literature involving the regression of some value variable (usually share prices) on earnings and dividends do not suppress the constant term, we reran all the DLS equations in Table 5 with constant included to see whether that would make any significant difference. It didn't.

[46] We can see, however, some possible mechanisms that might lead to a negative bias for the dividend coefficients under the two-stage approach. Consider, for example, the case of a company whose payout ratio is substantially greater than the average for the industry as a whole. Then since dividends enter the first-stage regression with substantial positive weight, the computed value of $\hat{X}$ for that company will tend to be pushed above its measured X and probably also its true X^*. Hence, in the second stage the value of V predicted from $\hat{X}$ will tend to be too high; and since this positive deviation will be associated with above-average dividends, the dividend coefficient will tend to be pushed down so as to reduce the size of the over-all residual. There will, of course, be a similar negative twist imparted to the dividend coefficient in the opposite case of payouts much below the average.

In an effort to determine how important such a bias might be we ran two kinds of supplementary tests. One was a simulation in which the true dividend effect was set at zero and in which about 10 per cent of the firms in the sample were assumed to have payout ratios widely divergent from the industry mean. The result was a very slight negative coefficient, but very far from being statistically significant. The second test consisted of extending the two-stage approach to the dividend variable itself and was motivated, of course, by the fact that the bias described above is closely akin to that occasioned by ignoring the fact that dividends are really an endogenous variable from the standpoint of the system as a whole (cf. n. 31).

TABLE 5—VALUE AND DIVIDEND POLICY

Dependent Variable: $(V-\tau D)/A$

Year	Coefficients of[a]				Mult. R	Adjusted Standard Error	Ratio of Adjusted Standard Error to Mean V/A
	Earnings $(\overline{X}^{\tau}-\tau\overline{R})/A$	Size $1/A \cdot 10^7$	Growth $\overline{\Delta A}/A$	Dividend Policy $div/A-\lambda(\overline{X}^{\tau}-\tau\overline{R})/A)$			
	A. Direct Least-Squares Estimates						
1957	16.0 (.46)	−.275 (.08)	1.41 (.24)	.915 (2.5)	.88	.057	.052
1956	16.6 (.40)	−.114 (.08)	.946 (.22)	.649 (2.3)	.87	.058	.052
1954	18.9 (.42)	−.213 (.07)	.617 (.17)	6.37 (2.1)	.79	.060	.050
	B. Two-Stage Estimates[b]						
1957	16.1 (.60)	−.281 (.08)	1.36 (.31)	− .104 (4.3)	.87	.057	.052
1956	16.8 (.50)	−.108 (.07)	.814 (.29)	−1.77 (4.0)	.86	.058	.052
1954	19.8 (.54)	−.247 (.08)	.233 (.22)	−2.19 (3.4)	.69	.067	.056

[a] Constant constrained to equal zero.
[b] Earnings constructed from Panel II of Table 2.

For present purposes of estimating the cost of capital, however, perhaps the main conclusion to be drawn from Table 5 is that the dividend effect is sufficiently small and uncertain to be safely neglected. From the statistical point of view the addition of this variable does not appear to improve the reliability of the estimates of the other coefficients, notably that of the critical earnings variable. Indeed, even the small reduction in the residual variability resulting from adding the dividend variable is more or less offset by the loss of one degree of freedom, so that on balance the standard errors of the regression coefficients actually tend to be somewhat higher. And from the substantive point of view, even if one were to accept the coefficients of the dividend variable as a reliable measure of the true dividend effect—which we would certainly hesitate to do in view of the large standard errors and the puzzling drift of the coefficients over time[47]—the implied quantitative effect of dividend policy on market valuation and the cost of capital would still be negligible. In 1954, for example, the year with the largest dividend coefficient in absolute value, the elasticity of the value of the shares at the mean with respect to dividends paid is only about -0.08. That is, a 20 per cent decrease in dividend payout—which would constitute a very large decrease in this industry, since the standard deviation of the payout ratio is about 10 per cent of the mean—would be associated with an increase in the value of the shares of only about one and one-half per cent, an amount far smaller than the relative standard error of estimate of S itself.[48] Accordingly, in the discussions of valuation and the cost of capital to follow we shall work exclusively with the results in Table 3 rather than those in Table 5.

Operationally, this meant regressing both X and dividends on the instrumental variables in the top panel of Table 2 and then regressing V on computed X and computed dividends (along with size and growth) in the second stage. The result turned out to be that in all three years the dividend coefficient was less negative than those of Table 5, and in 1957 the coefficient was actually positive and of essentially the same order of magnitude as the direct least-squares estimate. Although these results tend to confirm our suspicions about possible bias of the results in Table 5, we feel they also show that the bias is unlikely to be large enough to force us to revise our conclusion, namely, that the data rule out any substantial positive effect for dividends along the lines of the traditional literature in finance [10, esp. p. 363] or of Gordon [6] or Durand [5]. In this connection, we find it encouraging to note that other researchers are now also beginning to report that the dividend effect is actually relatively small even in the utility industry (see, e.g., Friend and Puckett in [5a] and Beranek [1a]).

[47] The only plausible rationalization for the drift that we have been able to find is that it may be reflecting the fact that the number of companies paying tax-exempt dividends (cf. n. 19) increased steadily after 1954.

[48] It may seem strange to some that we test for the effect of dividends on share prices in an equation describing total market value (including debt and preferred stocks) rather than one solely in terms of the value of the common shares. Remember, however, that under our valuation model, the variables debt and preferred stock must be included among the explanatory variables in the S relation (cf. above Sec. I.C). Since these variables are also among the

3. *Comparison with estimates from the yield formulation.* As noted earlier, an alternative way of estimating the cost of capital under our specification would be to use a "yield" equation of the form

$$\frac{\overline{X}^{\tau} - \tau\overline{R}}{V - \tau D} = a_1' + a_0' \frac{1}{V - \tau D} + a_2' \frac{\overline{\Delta A}}{V - \tau D}, \tag{18}$$

with the constant term a_1' providing the desired estimate of ρ. (Cf. Sec. II.A and n. 28.) The main drawback to this approach comes from the presence of $V - \tau D$ in the denominators of variables on both sides of the equation which imparts an upward bias to the coefficients of the independent variables and a consequent downward bias to the crucial constant term. Since the direction of the bias is known, however, we can use equations of this form to provide at least a rough check on the reasonableness of the estimates obtained by the more roundabout, two-stage approach.

The coefficients obtained in the yield equations are presented in Table 6. To facilitate comparison with the estimates in Table 3 a column has been added showing the reciprocal of the constant term, which is the estimate of the capitalization factor for earnings implied by the observed constant terms in the yield equations.[49] As predicted under our model, the capitalization factors obtained via the yield equations are indeed all higher than those obtained via the two-stage approach. The gap between the two sets of estimates tends to widen somewhat over time, but the differences are never very large. This close agreement should remove any lingering fears that major distortions in the estimates may somehow have been introduced in the two-stage approach.[50] At the same time, it suggests that the simpler yield equations may still have

instrumental variables, a second-stage S equation containing computed profits, debt, preferred and dividends would be just identified and that only by virture of the constant term included in the first stage but suppressed in the second. Since, as we have seen, the contribution of total assets as an instrumental variable is very small when dividends are included (along with debt and preferred) computed profits will be so nearly a linear combination of the other explanatory variables that no effective identification is possible. This extreme collinearity is avoided in the V tests, of course, since debt and preferred are excluded from the V equation along with the constant term. There is some possibility of success in designing dividend tests in terms of S by the use of more elaborate kinds of instrumental variables; but further discussion of these is best postponed to sequel papers. Note also that the practical necessity of excluding at least two of the instrumental variables in order to obtain adequate identification explains why we have not been able to obtain any joint tests of the various specification restrictions considered above singly.

[49] It can be shown (see Cramér [4, pp. 353–54]), that given a sample estimate $\hat{a}$ with standard error σ, the corresponding (large sample) estimate of $1/a$ is $1/\hat{a}$ and of its standard error, $\sigma/\hat{a}^2$.

[50] Initially we thought it might be possible to provide further tests of the specification by exploiting the information in the residuals of the estimating equations. For each sample

TABLE 6—DIRECT LEAST-SQUARES ESTIMATES FROM YIELD-FORM EQUATIONS
Dependent Variable: $(\overline{X}^{\tau}-\tau\overline{R})/V-\tau D$

Year	Coefficients of			Mult. R	Reciprocal of Constant Term and Its Implied Standard Error	Ratio of Standard Error of Regression to Mean of Dependent Variable
	Constant	Size $(1/V-\tau D)\cdot 10^6$	Growth $\overline{\Delta A}/V-\tau D$			
1957	.0592 (.002)	.166 (.04)	−.0516 (.02)	.58	16.8 (.44)	.07
1956	.0582 (.001)	.066 (.04)	−.0325 (.01)	.39	17.4 (.38)	.07
1954	.0506 (.001)	.121 (.04)	−.0124 (.01)	.45	19.8 (.45)	.07

a useful role to play in valuation studies, particularly where the interest is mainly in determining the direction of changes in the cost of capital over time rather than developing precise estimates or testing the basic specification as developed here.

F. *Summary*

In this section we have attempted to apply and to test the model of valuation developed in Sections I and II of this paper. Strong support has been presented for our contention that the usual direct least-squares value equations (and yield-form equations) lead to biased estimates of the key parameters. The evidence has also been seen to be consistent with our specifications with respect to the constant terms, the value of the debt coefficient and the role of dividends. Given these specifications, the most efficient, consistent estimates of the value relation are those shown in the second panel of Table 3; and it is to the economic interpretation of these results that we now turn.

year we have three residuals which can be expressed in previous notation as $u_{1t}=u_t-\alpha_t v_t$, $u_{2t}=u_t+\alpha_t w_t$, and $u_{3t}=v_t+w_t$. If, therefore, we could assume $E(u_t v_\tau)=E(u_t w_\tau)=E(v_t w_\tau)=0$ for $t, \tau=1957$, 1956, and 1954, we could obtain estimates of the variances of the specification and measurement errors for each year as well as of the interyear correlations of the specification and measurement errors. As it turns out, however, the assumption that the side correlations are zero is clearly untenable (there being particularly strong evidence of substantial negative correlation between v and w).

Although the residuals thus cannot be exploited to get directly at the specification error, the question arises as to whether they can at least be exploited to improve the efficiency of the estimates of the coefficients along the lines suggested in Zellner and Theil [23], Zellner [22], or Telser [20]. Even if there were no conceptual difficulties in applying these methods to models involving measurement error it seems very doubtful that the gain in efficiency would be worth the cost in our case because of the very high year-to-year correlation between the independent variables.

TABLE 7—SOURCES CONTRIBUTING TO THE VALUE OF THE FIRM

	Absolute Contribution to Mean V/A			Contribution as a Percentage of Mean V/A		
	1957	1956	1954	1957	1956	1954
1. Capitalized earnings on assets currently held	.758	.808	.914	68.1	72.0	75.9
2. Tax subsidy on debt	.262	.254	.285	23.5	22.6	23.7
3. Growth potential	.112	.072	.028	10.0	6.4	2.3
4. Difference between infinite size and mean size	−.019	−.008	−.021	−1.7	−0.7	−1.7
MEAN V/A	1.113	1.123	1.204	100.0	100.0	100.0

IV. *Valuation and the Cost of Capital in the Utility Industry*

A. *The Anatomy of Valuation*

In Table 7 the coefficients from Table 3 have been multiplied by the sample mean values of the variables so as to illustrate the relative importance of the various factors contributing to the market value of the typical firm in the industry. As expected, by far the largest component of market value is the capitalized earning power of the assets currently held. Next in importance is the tax subsidy to debt reflecting, of course, both the high corporate tax rate during the sample years and the very high debt ratios characteristic of this industry. Future growth potential, though small, was apparently increasing steadily in importance relative to current earning power over this period and by 1957 was accounting for something over 10 per cent of market value. The only mild surprise perhaps is the virtually negligible contribution of size to value. The difference in valuation, other things equal, between a firm of indefinitely large size and that of mean size in our sample turns out to be only on the order of 1 or 2 per cent of total market value. It should be remembered, however, that by ordinary standards, *all* the firms in our sample are extremely large.[51]

B. *The Cost of Equity Capital*

Turning now from valuation to the other side of the coin, the cost of capital, we show in Table 8 the estimates of the cost of equity capital implied by the earnings coefficients of Table 3.[52] For comparison, the table also shows two other measures of the cost or "ease of acquisition"

[51] To see whether a more direct size variable than our $1/A$ might lead to different results we reran the equations using log A as the measure of size (in the deflated equations). No significant change occurred either in the relative importance of the size effect or in the values of other coefficients.

[52] Cf. note 49.

TABLE 8—ESTIMATED COST OF EQUITY CAPITAL AND SOME ALTERNATIVE MEASURES OF EQUITY COSTS

Year	Estimated Cost of Equity Capital (ρ)		Average Earnings Yield on Shares ($\bar{\pi}^\tau/S$)		Repicrocal of Price to Book-Value Ratio (B/S)		Average Tax- and Leverage-Adjusted Total Earnings Yield $(\bar{X}^\tau - \tau\bar{R})/(V - \tau D)$	
	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954
1957	.062	122	.070	106	.64	105	.056	110
1956	.060	118	.070	106	.63	103	.056	110
1954	.051	100	.066	100	.61	100	.050	100

of equity capital frequently used by economists in investment studies, namely, the average earnings-price ratio and the reciprocal of the average price-book value ratio for the shares. Notice that all three measures indicate a rise in the cost of equity capital between 1954 and 1957, but our measure indicates a steeper and more substantial increase over the interval. The causes and implications of this apparently lesser responsiveness of the standard measures will become clear in subsequent discussion.

Insofar as levels are concerned, notice that the average earnings yield happens to be consistently higher than our estimate of the cost of equity capital. We say "happens to be" to emphasize that under our model of valuation there is no "normal" or even simple relation to be expected between the two concepts. The earnings yield for any company is not a given fixed number for each member of the class, as so much of the traditional discussion in finance and economics would suggest, but rather a function whose arguments include the cost of equity capital for the class, the firm's growth potential, its leverage policy, and its size. The sample mean earnings yield shows only the combined effect of these different and to some extent offsetting influences.

The precise way in which the various components of the yield fit together in the present sample is illustrated in Table 9. The entries have been computed by "solving" our fitted V equations for their implied $\bar{\pi}^\tau/S$ functions, and then substituting the appropriate sample mean values for the variables into these derived functions. Specifically, if the V equation (neglecting the deflator) is represented as

$$V - \tau D = -a_0 + a_1(\bar{X}^\tau - \tau\bar{R}) + a_2\overline{\Delta A}; \tag{19}$$

then, making use of the identities $V \equiv S + D + P$ and $\bar{X}^\tau = \bar{\pi}^\tau + \overline{Pdv} + \bar{R}$, we have as the implied function for the value of the shares (cf. II. C):

$$S = -a_0 + a_1\bar{\pi}^\tau - (1-\tau)[D - a_1\bar{R}] - [P - a_1\overline{Pdv}] + a_2\overline{\Delta A}, \tag{20}$$

and as the implied earnings yield function:

$$\frac{\bar{\pi}^\tau}{S} = \frac{1}{a_1} - \frac{\left(\frac{a_2}{a_1}\right)\overline{\Delta A}}{S} + \frac{(1-\tau)\left(\frac{D}{a_1} - \bar{R}\right)}{S} + \frac{\left(\frac{P}{a_1} - \overline{Pdv}\right)}{S} + \frac{\left(\frac{a_0}{a_1}\right)}{S}. \tag{21}$$

The first term of (21) is the cost of equity capital in our sense for the class as a whole; the second is the growth effect; the third and fourth, the leverage effects for debt and preferred stock, respectively; and the last term is the size effect. The presence of leverage, by raising the "risk" attaching to the shares, drives up the expected earnings yield relative to the cost of equity capital. The existence of growth potential with its implied prospect of future capital gains tends to reduce the current expected earnings yield on the shares.

As can be seen from Table 9, the leverage effect was the dominant one over the period as a whole which, of course, explains why the earnings yield consistently overstated the cost of equity capital during this period. The substantial narrowing of the gap between 1954 and 1957 reflects the large rise in the growth contribution after 1954 both in absolute and relative terms.

C. *Valuation, Growth, and the Cost of Equity Capital*

Although the conventional earnings/price ratio is thus distorted as a measure of the cost of equity capital both by the growth effect and the

Table 9—Relation Between Average Earnings Yield and the Cost of Equity Capital

	1957		1956		1954	
Average Earnings Yield		.070		.070		.066
Less:						
Leverage Effect		.019		.017		.014
Debt	.016		.014		.012	
Preferred Stock	.003		.003		.002	
Size Effect		.002		.001		.002
Plus:						
Growth Effect		.014		.008		.003
Total		.063		.060		.053
Estimated Cost of Equity Capital		.062		.060		.051

leverage effect, it is important to remember that the real culprit is the growth effect. That is, it is the growth effect and not the leverage effect that keeps us from being able to infer the cost of equity capital directly from market yields. We know, for example, that if there were no growth and no taxes the fundamental valuation equation, expressed in yield form, would be simply $\bar{X}/V=\rho$. The ratio of expected total earnings to total market value—which may be thought of as a "leverage-corrected" yield—would thus provide a direct estimate of ρ. In principle, any firm could so approximate its cost of equity capital from its own company data, although, of course, as a practical matter, a better estimate would be obtained by averaging over a large group of similar firms so as to wash out any random noise in $\bar{X}$ or V. When we allow for taxes and the consequent tax subsidy on debt, the picture becomes slightly more complicated, but a direct approximation of ρ still exists. The appropriate yield [cf. (6) above] now becomes $(\bar{X}^{\tau}-\tau\bar{R})/(V-\tau D)$—the ratio of total tax-adjusted earnings to total market value minus the value of the tax subsidy.

This method of direct approximation breaks down, however, in the presence of growth. The leverage-adjusted yield will be systematically too low as an estimate of ρ for any company with growth potential, as will be the group average yield for any sample that contains significant numbers of growth companies. Nor will the movements of the yield over time conform well with changes in ρ to the extent that the market's evaluation of future growth potential changes over time (and, of course, much of the short-term variation we see in share prices stems precisely from this source). Some idea of how sizable the distortions of level and movement of the yield relative to ρ can be—even in such a low-growth industry as our electric utilities and even over such a short span of time—can be gained by comparing our estimates of the cost of equity capital in the first column of Table 8 with those of the tax- and leverage-adjusted yield, $[(\bar{X}^{\tau}-\tau\bar{R})/(V-\tau D)]$, in the last column of that table.

One somewhat surprising aspect of this comparison that is perhaps worth some special mention is the relative stability of the yield series over this period. Because of the many uncertainties surrounding estimates of future growth potential and because of the sensitivity of current market values to even small changes in projected growth rates one would expect the growth component in the denominator of the yield ratio to be quite volatile; and hence that the market yield would tend to swing quite substantially in response to these continuing re-evaluations. Some idea of why this "normal" pattern did not obtain during our sample period can be gained from Figure 1. The solid line functions there plotted are the basic value regressions of Table 3 for the beginning and ending years, 1954 and 1957, expressed in ratio form as

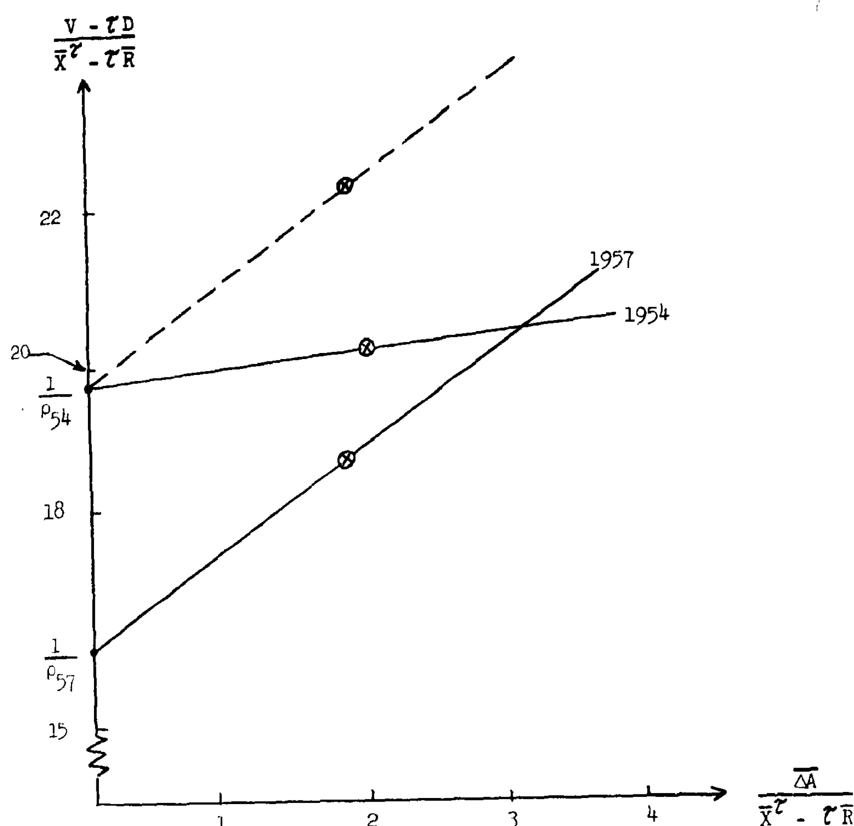


FIGURE 1

$$\frac{V - \tau D}{\overline{X}^{\tau} - \tau\overline{R}} = a_1 + a_2 \frac{\overline{\Delta A}}{\overline{X}^{\tau} - \tau\overline{R}}$$

(and hence ignoring the minor size effect). The dependent variable is thus the reciprocal of the tax- and leverage-adjusted yield; the intercept a_1 is our estimate of $1/\rho$; and the slope a_2 is the coefficient of growth in the V equation.[53]

Notice that in 1954, at the beginning of the period, the market's estimate of the growth potential of the industry was quite low. Because the slope was so flat, the approximate sample mean value of $(V-\tau D)/(\overline{X}^{\tau}-\tau\overline{R})$—indicated by the circled cross—differed only very slightly from the estimate of $1/\rho$ implied by the intercept. By 1957, however, a striking increase had taken place in the market's valuation of future

[53] The use of the reciprocal of the yield rather than the yield itself is simply a matter of convenience since the presence of growth impounded in V would lead to a nonlinear relation between $(\overline{X}^{\tau}-\tau\overline{R})/(V-\tau D)$ and growth as measured by $\overline{\Delta A}/(V-\tau D)$.

growth in the industry. As can be seen from the broken line—which has been plotted with 1954 intercept and 1957 slope—this large revaluation would have pushed the average value of $(V-\tau D)/(\overline{X}^{\tau}-\tau\overline{R})$ up by nearly 15 per cent to 22.4 (equivalent to a yield of about .044) if no other changes had occurred. But instead of this "pivoting" around a stable intercept of $1/\rho_{54}$, our estimates indicate that there happened to take place a simultaneous and quite substantial drop in the intercept (i.e., rise in the cost of equity capital). So substantial, in fact (when combined with the slight fall in the mean value of the growth variable itself), that the upward push of the revaluation of growth was more than offset; and the mean value of $(V-\tau D)/(\overline{X}^{\tau}-\tau\overline{R})$ actually fell by about 10 per cent.

Although these compensating movements in ρ and the market's evaluation of growth "explain" the relative stability of the tax- and leverage-adjusted yield during the sample period, the explanation may strike the reader as having a somewhat paradoxical flavor. Growth potential, after all, is the opportunity to invest in the future in projects whose rates of return exceed the cost of capital. One would expect, therefore, that a rise in the cost of capital would normally be associated with a *fall* in growth potential. There are a number of possible explanations for the opposite behavior in the present instance, but discussion of them is perhaps best postponed until we have first provided estimates of the average cost of capital relevant for investment decisions.

D. *The Required Yield or Average Cost of Capital*

As emphasized earlier, the relevant cost of capital for investment decisions at the level of the firm is the average cost of capital, $\rho(1-\tau L)$, where L measures the "target" proportion of debt in future financing. The average cost is thus not a fixed number, but a schedule or function whose arguments are ρ (which is an "external" property of the class or industry determined by the market) and L (which is a matter of "internal" company policy). Although the average cost of capital, unlike the cost of equity capital, is thus in principle different for each firm in the industry, we can get some idea of its value and behavior for the typical electric utility by using a typical or average value for L. The obvious candidate, of course, is the actual sample average of D/A for each year, since D/A measures the average proportion of debt in past financing, and this proportion is likely to be quite stable (particularly when averaged over the industry). Estimates with these values for L are shown in block 1 of Table 10. (N.B. For notational convenience we shall hereafter refer to these estimates as $C(\overline{D/A})$, using $C(L)$ to mean the function itself, and $C(D/A)$, i.e., without the bar on D/A, to refer to

TABLE 10—THE AVERAGE COST OF CAPITAL AND SOME COMPARISON SERIES

Year	Average Cost of Capital $C(\overline{D/A})$		Yields on AAA Public Utility Bonds[a]		Sample Mean Yield on Debt (R/D)		Sample Mean Yield on Preferred Stock (Pdv/P)		Weighted Average Yields: With Book-Value Weights $\bar{C}_B(\overline{D/A})$		Weighted Average Yields: With Market-Value Weights $\bar{C}_M(\overline{D/A})$		Growth Adjusted Average Yield[b] $(\bar{X}^\tau - \tau\bar{R})/V - G$	
	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954	Amt.	As per cent of 1954
1957	.046	128	.043	137	.029	112	.047	115	.034	104	.042	108	.047	122
1956	.045	125	.039	126	.029	112	.046	112	.035	106	.043	110	.045	115
1954	.036	100	.031	100	.026	100	.041	100	.033	100	.039	100	.039	100

[a] Monthly average for December. *Source: Federal Reserve Bulletin.*
[b] With G equal to growth coefficient in Table 3, Panel 2, times sample mean value of $\overline{\Delta A}/A$.

the function evaluated not at the industry mean but at a particular company's value of D/A.)

These estimates $C(\overline{D/A})$ of the average cost of capital are, of course, always below the corresponding estimates of ρ (column 1 of Table 8); but the movements over time of the two series are closely similar since, as expected, the sample mean value of D/A is quite stable.[54] Notice also that the estimates of ρ and hence of $C(\overline{D/A})$ conform quite closely in their movements with the average yield on AAA bonds in the industry (block 2 of Table 10)—probably the most popular surrogate for the cost of capital in investment studies. This conformity is particularly interesting since the rate of interest on bonds enters only very indirectly into our calculations of ρ and $C(\overline{D/A})$ and, as can be seen from block 3, the implied average rate of interest in our sample does not even seem to conform well with the AAA series.[55] From the economic point of view, this parallelism between movements in ρ and the AAA yields would seem to suggest that over this short interval at least the movements of both series were dominated by factors affecting the supply of and demand for capital generally. Changes, if any, in investors' tastes for risk-bearing or in their evaluation of the riskiness of this industry in relation to others were apparently not large enough to cause any significant divergence of movement in the period under study.

It is also instructive to contrast our estimates of the average cost of capital with those that would be obtained by following the prescriptions laid down in much of the traditional literature of corporation finance. Essentially, these call for computing the weighted sum of the market yields of each type of security, the weights being the "target" proportions of each security in the capital structure. That is, if we let i=the earnings/price ratio (our $\bar{\pi}^{\tau}/S$); p=the preferred dividend yield (our $\overline{Pdv}/P$); r=the average rate of interest on bonds (our $\overline{R}/D$); l=the target debt ratio; and l'=the target preferred ratio then the weighted average cost of capital function under the traditional view can be expressed as $i(1-l-l')+p(l')+r(1-\tau)(l)$. Where the target weights l and l' are computed at book value as is usually recommended (i.e., with $l=D/A$ and $l'=P/A$ and $(1-l-l')=B/A$ in our notation) we shall refer to the resulting average as $\overline{C}_B(D/A)$; where they are taken at market value (i.e., with $l=D/V$ and $l'=P/V$ and $1-l-l'=S/V$) we

[54] In interpreting these estimates of the average cost of capital it may be worthwhile to remind readers that the figures in column 1 represent *tax-adjusted* yields (cf. notes 8 and 11) and are not adjusted for size (i.e., they would apply, strictly speaking, only to firms of infinite size). Adjusting to the average size of firm in our sample and converting to the more familiar *before-tax* basis, the corresponding average costs would be .073, .087 and .090, respectively, for 1954, 1956, and 1957.

[55] One important reason for the divergence between market rates on bonds and our implied average yields is the fact that our debt variable in the denominator contains substantial and varying amounts of short-term liabilities.

shall refer to the average as $\bar{C}_M(D/V)$, with unbarred values of the argument standing as before for a single company value and barred values for industry means. Estimates of both $\bar{C}_B(\overline{D/V})$ and $\bar{C}_M(\overline{D/A})$ for the typical firm in the sample, using actual sample mean values of i, p, and r, as well as of the book- and market-value measures of l and l' in each case, are shown in blocks 5 and 6 of Table 10.[56]

As can be seen from Table 10, both the levels and the time paths of $\bar{C}_B(\overline{D/A})$ and $\bar{C}_M(\overline{D/V})$ differ significantly from those of $C(\overline{D/A})$. The largest discrepancies arise in the case of the widely used $\bar{C}_B(\overline{D/A})$ measure which is substantially below $C(\overline{D/A})$ in all three years and which shows only a very slight rise over the period. The market value estimates, $\bar{C}_M(\overline{D/V})$ are considerably closer to those of $C(\overline{D/A})$, but they too fail to indicate the sizable increase in the cost of capital which seems to have occurred during this period.

E. *Reconciliation with Conventional Averages*

To understand precisely why these three methods of estimating the average cost of capital gave such different answers for the years under study (and why they are likely to continue to diverge for other years and other industries), it is helpful to begin by showing how these estimates would relate to each other in a much simpler world in which no growth potential ever existed. In such a world, we have seen that the ratio $(\bar{X}^\tau - \tau\bar{R})/(V - \tau D)$—the tax- and leverage-adjusted yield of the previous section—would be a measure of our ρ, the cost of equity capital. Hence, from the standpoint of any individual firm, the $C(L)$ function can be expressed as

$$(22) \qquad C(L) = \frac{\bar{X}^\tau - \tau\bar{R}}{V - \tau D}(1 - \tau L) = \frac{\bar{X}^\tau - \tau\bar{R}}{V}\cdot\frac{1 - \tau L}{1 - \tau D/V}\,.$$

[56] An alternative way of computing industry averages would be not to average the product of the mean yield times the mean weight as was done in Table 10, but to average the product of the yield and weight for each company separately. In the case of $\bar{C}_M(D/V)$ this leads to the very simple relation

$$\bar{C}'_M(\overline{D/V}) = \left(\overline{\frac{\bar{\pi}^\tau}{S}\cdot\frac{S}{V}}\right) + \left(\overline{\frac{\overline{Pd\nu}}{P}\cdot\frac{P}{V}}\right) + \left(\overline{\frac{\bar{R}(1-\tau)}{D}\cdot\frac{D}{V}}\right)$$

$$= \left(\overline{\frac{\bar{\pi}^\tau}{V}}\right) + \left(\overline{\frac{\overline{Pd\nu}}{V}}\right) + \left(\overline{\frac{\bar{R}(1-\tau)}{V}}\right) = \left(\overline{\frac{\bar{X}^\tau - \tau\bar{R}}{V}}\right).$$

The sample values of the latter yield are .038, .043, and .042, respectively, for 1954, 1956, and 1957. The (slight) difference between the two estimates arises, of course, from the fact that the company values of $\bar{\pi}^\tau/S$ are not independent of the values of S/V, i.e., of the company's degree of leverage.

Note also, for simplicity, we shall here and hereafter ignore the small difference between the book value and the market value of debt and preferred, using the market values in all cases.

The weighted average cost of capital function with market-value weights is

$$(23)\qquad \bar{C}_M(D/V) = \frac{\bar{\pi}^\tau}{S}\cdot\frac{S}{V} + \frac{\overline{Pdv}}{P}\cdot\frac{P}{V} + \frac{\bar{R}(1-\tau)}{D}\cdot\frac{D}{V} = \frac{\bar{X}^\tau - \tau\bar{R}}{V}$$

and with the book-value weights,

$$(24)\qquad \bar{C}_B(D/A) = \frac{\bar{\pi}^\tau}{S}\cdot\frac{B}{A} + \frac{\overline{Pdv}}{P}\cdot\frac{P}{A} + \frac{\bar{R}(1-\tau)}{D}\cdot\frac{D}{A}.$$

Notice first that for the special case of $L=D/V$—i.e., when the target leverage coincides with current leverage at market value, D/V, the function $C(L)$ takes the value

$$C(D/V) = \frac{\bar{X}^\tau - \tau\bar{R}}{V} = \bar{C}_M(D/V).$$

In other words, if a firm's current and future target leverage is D/V it will get precisely the same estimate for its average cost of capital regardless of whether it chooses to multiply its current tax- and leverage-adjusted yield by $[1-\tau(D/V)]$; or to compute the weighted average of the current yields of its outstanding securities, with market-value weights for each; or if it simply uses the ratio of expected tax-adjusted earnings to total market value. A similar equivalence of estimates (at least to a very close degree of approximation) would also hold, of course, for economists concerned with "typical" values for the industry and using industry mean values of D/V and of the various yields, i.e., $C(\overline{D/V})\cong\bar{C}_M(\overline{D/V})$.[67] Note also that if $V=A$—which would tend to be the case if there were no growth past or future—then $\bar{C}_B(D/A)$ becomes the same function as $\bar{C}_M(D/V)$ and, by extension, as $C(L)$. In this special case of no growth, therefore, all three company and industry-wide estimates will coincide.

This simple picture changes quite drastically, however, as soon as growth potential is introduced. The function $C(L)$ must now be expressed as

[67] Although the equivalence holds for individual company data and for industry averages, there is one important case in which the equivalence very definitely does *not* hold. This is the common case of the firm following the weighted average approach of (23) with current (or prospective future) company weights, but using industry-wide averages of the component yields so as to obtain less noisy estimates. The trouble here is that the market yield on shares (and to some extent the yields on preferred and bonds as well) are increasing functions of leverage. Hence, for a firm whose target leverage is greater (smaller) than the average for the industry the industry mean yield will be an underestimate (overestimate) of its own yield and the resulting average cost of capital will be too low (high). This problem does not arise under our (22), of course, since $(\bar{X}^\tau-\tau\bar{R})/(V-\tau D)$ is not a function of firm policy (as is $\bar{\pi}^\tau/S$), but an estimate of the external, market-given parameter, ρ.

$$(25)\quad C(L) = \frac{\overline{X}^\tau - \tau\overline{R}}{V - \tau D - G}(1 - \tau L) = \frac{\overline{X}^\tau - \tau\overline{R}}{V - G} \cdot \frac{1 - \tau L}{1 - \tau\left(\frac{D}{V - G}\right)},$$

where G = the market's current valuation of future growth potential. Hence, as can be seen by reference back to (23), there no longer exists any concept of L for which the function $C(L)$ will be the same as $\overline{C}_M(D/V)$. Note also that in the special case in which future growth potential constitutes the only major source of divergence between V and A, $(\overline{D/A}) \cong [\overline{D/(V-G)}]$, so that our estimates of the average cost of capital $C(\overline{D/A})$ would be closely approximated by the ratio $[(\overline{X}^\tau - \tau\overline{R})/(V-G)]$. The actual sample mean values of that ratio (with G taken as the product of the growth coefficient in Table 3 and the mean value of our growth variable $\Delta A/A$) are shown in the last block of Table 10. As can be seen, the approximation to $C(\overline{D/A})$ is indeed quite close in 1956 and 1957; but it is less satisfactory in 1954 where the growth contribution is small both in absolute terms and relative to the other sources of divergence between V and A.

Where the ratio $[(\overline{X}^\tau - \tau\overline{R})/(V-G)]$ is a good approximation to $C(\overline{D/A})$, it will, of course, also follow that both measures will exceed $\overline{C}_M(\overline{D/V})$, which, as we saw above, is given approximately by $[(\overline{X}^\tau - \tau\overline{R})/V]$. As for the relation between the popular $\overline{C}_B(\overline{D/A})$ and $C(\overline{D/A})$ note that we can express the ratio $[(\overline{X}^\tau - \tau\overline{R})/(V-G)]$ approximately as

$$(26)\quad \left(\frac{\overline{X}^\tau - \tau\overline{R}}{V - G}\right) = \left(\frac{\overline{\pi}^\tau}{S - G} \cdot \frac{S - G}{V - G}\right) + \left(\frac{Pdv}{P} \cdot \frac{P}{V - G}\right) + \left(\frac{\overline{R}(1 - \tau)}{D} \cdot \frac{D}{V - G}\right) \cong \left(\frac{\overline{\pi}^\tau}{S - G}\right)\left(\frac{\overline{B}}{A}\right) + \left(\frac{\overline{\overline{Pdv}}}{P}\right)\left(\frac{\overline{P}}{A}\right) + \left(\frac{\overline{R}(1 - \tau)}{D}\right)\left(\frac{\overline{D}}{A}\right),$$

since the assumption $V - G \cong A$ implies

$$\frac{D}{V - G} \cong \frac{D}{A}, \quad \frac{P}{V - G} \cong \frac{P}{A} \quad \text{and} \quad \frac{S - G}{V - G} \cong \frac{B}{A}.$$

Comparison with (24) shows that the weights in the two expressions are essentially the same; but since $(\overline{\pi}^\tau/S) < [\overline{\pi}^\tau/(S-G)]$, $\overline{C}_B(\overline{D/A})$ too will necessarily fall short of $C(\overline{D/A})$ when growth is present, and the gap will be larger, the larger is the contribution of growth to the value of the shares.

Once again, then, we see that attempts to infer the cost of capita directly from market yields rather than by the more detailed, cros sectional estimating procedures developed in this paper break down i the face of growth. Where growth is present all of the popular, short-cu approximations will underestimate the cost of capital; and, where th market changes its evaluation of growth potential over time (as i inevitable in view of the nature of growth) the time path of the yiel measures may give a quite misleading picture of the true changes in th cost of capital. In particular, in our sample it happens that the market' evaluation of growth increased substantially over the period, thereb causing the yield measures to understate seriously the rise in capita costs that appears to have been taking place at the same time. As note earlier, it is somewhat paradoxical that these two changes should hav occurred simultaneously, since an increase in the cost of capital shoul tend to reduce what the market is willing to pay for given investmen opportunities. We can perhaps throw some light on this paradox b taking a closer look at our growth coefficients and their implicit com ponents.

F. *A Further Analysis of the Valuation of Growth*

As noted earlier (cf. Sec. II.E above) the growth term in our basi valuation equation is of the form

$$k\bar{X}(1-\tau)\left[\frac{\rho^*-C}{C(1+C)}\right]T,$$

where k is the ratio of investment to tax-adjusted earnings; $C=$ th average cost of capital; $\rho^*=$ the tax-adjusted rate of return on ne investment; and T is a measure of the length of time for which th opportunities to invest at the rate ρ^* are expected to last. In the actua estimating equations we have taken as our growth variable an estimat of $k\bar{X}(1-\tau)$, the level of future investment opportunities. Hence, if on accepts the underlying model, the observed coefficients of the growt variable can be interpreted as an approximation to

$$[(\rho^*-C)/(C(1+C))]T.$$

Now that we have estimates of C, the average cost of capital for typical firm, we can attempt some further decomposition of these growt coefficients.

In particular, it should be possible from what we know about pas earnings and about the regulatory process governing earnings in th industry to make at least a rough approximation of ρ^*.

An obvious first candidate as an approximation to ρ^* is, of course, th current, tax-adjusted rate of return on assets, $(\bar{X}^{\tau}-\tau\bar{R})/A$. Such

TABLE 11—ANALYSIS OF THE GROWTH EFFECT

Year	Growth Coefficient[a] (G)	Average Cost of Capital[b] $C(\overline{D/A})$	Average Tax-Adjusted Return on Assets (ρ_1^*)	Average Return Assuming 6 per cent Return after Taxes (ρ_2^*)	Implied Value of T^c (in years) For ρ_1^*	For ρ_2^*
1957	1.36	.046	.047	.052	51	11
1956	.90	.045	.048	.052	12	6
1954	.30	.036	.046	.052	1	1

[a] From Table 3, Panel 2.
[b] From Table 10, Column 1.
[c] Computed as $G((C)(1+C)/(\rho^*-C))$.

measure, however, is almost certainly an underestimate of ρ^*, since we know that there are components of total assets—actually, of total liabilities—that regulatory commissions systematically exclude from the rate base.[58] Some idea of the extent of this underestimate is provided by the knowledge that during these years most of the state commissions were still setting the "reasonable return on the rate base" in the near neighborhood of the classical 6 per cent. By contrast, the sample average values of $\overline{X}^\tau/A$—$\overline{X}^\tau$, rather than $(\overline{X}^\tau-\tau\overline{R})$, being the relevant earnings concept in rate-setting—were only .054, .056, and .055 per cent in 1954, 1956, and 1957, respectively. One simple adjustment, therefore, would be to blow up each sample mean value of $(\overline{X}^\tau-\tau\overline{R})/A$ by the ratio of 6 per cent to the sample mean value of $\overline{X}^\tau/A$. The rates of return so adjusted, as well as the original unadjusted rates of return, are presented in Table 11 along with the estimates of T they imply.

These results would seem to suggest the following as the resolution of the paradox described in the previous section. The observed rise in the markets' valuation of the industry's growth potential, in the face of the sharp rise in the cost of capital during the period, cannot reasonably be attributed to any compensating increase in the expected rate of return on future investment. No sharp upward trend in earnings rates, adjusted or unadjusted, is visible in the data; nor would such a trend be

[58] A further word of caution is necessary because the so-called accounting rate of return (earnings after depreciation divided by net assets) is not the same as the ordinary internal rate of return when a firm is growing. This discrepancy does not seem likely to create any very serious problems insofar as the valuation equations or estimates of the cost of capital are concerned since in those equations assets appear only as a deflator and since an explicit growth variable is included. It may, however, raise difficulties for comparisons of the kind being attempted here. We say may, because the ρ^* in our formula is not the usual internal rate of return, but the so-called "perpetual rate of return" (see [14, p. 416]), and the relations between that rate and the accounting rate of return have, to our knowledge, nowhere yet been explored. We are indebted to Sidney Davidson and Robert Williamson for some helpful discussions on this general point.

expected in view of the regulatory controls over the level of earnings What seems to have been happening rather is that early in the perioc investors came to recognize that the regulatory authorities were setting rates at levels that would probably permit firms in the industry to earn significantly more than the cost of capital on any new capital invested The subsequent rise in the cost of capital narrowed the margin of gain somewhat; but its effects on valuation were more than offset by an increase in the length of time that favorable terms for new investment were expected to persist. The actual numerical estimates of this time horizon, as presented in Table 11, are not, of course, to be taken seriously in view of the many approximations, theoretical and empirical involved in their computation. But a general expansion of horizons (or at least an increasing awareness of growth potential on the part of investors) is very definitely indicated.

Conclusion

This has been a long and necessarily somewhat tedious paper, and we do not propose to add either to the length or the tedium by an extensive summary at this point. Instead, we shall simply close by indicating briefly what we believe to be the main conclusions that can be drawn from the material presented. These are:

1. With respect to the methodology of empirical valuation and cost of capital studies, we feel we have demonstrated that the two-stage instrumental variable approach developed here can be an effective way of dealing with the problems caused by errors of measurement of expected future earnings—problems that bear a major share of the responsibility for the meager progress in empirical research on this front to date. We can also report some success in our attempt to provide an explicit measure of growth potential for the utility companies in our sample, but, clearly, more general methods of dealing with this crucial variable are urgently needed before substantial further progress on the cost of capital problem can be made.

2. With respect to the theory of valuation, we found that the rational-behavior, perfect-market model of valuation under uncertainty stands up quite well when confronted with the data both in terms of what it says should be included, and what it says should be excluded. In particular, for the utility industry in these years we found no evidence of sizable leverage or dividend effects of the kind assumed in much of the traditional literature of finance.

3. As for our estimates of the cost of equity capital and of the average cost of capital we showed that they differ very considerably both in level and movement during the sample period from the conventional

kinds of "yield" estimates so widely used in economics and finance. On the other hand, our estimates for the utility industry do seem to conform reasonably closely over the (short) sample period with movements in the long-term rate of interest on bonds. It will be interesting to see whether this relation persists in other industries and over longer and less placid intervals of time.

Appendix A

Companies in the Sample

Atlantic City Electric Co.
Baltimore Gas & Electric Co.
Boston Edison Co.
Carolina Power and Light
Central Illinois Light
Central Illinois Public Service
Cincinnati Gas & Electric Co.
Cleveland Electric Illuminating
Columbus & Southern Ohio Electric Co.
Commonwealth Edison
Community Public Service
Consolidated Edison of N. Y.
Consumers Power
Dayton Power & Light
Delaware Power and Light
Duke Power
Duquesne Light
Florida Power Corp.
Florida Power and Light Co.
Gulf States Utilities
Public Service Electric & Gas Co.
Idaho Power Co.
Illinois Power Co.
Indianapolis Power & Light
Interstate Power Co.
Iowa Illinois Gas & Electric
Iowa Power & Light
Kansas City Power & Light Co.
Kansas Gas & Electric Co.
Kansas Power & Light
Long Island Lighting
Louisville Gas & Electric
Maine Public Service
Minnesota Power & Light
Missouri Public Service
Montana-Dakota Utilities Co.
New York State Electric & Gas Corp.
Niagara Mohawk Power Corp.
Ohio Edison
Oklahoma Gas & Electric
Pacific Gas & Electric
Pennsylvania Power & Light
Public Service Co. of Colorado
Puget Sound Power & Light
Rochester Gas and Electric Co.
St. Joseph Light & Power Co.
San Diego Gas & Electric
South Carolina Electric & Gas Co.
Southern Indiana Gas & Electric
Toledo Edison
United Gas Improvement Co.
Utah Power & Light
Virginia Electric & Power Co.
Washington Water Power Co.
Wisconsin Electric Power Co.
Central Hudson Gas & Electric
Detroit Edison
Empire District Electric Co.
Houston Lighting & Power Co.
Potomac Electric Power Co.
Public Service Co. (Indiana)
Southern California Edison Co.
Philadelphia Electric Co.

APPENDIX B

1. *Means and Standard Deviations of the Principal Variables*

Variable	1957	1956	1954
$\frac{V - \tau D}{A}$	.851 (.12)	.869 (.11)	.919 (.093)
$\frac{\bar{X}^{\tau} - \tau\bar{R}}{A}$	.0471 (.006)	.0484 (.006)	.0464 (.006)
$1/A \cdot 10^{8}$	.670 (.91)	.739 (.98)	.854 (1.1)
$\frac{\overline{\Delta A}}{A}$	.082 (.030)	.0799 (.032)	.095 (.045)
D/A	.504 (.051)	.488 (.049)	.548 (.050)
P/A	.100 (.047)	.109 (.052)	.126 (.062)
$\frac{div}{A}$	.0243 (.004)	.0249 (.004)	.0241 (.004)

(*Appendix B continued on next page*)

2. *Matrix of Simple Correlation Coefficients*

	$\frac{V-\tau D}{A}$	$\frac{\bar{X}^{\tau}-\tau\bar{R}}{A}$	$\frac{1}{A}\cdot 10^8$	$\frac{\overline{\Delta A}}{A}$	$\frac{D}{A}$	$\frac{P}{A}$	$\frac{div}{A}$
1957							
1. $\frac{V-\tau D}{A}$	1.00	.775	−.171	.365	−.068	−.075	.431
2. $\frac{\bar{X}^{\tau}-\tau\bar{R}}{A}$		1.00	.094	−.000	−.208	−.067	.656
3. $\frac{1}{A}\cdot 10^8$			1.00	−.092	.049	−.088	.040
4. $\frac{\overline{\Delta A}}{A}$				1.00	.457	−.100	−.277
5. D/A					1.00	−.374	−.255
6. P/A						1.00	−.350
7. div/A							1.00
1956							
1.	1.00	.833	−.162	.179	.014	−.022	.432
2.		1.00	−.040	−.083	−.017	−.077	.630
3.			1.00	−.106	.249	−.078	.104
4.				1.00	.344	.170	−.384
5.					1.00	−.288	−.199
6.						1.00	−.442
7.							1.00
1954							
1.	1.00	.733	−.106	.190	.083	.110	.498
2.		1.00	.181	.003	.060	.024	.554
3.			1.00	−.170	.049	−.061	.185
4.				1.00	.496	.152	−.290
5.					1.00	−.184	−.155
6.						1.00	−.331
7.							1.00

REFERENCES

1. A. BARGES, *The Effect of Capital Structure on the Cost of Capital.* Engle wood Cliffs, N.J. 1963.
1a. W. BERANEK, *The Effect of Leverage on the Market Value of Commo Stock,* Wisconsin Commerce Reports, Vol. VII, No. 2, Bur. Bus. Re search and Service, Univ. Wisconsin, Madison 1965.
2. H. BIERMAN AND S. SMIDT, *The Capital Budgeting Decision.* New Yor 1960.
3. G. CHOW, *The Demand for Automobiles in the United States.* Amsterdar 1957.
4. H. CRAMÉR, *Mathematical Methods of Statistics.* Princeton 1946.
5. D. DURAND, *Bank Stock Prices and the Bank Capital Problem,* Na Bur. Econ. Research Occas. Paper 54. New York 1954.
5a. I. FRIEND AND M. PUCKETT, "Dividends and Stock Prices," *Am. Eco Rev.,* Sept. 1964, *54,* 656–82.
6. M. GORDON, "Dividends, Earnings and Stock Prices," *Rev. Econ. Stat* May 1959, *41,* 99–105.
7. ———, *The Investment, Financing and Valuation of the Corporation* Homewood, Ill. 1962.
8. J. HIRSHLEIFER, "Investment Decision Under Uncertainty" (multi lith), April 1964.
9. ———, "On the Theory of Optimal Investment Decision," *Jour. Po Econ.,* Aug. 1958, *66,* 329–52.
10. P. HUNT, C. WILLIAMS, AND G. DONALDSON, *Basic Business Finance* Homewood, Ill. 1958
10a. B. KING, *The Latent Structure of Security Prices.* Unpublished doctora dissertation, Univ. Chicago, 1964.
11. J. LINTNER, "Dividends, Earnings, Leverage, Stock Prices and the Sup ply of Capital to Corporations," *Rev. Econ. Stat.,* Aug. 1962, *44,* 243-69.
12 ———, "Optimal Dividends and Corporate Growth Under Uncer tainty," *Quart. Jour. Econ.,* Feb. 1964, *78,* 49-95.
13. A. MADANSKY, "On the Efficiency of Three-Stage Least-Squares Esti mation," Econometrica, Jan.–April 1964, *32,* 51–56.
14. M. MILLER AND F. MODIGLIANI, "Dividend Policy, Growth and the Valuation of Shares," *Jour. Business,* Oct. 1961, *34,* 411–33.
15. F. MODIGLIANI AND M. MILLER, "The Cost of Capital, Corporation Finance, and the Theory of Investment," *Am. Econ. Rev.,* June 1958 *48,* 261–97.
16. ——— and ———, "The Cost of Capital, Corporation Finance, and the Theory of Investment: Reply," *Am. Econ. Rev.,* Sept. 1959, *49* 655–69.
17. ——— and ———, "Corporate Income Taxes and the Cost of Capital A Correction," *Am. Econ. Rev.,* June 1963, *53,* 433–43.
18. ——— and ———, "The Cost of Capital, Corporation Finance, and the Theory of Investment: Reply," *Am. Econ. Rev.,* June 1965, *55,* 524–27.

19. S. NEILSEN, *Market Value and Financial Structure in the Railroad Industry*, Travellers Ins. Co. Occas. Paper 4. Hartford, Conn., March 1961.
20. L. TELSER, "Iterative Estimation of a Set of Linear Regression Equations," *Jour. Am. Stat. Assoc.*, Sept. 1964, *59*, 845–62.
21. H. THEIL, *Economic Forecasts and Policy*, 2nd rev. ed. Amsterdam 1961.
22. A. ZELLNER, "An Efficient Method of Estimating Seemingly Unrelated Regressions and Tests for Aggregation Bias," *Jour. Am. Stat. Assoc.*, June 1962, *57*, 348–68.
23. ——— AND H. THEIL, "Three-Stage Least-Squares: Simultaneous Estimation of Simultaneous Equations," *Econometrica*, Jan. 1962, *30*, 54–78.

ALLOCATIVE EFFICIENCY VS. "X-EFFICIENCY"

By HARVEY LEIBENSTEIN*

At the core of economics is the concept of efficiency. Microeconomic theory is concerned with allocative efficiency. Empirical evidence has been accumulating that suggests that the problem of allocative efficiency is trivial. Yet it is hard to escape the notion that efficiency in some broad sense is significant. In this paper I want to review the empirical evidence briefly and to consider some of the possible implications of the findings, especially as they relate to the theory of the firm and to the explanation of economic growth. The essence of the argument is that microeconomic theory focuses on allocative efficiency to the exclusion of other types of efficiencies that, in fact, are much more significant in many instances. Furthermore, improvement in "nonallocative efficiency" is an important aspect of the process of growth.

In Section I the empirical evidence on allocative efficiency is presented. In this section we also consider the reasons why allocation inefficiency is frequently of small magnitude. Most of the evidence on allocative inefficiency deals with either monopoly or international trade. However, monopoly and trade are not the focus of this paper. Our primary concern is with the broader issue of allocative efficiency versus an initially undefined type of efficiency that we shall refer to as "*X*-efficiency." The magnitude and nature of this type of efficiency is examined in Sections II and III. Although a major element of "*X*-efficiency" is motivation, it is not the only element, and hence the terms "motivation efficiency" or "incentive efficiency" have not been employed.

As he proceeds, the reader is especially invited to keep in mind the sharp contrast in the magnitudes involved between Tables 1 and 2.

I. *Allocative Inefficiency: Empirical Evidence*

The studies that are of interest in assessing the importance of allocative efficiency are summarized in Table 1. These are of two types. On the one side we have the studies of Harberger and Schwartzman on the "social welfare cost" of monopoly. On the other side we have a number of studies, among them those by Johnson, Scitovsky, Wemelsfelder, Janssen, and others, on the benefits of reducing or eliminating restrictions to trade. In both cases the computed benefits attributed to the reallocation of resources turn out to be exceedingly small.

* The author is professor of economics at the University of California, Berkeley.

TABLE 1—CALCULATED "WELFARE LOSS" AS PERCENTAGE OF GROSS OR NET NATIONAL PRODUCT ATTRIBUTED TO MISALLOCATION OF RESOURCES

Study	Source	Country	Cause	Loss
A. C. Harberger	*A.E.R.* 1954	U.S.A. 1929	Monopoly	.07 per cent
D. Schwartzman	*J.P.E.* 1960	U.S.A. 1954	Monopoly	.01 per cent
T. Scitovsky	(1)	Common Market 1952	Tariffs	.05 per cent
J. Wemelsfelder	*E.J.* 1960	Germany 1958	Tariffs	.18 per cent
L. H. Janssen	(2)	Italy 1960	Tariffs	max. .1 per cent
H. G. Johnson	*Manchester School* 1958	U. K. 1970	Tariffs	max. 1.0 per cent
A. Singh	(3)	Montevideo Treaty Countries	Tariffs	max. .0075 per cent

Sources:
(1) [29].
(2) [16].
(3) Unpublished calculation made by A. Singh based on data found in A. A. Faraq, *Economic Integration: A Theoretical, Empirical Study*, University of Michigan, Ph.D. Thesis, 1963.

Let us look at some of the findings. In the original Harberger study [14] the benefits for eliminating monopoly in the United States would raise income no more than 1/13 of 1 per cent. Schwartzman's [28] study which recomputes the benefits of eliminating monopoly by comparing Canadian monopolized industries as against counterpart competitive U.S. industries, and vice versa in order to determine the excess price attributable to monopoly, ends up with a similar result. Similarly, the benefits attributed to superior resource allocation as a consequence of the Common Market or a European Free Trade Area are also minute—usually much less than 1 per cent.

The calculations made by Scitovsky of the benefits to the Common Market (based on Verdoorn's data) led him to the conclusion that ". . . the most striking feature of these estimates is their smallness. The one that is really important (for reasons to appear presently), the gain from increased specialization . . . which is less than one-twentieth of one per cent of the gross social product of the countries involved. This is ridiculously small . . ." [29, p. 64]. J. Wemelsfelder [33, p. 100] has calculated that the welfare gain of reducing import duties and increasing imports and exports accordingly amounts to .18 of 1 per cent of national income. Harry Johnson in an article on England's gain in joining a Free Trade Area [17, pp. 247 ff.] calculates the net gain from trade at less than 1 per cent. That is, Johnson arrives at the conclusion that 1 per cent of the national income would be the absolute maximum gain for Britain from entering the European Free Trade Area.

A recent study by L. H. Janssen [16, p. 132] calculates that the gains from increased specialization for the different countries of the European Economic Community would be largest for Italy, but even here the amount is only 1/10 of 1 per cent of total production.[1] Janssen points out that, if the production gain for Italy due to specialization were calculated by Scitovsky's method, which he believes involves an overestimation, "the production gain in the most extreme case is still less than .4 per cent." Janssen concludes, as have others, that the welfare effects of a customs union based on the superior allocation of resources are likely to be trivial. He does, however, point to the possibility "that the mere prospect of the frontiers opening would infuse fresh energy into entrepreneurs." He recognizes that certain qualitative factors may be highly important and that the consequences of growth are certainly more significant than those of allocative welfare.

My research assistant, A. Singh, has calculated the gains from trade (following the Scitovsky method) for the Montevideo Treaty Countries[2] (Argentina, Brazil, Chile, Mexico, Paraguay, Peru, and Uruguay) and found it to be less than 1/150 of 1 per cent of their combined GNP. Even if we double or triple this result to allow for such factors as the effect of failing to take account of quantitative restrictions in the analysis, the outcome is still trivial.

Harberger's study on Chile [14] which involves the reallocation of both labor and capital yields a relatively large estimate. Harberger intends to obtain as large an estimate as possible of the consequences of reallocating resources by using what I believe to be (and what he admits to be) rather extreme assumptions in order to obtain maximum outer bounds. Despite this he comes up with a number that is between 9 and 15 per cent. However, no actual data are employed. What are used are outer-bound estimates based on personal impressions. I expect that a careful study similar to the Verdoorn-Scitovsky study would probably come up with numbers that would be no larger than 1 or 2 per cent.

The empirical evidence, while far from exhaustive, certainly suggests that the welfare gains that can be achieved by increasing *only*

[1] R. A. Mundell in a review of Janssen's book appears to reach a similar conclusion to the point made in this paper when he speculates that:

> . . . there have appeared in recent years studies purporting to demonstrate that the welfare loss due to monopoly is small, that the welfare importance of efficiency and production is exaggerated, and that gains from trade and the welfare gains from tariff reduction are almost negligible. Unless there is a thorough theoretical re-examination of the validity of the tools on which these studies are founded, and especially of the revitalized concepts of producers' and consumers' surplus, some one inevitably will draw the conclusion that economics has ceased to be important! [22, p. 622].

[2] Based on data found in [11].

allocative efficiency are usually exceedingly small, at least in capitalist economies. In all but one of the cases considered all of the gains are likely to be made up in one month's growth. They hardly seem worth worrying about.

Let us see briefly why these gains are usually small. We cannot prove that we would expect them to be small on purely theoretical grounds. If we combine our theory with what we could agree are probably reasonable estimates of some of the basic magnitudes, then it appears likely that in many cases (but certainly not all *possible* cases) the welfare loss of allocative inefficiency is of trivial significance. The idea could be developed with the aid of the diagram employed by Harberger. (See Figure 1.) In Figure 1 we assume that costs are constant

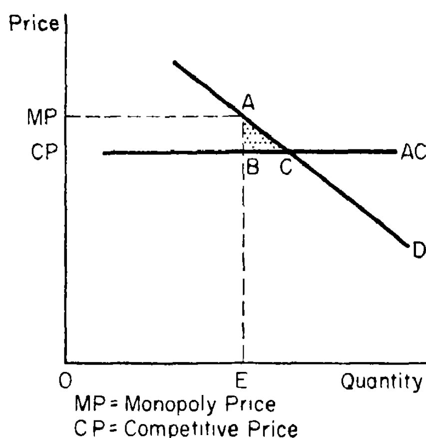


FIGURE 1

within the relevant range. D is the demand function. Under competition price and quantity are determined at the intersection C. The monopoly price is above the competitive price equal to AB in the figure. The monopoly output is determined at the point A. The welfare loss due to monopoly, which is the same as the welfare gain if we shifted to competition, is equal to the triangle ABC. We obtain an approximation to this amount by multiplying the price differential AB by the quantity differential BC by one-half and multiplying this by the proportion of national income in industries involving the misallocation.

Let us play around with some numbers and see the kind of results we get as a consequence of this formulation. Suppose that half of the national output is produced in monopolized industries and that the price differential is 20 per cent and that the average elasticity of demand is 1.5. Now the outcome will turn out to be 1½ per cent. But we really used enormous figures for the misallocation. And yet the result

is small. Monopoly prices, according to estimates, appear to be only about 8 per cent on the average above competitive prices. We can substitute some reason other than monopoly for the misallocation and still come out with similar results.[3]

Consider the cases of subsidized industries under some sort of governmental inducements to growth; and that of governmentally run industries. In the subsidy case the calculation would be similar. Suppose that as much as 50 per cent of the industries are subsidized to the extent of a 20 per cent difference in cost and that the output point on the demand function is where elasticity is unity. This last point may be reasonable since the operators of subsidized industries might want gross revenue to be as large as possible. If, on the other hand, we assume that they are profit maximizers and restrict output to a greater extent, then we might assume a price elasticity of two. This latter, however, is unlikely because monopoly profits are inconsistent with subsidized industries. Those who receive the subsidy would have the legitimate fear that the subsidy would be lowered if unusual profits were earned. Hence, behavior in the direction of revenue maximization appears reasonable and the calculated welfare loss is less than 2 per cent.

A similar result could be achieved in the case in which the government runs industries that affect 50 per cent of the national income of an economy. In all the cases we have considered, the magnitudes chosen appear to be on the large side and the outcome is on the small side.

Of course, it is possible that the magnitude of allocative inefficiency would be large if there are large discontinuities in productivity between those industries where inputs are located and those industries to which the same inputs could be moved. This, in effect. is the basic assumption that Harberger made in his study of Chile [14]. But if it turns out that there is a reasonable degree of continuity in productivity, and that the only way shifts could be made is by simultaneously increasing either social overhead capital or direct capital in order to make the shifts involved, then, of course, a great deal of the presumed gains would be eaten up by the capital costs and the net marginal gains would turn out to be rather small. My general impression is that this is likely to be the case in a great many underdeveloped countries where

[3] For the sake of completeness we should take the income effect into account in our estimation of consumer surplus. It may readily be seen that this magnitude is likely to be exceedingly small. Suppose that the initial effect of a superior allocation is 1 per cent; then the income effect for a noninferior good will be to shift the demand function to the right by 1 per cent on the average. Thus, the addition to consumers' surplus will be 1 per cent, and the consumers' surplus foregone will be roughly 1 per cent of 1 per cent. If we consider all consequent effects in a similar vein, then the estimated welfare loss will be $.010101 \ldots < .0102$. The actual magnitude will, of course, be smaller because the demand will shift to the left in the case of inferior goods. For an excellent discussion of these matters see A. P. Lerner [21].

differential productivities appear to exist between the agricultural sector and the industrial sector. One cannot go beyond stating vague impressions since there is a lack of hard statistical evidence on this matter.

Why are the welfare effects of reallocation so small? Allocational inefficiency involves only the net marginal effects. The basic assumption is that every firm *purchases and utilizes* all of its inputs "efficiently." Thus, what is left is simply the consequences of price and quantity distortions. While some specific price distortions might be large it seems unlikely that all relative price distortions are exceptionally large. This implies that most quantity distortions must also be relatively small since for a given aggregate output a significant distortion in one commodity will be counterbalanced by a large number of small distortions in the opposite direction in quantities elsewhere. While it is possible to *assume* relative price distortions and quantity distortions that would be exceedingly high, it would be difficult to believe that, without intent, the sum of such distortions should be high. However, it is not *necessarily* so on purely *a priori* grounds.

There is one important type of distortion that cannot easily be handled by existing microeconomic theory. This has to do with the allocation of managers. It is conceivable that in practice a situation would arise in which managers are exceedingly poor, that is, others are available who do not obtain management posts, and who would be very much superior. Managers determine not only their own productivity but the productivity of all cooperating units in the organization. It is therefore possible that the actual loss due to such a misallocation might be large. But the theory does not allow us to examine this matter because firms are presumed to exist as entities that make optimal input decisions, apart from the decisions of its managers. This is obviously a contradiction and therefore cannot be handled.

II. *X-Efficiency: The Empirical Evidence*

We have seen that the welfare loss due to allocational inefficiency is frequently no more than 1/10 of 1 per cent. Is it conceivable that the value of *X*-inefficiency would be larger than that? One way of looking at it is to return to the problem of the welfare loss due to monopoly. Suppose that one-third of the industries are in the monopolized sector. Is it possible that the lack of competitive pressure of operating in monopolized industries would lead to cost 3/10 of a per cent higher than would be the case under competition? This magnitude seems to be very small, and hence it certainly seems to be a possibility. The question essentially, is whether we can visualize managers bestirring themselves sufficiently, if the environment forced them to do so, in order to reduce

costs by more than 3/10 of 1 per cent. Some of the empirical evidence available suggests that not only is this a possibility, but that the magnitudes involved are very much larger. As we shall see, the spotty evidence on this subject does not prove the case but it does seem to be sufficiently persuasive to suggest the possibility that X-efficiency exists, and that it frequently is much more significant than allocational efficiency.

Professor Eric Lundberg in his studies of Swedish industries points to the case of the steel plant at Horndal that was left to operate without any new capital investment or *technological change,* and furthermore maintenance and replacement were kept at a minimum, and yet output per man hour rose by 2 per cent per annum. Professor Lundberg asserts that according to his interviews with industrialists and technicians "sub-optimal disequilibrium in regard to technology and utilization of existing capital stock is a profoundly important aspect of the situation at any time." (This according to Gorin Ohlin's summary of Lundberg's findings [24].) If a suboptimal disequilibrium exists at any time, then it would seem reasonable that under the proper motivations managers and workers could bestir themselves to produce closer to optimality, and that under other conditions they may be motivated to move farther away from optimality.

Frederick Harbison reports visiting two petroleum refineries in Egypt less than one-half mile apart. "The labor productivity of one had been nearly double that in the other for many years. But recently, under completely new management, the inefficient refinery was beginning to make quite spectacular improvements in efficiency with the same labor force" [15, p. 373]. We may inquire why the management was changed only recently whereas the difference in labor productivity existed for many years. It is quite possible that had the motivation existed in sufficient strength, this change could have taken place earlier.

In a recent book on the firm, Neil Chamberlain [5, p. 341] visualizes his firms reacting to variances between forecasted revenues and expenditures and actual. He quotes from the president of a corporation: "Actual sales revenue for the fiscal year varied one per cent from the original forecast. Expenditures varied 30 per cent. The reasons were practically entirely due to manufacturing problems of inefficiency and quality. . . . The only actions specificially taken were in attempted changes in methods of production . . . [and] the use of an engineering consulting firm. . . ." One would have thought that the cost-reducing activities mentioned could be carried out irrespective of the variance. Nevertheless, the quotation clearly implies that, in fact, they would not have been motivated to attempt the changes were it not that they were stimulated by the variance.

Before proceeding to present more empirical evidence on the possi-e magnitude of X-efficiency it is of importance to say something ıout the nature of the data. The empirical evidence does not present any unambiguous cases. Most of the evidence has to do with specific ms or, at best, industries, and not for the economy as a whole. In the idence presented on allocative efficiency the entire economy was con-lered. It is quite possible that the cases considered are entirely atyp-al and could not be duplicated in large segments of the economy. In ldition, the cases do not always deal with X-efficiency in a pure sense. me additional inputs or reallocations are sometimes involved. Also ıcertainty elements and accidental variations play a role. Neverthe-ss, it seems that the magnitudes involved are so large that they sug-st that the conjecture that X-efficiency is frequently more significant ıan allocative efficiency must be taken seriously.

Now let us turn to Tables 1 and 2. In contrast to Table 1 where the isallocation effects are small, we see in Table 2 that the X-efficiency fects, at least for specific firms, are usually large. Table 2 abstracts in the interest of conserving space) from a much more comprehensive ıble developed by Kilby [19] that summarizes the results of a umber of ILO productivity missions. (I usually picked for each coun-y the first three and the last items contained in Kilby's table.) It is) be observed that the cost-reducing methods used do not involve ad-itional capital nor, as far as one can tell, any increase in depreciation r obsolescence of existing capital. The methods usually involve some imple reorganizations of the production process, e.g., plant-layout eorganization, materials handling, waste controls, work methods, and ayments by results. It is of interest that the cost reductions are fre-uently above 25 per cent and that this result is true for a technically dvanced country such as Israel as well as for the developing countries onsidered in other parts of the table. If the firms and/or operations onsidered are representative, then it would appear that the contrast ı significance between X-efficiency and allocative efficiency is indeed tartling. Representativeness has not been established. However, the eports of the productivity missions do not suggest that they went out f their way to work only on cases where large savings in costs could e obtained. By comparative standards (with other productivity mis-ions) some of the results were modest, and in some cases Kilby re-orts that when some members of the missions returned to some of the irms they had worked on previously (e.g., in Pakistan) they found a eversion to previous methods and productivities.

There are of course a number of other studies, in addition to those y Lundberg and Harbison just mentioned which present results imilar to the ILO reports. L. Rostas in his study of comparative pro-

TABLE 2—ILO PRODUCTIVITY MISSION RESULTS

Factory or Operation	Method*	Increase in Labor Productivity %	Impact on the Firm (Unit Cost Reduction)	
			Labor Savings %	Capital† Savings %
India				
Seven textile mills	n.a.	5-to-250	5–71	5–71
Engineering firms				
All operations	F, B	102	50	50
One operation	F	385	79	79
One operation	F	500	83	83
Burma				
Molding railroad brake shoes	A, F, B	100	50	50
Smithy	A	40	29	29
Chair assembly	A, B	100	50	50
Match manufacture	A, F	24	19	—
Indonesia				
Knitting	A, B	15	13	—
Radio assembly	A, F	40	29	29
Printing	A, F	30	23	—
Enamel ware	F	30	23	—
Malaya				
Furniture	A, D	10	9	9
Engineering workshop	A, D	10	9	9
Pottery	A, B	20	17	17
Thailand				
Locomotive maintenance	A, F	44	31	31
Saucepan polishing	E, D	50	33	—
Saucepan assembly	B, F	42	30	—
Cigarettes	A, B	5	5	—
Pakistan				
Textile plants	C, H, G			
Weaving		50	33	33
Weaving		10	9	9
Bleaching		59	37	37
Weaving		141	29	29
Israel				
Locomotive repair	F, B, G	30	23	23
Diamond cutting and polishing	C, B, G	45	31	—
Refrigerator assembly	F, B, G	75	43	43
Orange picking	F	91	47	—

* A = plant layout reorganized; B = machine utilization and flow; C = simple technical alterations; D = materials handling; E = waste control; F = work method; G = payment by results; H = workers training and supervision

† Limited to plant and equipment, excluding increased depreciation costs.

Source: P. Kilby [19, p. 305].

ductivity in British and American industry [26] points to the finding that differences in amount and quality of machinery per worker and the rates of utilization and replacement do not account for the entire difference in output per worker in the two countries. He further states

that ". . . in a number of industries (or firms) where the equipment is very largely identical in the U.S. and U.K., eggs, boots and shoes, tobacco, strip steel (or in firms producing both in the U.K. and U.S. . . .), there are still substantial differences in output per worker in the U.K. and the U.S." Clearly there is more to the determination of output than the obviously observable inputs. The nature of the management, the environment in which it operates, and the incentives employed are significant.

That changes in incentives will change productivity per man (and cost per unit of output) is demonstrated clearly by a wide variety of studies on the effects of introducing payments by results schemes. Davison, Florence, Gray, and Ross [7, p. 203] review the literature in this area for British industry, survey the results for a number of manufacturing operations, and present illustrative examples of their findings from a number of firms. The summary of their findings follows: "The change in output per worker was found to vary among the different operations all the way from an increase of 7.5 per cent to one of 291 per cent, about half the cases falling between 43 per cent and 76 per cent. Such increases in output, most of them large, from our 'first-line' case histories, and from additional evidence, were found not to be just a 'flash in the pan' but were sustained over the whole period of study."

Roughly similar findings were obtained for the consequences of introducing payments by results in Australia, Belgium, India, the Netherlands, and the United States [36]. In Victoria it was found that "soundly designed and properly operated incentive plans have in practice increased production rate in the reporting firms from 20 to 50 per cent." In the Netherlands labor efficiency increases of 36.5 per cent were reported. It seems clear that with the same type of equipment the working tempo varies considerably both between different workers and different departments. Appropriate incentives can obviously change such tempos considerably and reduce costs, without any changes in purchasable inputs per unit.

The now-famous Hawthorne Studies [25] suggest that the mere fact that management shows a special interest in a certain group of workers can increase output. That is, management's greater interest in the group on whom the experiments were tried, both when working conditions were improved and when they were worsened, created a positive motivation among the workers. (The magnitudes were from 13 to 30 per cent [20].) In one of the ILO missions to Pakistan an improvement in labor relations in a textile mill in Lyallpur resulted in a productivity increase of 30 per cent. Nothing else was changed except that labor turnover was reduced by one-fifth [37] [38].

Individual variations in worker proficiency are probably larger than plant differences. Frequently the variation between the best to poorest worker is as much as four to one. Certainly improved worker selection could improve productivity at the plant level. To the extent that people are not working at what they are most proficient at, productivity should rise as a consequence of superior selection methods [13, p. 147].

Although there is a large literature on the importance of psychological factors on productivity, it is usually quite difficult to assess this literature because many psychologists work on the basis of high- and low-productivity groups but do not report the actual numerical differences. In general, it seems that some of the psychological factors studied in terms of small-group theory can account for differences in productivity of from 7 to 18 per cent. The discoveries include such findings as (1) up to a point smaller working units are more productive than larger ones; (2) working units made up of friends are more productive than those made up of nonfriends; (3) units that are generally supervised are more efficient than those that are closely supervised [1]; and (4) units that are given more information about the importance of their work are more proficient than those given less information [32]. A partial reason for these observed differences is probably the likelihood that individual motivation towards work is differently affected under the different circumstances mentioned.

The shorter-hours movement in Western Europe and in the United States, especially up to World War I, has some interesting lessons for productivity differentials without capital changes. Economists frequently assume that for a given capital stock and quality of work force, output will be proportional to number of hours worked. Experiments during World War I and later showed that not only was the proportionality law untrue, but that frequently *absolute* output actually increased with reductions in hours—say from a ten-hour day to an eight-hour day.[4] It was also found that with longer hours a disproportionate amount of time was lost from increased absenteeism, industrial accidents, and so on. In many cases it would obviously have been to a firm's interest to reduce hours below that of the rest of the industry. Firms could have investigated these relations and taken advantage of the findings. For the most part, governments sponsored the necessary research on the economics of fatigue and unrest under the stimulus of the war effort, when productivity in some sectors of the economy was believed to be crucial. The actual reduction of hours that took place

[4] The empirical findings and experimental literature are reviewed in a number of places. For a brief review of the literature see [37]. See page 5 for bibliography of major works in the area.

was a consequence of the pressure of labor unions and national legislation.

In this connection it is of interest to note that Carter and Williams [4, pp. 57ff.] in their study of investment in innovations found that a high proportion (over 40 per cent) was of a "passive" character—i.e., either in response to the "direct pressure of competition" or "force of example of firms (etc.) other than immediate rivals." Unfortunately it is difficult to find data that would represent the obverse side of the coin; namely, data that would suggest the degree to which firms do not innovate for lack of a sufficient motivating force, such as a lack of competitive pressure. However, there is a great deal of evidence that the delay time between invention and innovation is often exceedingly long (sometimes more than 50 years),[5] and the lag time between the use of new methods in the "best practice" firms in an industry and other firms is also often a matter of years. Salter in his study on *Productivity and Technical Change* [27, p. 98] points to the following striking example. "In the United States copper mines, electric locomotives allow a cost saving of 67 per cent yet although first used in the mid-twenties, by 1940 less than a third of locomotives in use were electric."[6] Other similar examples are mentioned by Salter and others. A survey of industrial research undertaken by 77 companies showed that one-third were carrying on research for "aggressive purposes," but that two-thirds were "forced into research for defensive purposes."[7]

The relation between the "cost" of advice or consulting services and the return obtained has not been worked out for the ILO productivity missions as a whole. In one case (in Pakistan) the savings affected in three textile mills as a consequence of the work of the mission during the year that the mission was there "represented about 20 times the entire cost of the mission in that year." While the study does not indicate how representative this result was, the impression one gets is that rates of return of rather large magnitudes are not entirely unusual.

J. Johnston studied the return to consulting services in Great Britain.

[5] See the table in [9, pp. 305-6].

[6] [27]. See especially Appendix to Chapter 7, "Evidence Relating to the Delay in the Utilization of New Techniques." It seems to me that Salter did not quite draw the only possible conclusion from his Table 11. Plants with no significant changes in equipment, method, and plant layout had quite startling changes in output per man-hour, especially if we consider the fact demonstrated in the table that output per man-hour frequently falls under such circumstances. The range of variation in the changes (24 per cent) is larger for the plants without significant changes in equipment, etc., than for those with significant improvements. This is not to argue against the thesis that changes in techniques are important, but to suggest that significant variations in production can and do occur without such changes.

[7] See [3] for source.

For the class of jobs where it was possible to make a quantitative assessment of the results (600 jobs were involved), it was found that on the average the rate of return was about 200 per cent on consulting fees [18, p. 248]. Johnston's study is of special interest for our purposes because (a) it is a very careful study, and (b) the magnitudes of increases in productivity are of the same order (although the variations are less extreme) as those obtained in underdeveloped countries. The nature of the consulting work was not too dissimilar to that carried out by the ILO teams. On the whole they involved improvements in general management, plant layout, personnel, production procedures, selling organization, management and budgeting and accounting systems. For the consulting jobs whose consequences were quantitatively assessed, the average increase in productivity was 53 per cent, the lowest quartile showed an increase of 30 per cent, and the highest quartile 70 per cent [18, p. 273].

The studies mentioned deal with examples that are more or less of a microeconomic nature. In recent years we have had a number of studies that are their *macro*economic complements. The work of Solow, Aukrust, Denison, and others show that only a small proportion of increase in GNP is accounted for by increases in inputs of labor or capital. The "unexplained residual" covers about 50 per cent to 80 per cent of growth in advanced countries [2] [10] [23] [30] [31]. The residual comprehends a greater range of "noninput" growth factors (e.g., technological change, education of the labor force) than was covered in the examples we considered, but the motivational efficiency elements may account for some fraction of the residual. (E.g., Johnston estimates that one quarter of the annual increase in product is accounted for by consulting services.)

What conclusions can we draw from all of this? First, the data suggest that there is a great deal of possible variation in output for similar amounts of capital and labor and for similar techniques, in the broad sense, to the extent that technique is determined by similar types of equipment. However, in most of the studies the nature of the influences involved are mixed, and in some cases not all of them are clear to the analyst. In many instances there appears to have been an attempt to impart knowledge, at least of a managerial variety, which accounts for *some* of the increase in output. But should this knowledge be looked upon as an increase in inputs of production in all instances? Although the first reaction might be that such attempts involve inputs similar to inputs of capital or labor, I will want to argue that in many instances this is not the case.

It is obvious that not every change in technique implies a change in knowledge. The knowledge may have been there already, and a change

in circumstances induced the change in technique. In addition, knowledge may not be used to capacity just as capital or labor may be underutilized. More important, a good deal of our knowledge is vague. A man may have nothing more than a sense of its existence, and yet this may be the critical element. Given a sufficient inducement, he can then search out its nature in detail and get it to a stage where he can use it. People normally operate within the bounds of a great deal of intellectual slack. Unlike underutilized capital, this is an element that is very difficult to observe. As a result, occasions of genuine additions to knowledge become rather difficult to distinguish from those circumstances in which no new knowledge has been added, but in which existing knowledge is being utilized to greater capacity.

Experience in U.S. industry suggests that adversity frequently stimulates cost-reducing attempts, some of which are successful, within the bounds of existing knowledge [12]. In any event, some of the studies suggest that motivational aspects are involved entirely apart from additional knowledge. The difficulty of assessment arises because these elements are frequently so intertwined that it is difficult to separate them.

Let us now consider types of instances in which the motivational aspect appears fairly clearly to play a role. The ILO studies discuss a number of cases in which there had been a reversion to previous less efficient techniques when demonstration projects were revisited after a year or more. This seems to have occurred both in India and in Pakistan [38, p. 157]. Clearly, the new knowledge, if there were such knowledge, was given to the management by the productivity mission at the outset, and the new management methods were installed at least for the period during which the productivity mission was on hand, but there was not a sufficient motivational force for the management to maintain the new methods. The "Hawthorne Effects" are of a more clear-cut nature. Here an intentional reversion to previous methods still led to some increases in output simply because the motivational aspects were more important than the changes in the work methods. The ILO mission reports also mention with regret the fact that techniques applied in one portion of a plant, which led to fairly large increases in productivity, were not taken over by the management and applied to other aspects of the production process, although they could quite easily have done so [38, p. 157]. In a sense we may argue that the knowledge was available to the management, but that somehow it was not motivated to transfer techniques from one portion of a plant to another.

Studies which showed increases in output as a consequence of introducing payment by results clearly involve motivational elements. For

the men subjected to the new payment scheme economic motivations are involved. For the management the situation is less clear. It is possible that in many instances the firms were not aware of the possible advantages of payment by results until they obtained the new knowledge that led to the introduction of the scheme. However, it seems most likely that this scheme is so well known that this is not the case in all, or in many instances. Management quite likely had to be motivated to introduce the scheme by some factors either within the firm or within the industry. In any event, these studies clearly suggest that for some aspects of production, motivational elements are significant.

Both the ILO studies and the Johnston study speak of the need to get the acceptance of top management for the idea of obtaining and implementing consulting advice. In addition, the ILO studies make the point that low productivity is frequently caused by top management's concern with the commercial and financial affairs of the firm rather than with the running of the factory. The latter was frequently treated as a very subordinate task. Whether this last aspect involves a lack of knowledge or a lack of motivation is difficult to determine. However, it seems hard to believe that if some top-management people in some of the firms in a given industry were to become concerned with factory management and achieve desirable results thereby, some of the others would not follow suit. Johnston makes the point that, "without the willing cooperation of management the consultant is unlikely to be called in the first instance or to stay for long if he does come in" [18, p. 237]. The ILO missions make similar remarks.

It is quite clear that consulting services are not only profitable to consultants but also highly profitable to many of the firms that employ them. But it is rather surprising that more of these services are not called for. Part of the answer may be that managements of firms are not motivated to hire consultants if things appear to be going "in any reasonably satisfactory rate." There are, of course, numerous personal resistances to calling for outside advice. If the motivation is strong enough, e.g., the threat of the failure of the firm, then it is likely that such resistances would be overcome. But these are simply different aspects of the motivational elements involved.

III. *The Residual and X-Efficiency: An Interpretation*

The main burden of these findings is that X-inefficiency exists, and that improvement in X-efficiency is a significant source of increased output. In general, we may specify three elements as significant in determining what we have called X-efficiency: (1) intra-plant motiva-

tional efficiency, (2) external motivational efficiency, and (3) nonmarket input efficiency.

The simple fact is that neither individuals nor firms work as hard, nor do they search for information as effectively, as they could. The importance of motivation and its association with degree of effort and search arises because the relation between inputs and outputs is *not* a determinate one. There are four reasons why given inputs cannot be transformed into predetermined outputs: (a) contracts for labor are incomplete, (b) not all factors of production are marketed, (c) the production function is not completely specified or known, and (d) interdependence and uncertainty lead competing firms to cooperate tacitly with each other in some respects, and to imitate each other with respect to technique, to some degree.

The conventional theoretical assumption, although it is rarely stated, is that inputs have a fixed specification and yield a fixed performance. This ignores other likely possibilities. Inputs may have a fixed specification that yields a variable performance, or they may be of a variable specification and yield a variable performance. Some types of complex machinery may have fixed specifications, but their performance may be variable depending on the exact nature of their employment. The most common case is that of labor services of various kinds that have variable specifications and variable performance—although markets sometimes operate as if much of the labor of a given class has a fixed specification. Moreover, it is exceedingly rare for all elements of performance in a labor contract to be spelled out. A good deal is left to custom, authority, and whatever motivational techniques are available to management as well as to individual discretion and judgment.

Similarly, the production function is neither completely specified nor known. There is always an experimental element involved so that something may be known about the current state; say the existing relation between inputs and outputs, but not what will happen given changes in the input ratios. In addition, important inputs are frequently not marketed or, if they are traded, they are not equally accessible (or accessible on equal terms) to all potential buyers. This is especially true of management knowledge. In many areas of the world managers may not be available in well-organized markets. But even when they are available, their capacities may not be known. One of the important capacities of management may be the degree to which managers can obtain factors of production that in fact are not marketed in well-organized markets or on a universalistic basis. In underdeveloped countries the capacity to obtain finance may depend on family connections. Trustworthiness may be similarly determined. Some types of

market information may be available to some individuals but not purchasable in the market. For these and other reasons it seems clear that it is one thing to purchase or hire inputs in a given combination; it is something else to get a predetermined output out of them.

Another possible interpretation of the data presented is in connection with the "residual" in economic growth analysis. The residual manifests itself in three basic ways: (1) through cost reduction in the production of existing commodities without inventions or innovations; (2) the introduction of innovations in processes of production; and (3) the introduction of new commodities or, what is the same thing, quality improvements in consumer goods or inputs. We have ignored the introduction of new commodities, but the other two elements are pertinent here. The data suggest that cost reduction that is essentially a result of improvement in X-efficiency is likely to be an important component of the observed residual in economic growth. In addition, there is no doubt that, in some of the cases of reduced cost, new knowledge was conveyed to the firms involved, and this too is part of the residual. It is of special interest that such new knowledge involves knowledge dissemination rather than invention. The detailed studies suggest that the magnitudes are large, and hence a significant part of the residual does not depend on the types of considerations that have been prominent in the literature in recent years, such as those that are *embodied* in capital accumulation or in invention. We have considered the problem in terms of decreasing real costs per unit of output. It is clear that for a given set of resources, if real costs per unit of output are decreased, then total output will grow, and output per unit of input will also rise. Such efforts to reduce cost are part of the contribution of the residual to economic growth.

Both competition and adversity create some pressure for change. Even if knowledge is vague, if the incentive is strong enough there will be an attempt to augment information so that it becomes less vague and possibly useful. Where consulting advice is available it is significant that relatively few firms buy it. Clearly, motivations play a role in determining the degree that consulting advice is sought. The other side of the coin is that, where the motivation is weak, firm managements will permit a considerable degree of slack in their operations and will not seek cost-improving methods. Cyert and March [6, pp. 37, 38, 242] point to cases in which costs per unit are allowed to rise when profits are high. In the previous sections we have cited cases in which there was a reversion to less efficient methods after the consultants left the scene. Thus we have instances where competitive pressures from other firms or adversity lead to efforts toward cost reduc-

tion, and the absence of such pressures tends to cause costs to rise.

Some of the essential points made in the previous paragraphs can be illustrated diagramatically, if (in the interest of simplicity) we allow for abstraction from some of the realities of the situation. The main ideas to be illustrated are as follows: (1) Some firms operate under conditions of nonminimum costs, and it is possible for an industry to have a nonminimal cost equilibrium. (2) Improvements in X-efficiency are part of the process of development, and probably a significant proportion of the "residual." In what follows we assume that there are many firms, and that each firm's output is sufficiently small so as not to affect the output, costs, or prices set by other firms. For simplicity we also assume that for each firm there is an average total unit cost (ATUC) curve that has a significant horizontal segment at its trough, and that the output selected will be on that segment. When we visual-

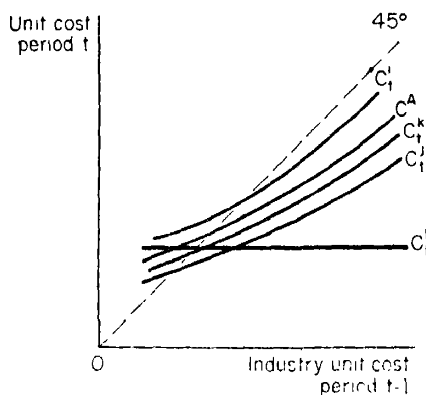


FIGURE 2

ize a firm's costs reacting to competitive conditions in the industry we imply that the entire ATUC curve moves up or down. Some firms are presumed to react to changes in the unit cost of production of the industry as a whole, i.e., to the weighted average of the unit costs of all the firms, in which each firm's weight is in proportion to its contribution to the output of the industry. Here we posit a one-period lag relation. Each firm's expectations of current industry units costs depends on actual industry unit cost in the previous period. If we choose sufficiently small periods, then this seems to be reasonable relation.

In Figure 2 each curve represents the "reaction cost line" of a firm. The ordinate shows the actual unit cost of any firm determined by that firm's reaction to what it believes or expects to be the unit cost performance of the industry as a whole. The alternate expected unit cost per-

formance of the industry is shown on the abscissa. Thus each point on line C_t^i associates the unit cost for firm *i* in period *t*, given the average unit cost in the industry in period *t*-1. The lines are drawn in such a way that they reflect the idea that if the unit cost that is the average for the industry is higher, then the firm's unit cost will also be higher. As average industry unit costs fall, some firms are motivated to reduce their unit costs accordingly. The higher the industry unit cost, the easier it is for any firm to search and successfully *find* means for reducing its own cost. Therefore, for a given incentive toward cost reduction, the firm is likely to find more successful ways of reducing its cost when industry costs are high compared to what they might find when they are low. As a consequence the typical reaction unit cost lines are more steeply sloped where industry unit costs are high compared to when they are low. Indeed, at very low industry unit costs the firm reaction cost lines approach an asymptote. It is not necessary for our analysis to assume that all firms are nonminimizers. Therefore some firms may have reaction cost lines that are horizontal.

The curve C^A is the average of the unit costs of all the firms in question, where the weight for any firm's cost is the proportion of its output to the total industry output. C^A is the average reaction cost line for all the firms. The basic assumption is that a firm's costs will be higher if the average industry costs are expected to be higher, and vice versa. Beyond some point, where expected average industry costs are very low, every reaction cost line will be above the 45° line.

In Figure 3 the line P is a locus of equilibrium prices. Each point on the line associates an equilibrium price with a level of industry unit cost in the previous period, which in turn determines the unit costs level of the various firms in the current period. Thus, given the industry unit cost in period *t*-1, this determines the unit cost level for each firm in period *t*. Each firm in turn will pick that output that maximizes its profits. The sum of all the outputs determines the industry output, and given the demand function for the product, the industry output determines the price. The price will be an equilibrium price if at that price no additional firms are induced to enter the industry or to withdraw from it. Thus the price for each industry unit cost is determined in accordance with conventional price theory considerations. If the price at the outset is above equilibrium price, then the entry of firms will bring that price down toward equilibrium, and if the price is below equilibrium, marginal firms will be forced to leave the industry, which in turn will cause the price to rise. Thus at every level of industry unit cost in period *t*-1 there is a determinate number of firms, that number consistent with the associated equilibrium price.

The point E in Figure 3, the intersection between curve C^A and the 45° line, is an equilibrium point for all the firms. The process envisioned is that each firm sets its cost in period t in accordance with its expectation of the industry cost, which by assumption is what the industry cost was in period t-1. This is a one-period lag relation. Each firm finds out what all of its competitors were doing as a group in terms of cost and reacts accordingly in the next period. If the industry cost is equal to oe then in the subsequent period each firm would set its cost so that the weighted average unit cost of all the firms would be equal to oe. Hence E is an equilibrium point.[8]

But suppose that the initial industry costs were equal to oa. We want to show that this sets up a movement that leads eventually to the point E. The firms' unit costs will average out at ab, which gener-

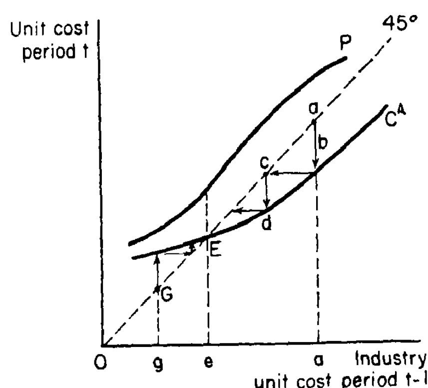


FIGURE 3

ates a process shown by the set of arrows $abcd$, etc., toward the point E. In a similar fashion, if we start with an industry cost of og, a process is set in motion so that costs move from G toward the point E. Clearly E is a stable equilibrium point. It is to be noted that every point on curve C^A need not presume that the same number of firms exist in the industry. At higher costs more firms exist, but as costs decline, some firms are forced out and fewer firms exist. In terms of the

[8] In essence the existence of an equilibrium can be shown on the basis of Brouwer's fixed-point theorem. (Point E in Figure 3 can be interpreted as a fixed point.) It would be possible to develop a much more general theory along the same lines based on less restrictive assumptions and achieve essentially the same result. For instance the one-period lag in the reaction unit cost relation can readily be eliminated. Similarly, the unique relation between the firm's unit cost and the industry unit cost level can be relaxed. See G. Debreu [8, pp. 17-18 and p. 26]. However the essence of the theory would remain the same. To conserve space and in the interest of simplicity I present the more restrictive version

weighted average indicated by the points on the curve C^A, this simpl[y] means that some of the outputs will be zero for some of the firms a[s] we get to lower and lower industry costs.

Figure 4 is intended to illustrate the cost reduction aspect of th[e] residual in growth. When we begin the process the average reactio[n] cost line is $C_1{}^A$. Firms start at point *a* and reduce costs along the arro[w] shown by *ab*. At this point additional information is introduced int[o] the industry which is reflected in the diagram by the shift in the reac[-] tion cost line from $C_1{}^A$ to $C_2{}^A$. Once firms are on $C_2{}^A$ they then procee[d] with the cost reduction process as shown by the arrow *cd*. This illus[-] trates two basic elements involved in the residual, the process of cos[t] reduction in response to the motivation created by competitive pres[-] sures, as well as that part of cost reduction that is reflected in actua[l] innovations, and is illustrated by downward shifts in the reaction cos[t] lines.

IV. *Conclusions*

We have suggested three reasons for X-inefficiency connected with the possibility of variable performance for given units of the inputs.

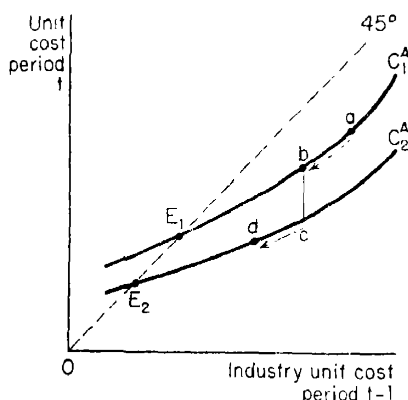


FIGURE 4

These are: (a) contracts for labor are incomplete, (b) the production function is not completely specified or known, and (c) not all inputs are marketed or, if marketed, are not available on equal terms to all buyers. These facts lead us to suggest an approach to the theory of the firm that does not depend on the assumption of cost-minimization by all firms. The level of unit cost depends in some measure on the degree of X-efficiency, which in turn depends on the degree of competitive pressure, as well as on other motivational factors. The re-

sponses to such pressures, whether in the nature of effort, search, or the utilization of new information, is a significant part of the residual in economic growth.

One idea that emerges from this study is that firms and economies do not operate on an outer-bound production possibility surface consistent with their resources. Rather they actually work on a production surface that is well within that outer bound. This means that for a variety of reasons people and organizations normally work neither as hard nor as effectively as they could. In situations where competitive pressure is light, many people will trade the disutility of greater effort, of search, and the control of other peoples' activities for the utility of feeling less pressure and of better interpersonal relations. But in situations where competitive pressures are high, and hence the costs of such trades are also high, they will exchange less of the disutility of effort for the utility of freedom from pressure, etc. Two general types of movements are possible. One is along a production surface towards greater allocative efficiency and the other is from a lower surface to a higher one that involves greater degrees of X-efficiency. The data suggest that in a great many instances the amount to be gained by increasing allocative efficiency is trivial while the amount to be gained by increasing X-efficiency is frequently significant.

REFERENCES

1. M. Argyle, G. Gardner, and F. Cioffi, "Supervisory Methods Related to Productivity, Absenteeism, and Labor Turnover," *Human Relations,* 1958, *10,* 23-29.
2. O. Aukrust, "Investment and Economic Growth," *Prod. Meas. Rev.,* Feb. 1959, *16,* 35-53.
3. Y. Brozen, "Research, Technology, and Productivity," *Industrial Productivity,* December 1951, Industrial Relations Research Association Paper 7, p. 30.
4. C. F. Carter and B. R. Williams, *Investment in Innovations.* London 1958.
5. N. Chamberlain, *The Firm: Micro Economic Planning and Action.* New York 1962.
6. R. M. Cyert and J. G. March, *A Behavioral Theory of the Firm,* Englewood Cliffs, N.J. 1963, pp. 36-38, and Ch. 9, by O. E. Williamson, pp. 237-52.
7. J. P. Davison, P. S. Florence, and B. Gray and N. Ross, *Productivity and Economic Incentives.* London 1958.
8. G. Debreu, *Theory of Value.* New York 1959.
9. J. L. Enos, "Invention and Innovation in the Petroleum Refining Industry," in *The Rate and Direction of Inventive Activity,* Princeton, NBER, 1962, pp. 305-6.

10. S. Fabricant, Basic Facts on Productivity. New York, NBER, 1959.
11. A. A. Faraq, *Economic Integration: A Theoretical Empirical Study*. Unpublished doctoral dissertation, Univ. Michigan, 1963.
12. J. J. Friedman, "Top Management Faces the Cost Challenge," *Dun's Rev. and Mod. Industry*, Jan. 1961, *77*, 34-36.
13. E. E. Ghiselli and C. W. Brown, *Personnel and Industrial Psychology*. New York 1948.
14. A. Harberger, "Using the Resources at Hand More Effectively," *Am. Econ. Rev., Proc.*, May 1959, *59*, 134-47.
15. F. Harbison, "Entrepreneurial Organization as a Factor in Economic Development," *Quart. Jour. Econ.*, Aug. 1956, *70*, 364-79.
16. L. H. Janssen, *Free Trade, Protection and Customs Union*, Leiden 1961, p. 132.
17. H. Johnson, "The Gains from Freer Trade with Europe: An Estimate," *Man. School Econ. Soc. Stud.*, Sept. 1958, *26*, 247-55.
18. J. Johnston, "The Productivity of Management Consultants," *Jour. Royal. Stat. Soc.*, Ser. A., 1963, *126*, Pt. 2, 237-49.
19. P. Kilby, "Organization and Productivity in Backward Economies," *Quart. Jour. Econ.*, May 1962, *76*, 303-10.
20. H. A. Landsberger, *Hawthorne Revisited*, Ithaca 1958, pp. 13 ff.
21. A. P. Lerner, "Consumer Surplus and Micro-Macro," *Jour. Pol. Econ.*, Feb. 1963, *71*, pp. 76 ff.
22. R. A. Mundell, review of L. H. Janssen, *Free Trade, Protection, and Customs Union*, in *Am. Econ. Rev.*, June 1962, *52*, pp. 621-22.
23. O. Niitamo, "Development of Productivity in Finnish Industry, 1925-1952," *Prod. Meas. Rev.*, Nov. 1958, *15*, 30-41.
24. G. Ohlin, review of E. Lundberg, *Productivity and Profitability: Studies of the Role of Capital in the Swedish Economy*, in *Am. Econ. Rev.*, Sept. 1962, *52*, 827-29.
25. F. T. Roethlisberger and W. J. Dickson, *Management and the Worker*. Cambridge 1939.
26. L. Rostas, *Comparative Productivity in British and American Industry*, Nat. Inst. Econ. Soc. Research Paper 13, Cambridge (England) 1964, pp. 64 ff.
27. W. E. G. Salter, *Productivity and Technical Change*. Cambridge (England) 1960.
28. D. Schwartzman, "The Burden of Monopoly," *Jour. Pol. Econ.*, Dec. 1960, *68*, 727-29.
29. T. Scitovsky, *Economic Theory and Western European Integration*. Stanford 1958.
30. R. Solow, "Technical Progress and the Aggregate Production Function," *Rev. Econ. Stat.*, Aug. 1957, *39*, 312-20.
31. ———, "Investment and Economic Growth," *Prod. Meas. Rev.*, Nov. 1959, *16*, 62-68.
32. T. Tomekovic, "Levels of Knowledge of Requirements as a Motivational Factor in the Work Situation," *Human Relations*, 1962, *15*, 197-216.

33. J. WEMELSFELDER, "The Short-Term Effect of Lowering Import Duties in Germany," *Econ. Jour.*, March 1960, *60*, 94-104.
34. S. G. WINTER, JR., *Economic Natural Selection and the Theory of the Firm.* Unpublished doctoral dissertation, mimeo, Yale Univ. 1963, pp. 83-85.
35. ANGLO-AMERICAN COUNCIL ON PRODUCTIVITY, *Productivity Measurement in British Industry.* New York 1950.
36. INTERNATIONAL LABOR ORGANIZATION, "Payment by Results," *ILO Studies and Reports,* New Ser. No. 27. Geneva 1951.
37. ———, "ILO Productivity Missions to Underdeveloped Countries, Part 1," *Internat. Lab. Rev.*, July 1957, *76*, 1-29.
38. ———, "ILO Productivity Missions to Underdeveloped Countries, Part 2," *Internat. Lab. Rev.*, Aug. 1957, *76*, 139-66.
39. ———, "Repercussions of a Reduction in Hours of Work," *Internat. Lab. Rev.*, July 1956, *74*, 23-45.

ON THE SIZE DISTRIBUTION OF FIRMS

By RICHARD E. QUANDT*

The economist's concern with monopoly and industrial concentration and the statistician's interest in stochastic processes have led in recent years to several investigations of the size distribution of firms.[1] The discussion has focused mainly on two problems: (a) the nature of the stochastic processes which might generate the growth of industries, and (b) the statistical matters that arise when we wish to assert that this or that sample can be said to have been drawn from a particular statistical distribution.[2]

The purpose of this paper is to make a contribution to this subject by using more disaggregated data than has been the case in the past and more refined methods of fitting distributions to the data and judging the goodness of fit. Section I is devoted to some general economic considerations of what one might expect to find as one examines the size distributions of various industries. Section II discusses briefly the sources of data and the distributions that were considered as candidates for explaining the size distribution of firms and considers the merits of judging goodness of fit on the basis of statistical significance. Section III points out the inadequacy of standard procedures for fitting distributions and judging the goodness of fit and introduces the basic notions of a superior method. Section IV finally applies the techniques described in Section III to the various industries and analyzes the results.

I. *Aggregation and Economic Considerations*

A basic question that has to be answered before an actual sample of observations can be chosen is what the proper level of aggregation is in a study which attempts to explain the size distribution of firms. A particular distribution explains the size distribution of firms only in that it is the limiting outcome of the stochastic process which generates the birth, growth, decline, and death of individual firms.[3] Clearly, any such sto-

* The author, professor of economics at Princeton University, is greatly indebted to Stephen M. Goldfeld, John Tukey, and Herbert Simon for many ideas and constructive suggestions. All errors are, of course, his own responsibility. Acknowledgment is made to the National Science Foundation grants NSF G 24462 and NSF-GP 579 for support.

[1] See [6] [15].

[2] See [1] [7] [8] [10] [11] [14].

[3] One may debate what the proper measure for a firm's size is. It is not the purpose of this paper to add anything to that debate. We measure size in terms of assets in all but two of the samples discussed subsequently. In the case of the two exceptions we use sales as the measure.

chastic process has a set of parameters which characterize it completely; the resulting empirical frequency function of asset sizes must in some sense reflect the parameters of the generating process.

In general one does not know the nature of the stochastic process which generates an industry. Nor is the generating process normally observable directly. We cannot, for example, assume without some solid evidence that firm growth is governed by a Markov process. But in fact this often is assumed, and even as detailed and excellent a study as that of Hart and Prais [6] must content itself in that regard with an analysis of one-period transition matrices[4] containing the estimated probabilities that a given firm will transit from each size category to each other size category. Empirically observed transition matrices do not, however, imply anything conclusive about the manner in which the observations are generated. The study by Simon and Bonini [15] undertakes a direct test of the Law of Proportionate Effect,[5] and their sample confirms this hypothesis. The study of Hart and Prais further confirms it [6, p. 170]. But since we can observe the stochastic process which generates an industry only indirectly, an attempt at detailed verification of such hypotheses as those mentioned above either must be based on the assumption that the same stochastic process applies to all firms or it must rest on some reasonable criteria of disaggregation so that a given stochastic process can be thought to apply to all firms in the disaggregated sector.

The study of Simon and Bonini deals, for the most part, with highly aggregative data such as the 500 largest firms in 1955 as published by *Fortune*. Hart and Prais use figures which are equally unselective, in that they choose firms whose shares were quoted on the London Stock Exchange. The use of firms which encompass the entire economy rather than the use of firms restricted to some particular industry is justified by Simon and Bonini on two grounds: (1) that their stochastic growth model makes no reference to any specific features of cost functions—except that costs are constant above some minimum point; and (2) that if firms in each industry are distributed according to the Pareto distribution with slopes close to one, so will firms in the composite industry obtained by amalgamating individual industries [15, pp. 612–13].[6]

Neither of these arguments is fully convincing. The former omits the fact that—whatever specific models may assume—in reality cost functions are likely to be different and conditions of entry and exit are likely to be different, not only among but within industries. The latter

[4] The period is fairly long in the Hart and Prais study, ranging from 11 to 15 years.

[5] A random variable is said to obey the Law of Proportionate Effect if it undergoes random proportionate changes from period to period [15, p. 612].

[6] By slope the authors refer to the a in the formula for the Pareto distribution $F(x) = 1 - (k/x)^a$.

suffers from ambiguity in that it is not clear how individual industries are to be defined in order to yield Pareto distributions with comparable slopes; clearly there always exists *some* definition of industries such that the resulting asset size distributions within each are not Pareto distributions at all. The question then is whether such definitions are more or less reasonable with respect to the homogeneity of firms contained in the specific industries than some alternative definitions might be. We can therefore argue that (1) we have no assurance a priori that various industries will have Pareto distributions with comparable slopes, and (2) any assumption about the comparability on an interindustrial basis of the parameters of the distribution of firms must be based on an explicit model of how industries are generated.

For illustrative purposes consider the simplest of Champernowne's [1] models of income distribution and assume that, *mutatis mutandis*, it is to be applied to the asset distribution of firms. The model assumes that a denumerable set of proportional asset (income) classes is given with transition probabilities governing the movement of assets from class to class. It is assumed (1) that the probability of an asset shifting by k classes depends only on the number k and not on the absolute initial level of assets; (2) that upward shifts involve at most one asset class; and (3) that the expected number of transitions for all assets is negative.[7] Under these assumptions it can be shown that the limiting distribution of asset (income) sizes is the Pareto distribution exactly. Even if all the assumptions of this attractive model are retained, what guarantee or what reason for belief does one have that the transition matrix in one industry will be the same, or nearly the same, as in another? In general one cannot assume the interindustry stability of transition matrices. If one is willing to hypothesize, contrary to what some have held, that cost and demand functions have something to do with the manner in which industries develop, one may arrive at a model of industry development in which transition matrices depend on the following factors:

1. The nature of the short-run cost function. The more steeply the short-run marginal cost function rises in the neighborhood of the profit-maximizing point, the less likely are significant increases in output; but what is perhaps more important, the greater is the penalty for not achieving the optimum. Thus failure to reach the optimum has a higher probability of causing a downward transition. Of course, if short-run marginal cost curves were typically horizontal, no interindustry differences would arise from this source.

2. The nature of the long-run cost function, the general ease or difficulty of expanding plant and equipment—including such items as the relation between increases in demand which are smooth and increases

[7] This assumption assures that the process is nondissipative.

in output which, by the nature of the relevant technology, may be achieved only in discrete steps.

3. The nature of oligopolistic arrangements—or the absence thereof—in a given industry.

4. The general configuration of competing products, changes in relative technology, and changes in relative demands. It would indeed be surprising if individual industries behaved substantially similarly, irrespective of wide differences among the underlying forces that generate them. But it is not even plausible to suggest that only the values of certain parameters will be different; in all likelihood the nature of the stochastic process itself will differ from case to case. Accordingly it would be surprising if the same distribution (with different estimated values for the parameters) were to fit all cases.

These considerations clearly suggest the dangers in using aggregated data drawn from several sectors of the economy. These data, having been generated in part by one sort of mechanism and in part by another, cannot be expected to yield reliable results on the whole. In particular, the distribution of firms in a composite industry may be quite different from the distributions of the individual industries which are the sources of the composition.

II. *Data and Distributions*

Selection of Samples. Since there are no criteria for defining an industry which are free from arbitrariness, it seemed sensible to use definitions commonly used and accepted. With the exception of the two composite samples of the *Fortune* lists of the 500 largest firms in the United States in 1955 and 1960, the samples chosen represent industries according to 4-digit S.I.C. classes. The list of actual industries selected is displayed in Table 1.[8] The sizes of firms are measured in terms of assets. The 1963 Dun and Bradstreet *Million Dollar Directory* was the source for the firms in each classification. Actual asset figures were procured from the various relevant *Moody's Manuals*.[9] If a given firm listed in Dun and Bradstreet was not listed in Moody's, it was eliminated from the sample. Similarly we eliminated firms for which assets were not listed in Moody's or which were listed as subsidiaries of some parent firm and for which no separate balance sheet was provided. These restrictions, together with our reliance on the *Million Dollar Directory*, tend to eliminate the small firms. Although it has been argued in the literature that it is basically the right tail of the distribution which is of prime interest, the selection procedure is thus not free of arbitrariness.

[8] We also display abbreviated code names given to them, which were retained throughout the computations as having higher mnemonic value than S.I.C. numbers.

[9] *Moody's Industrial Manual*, 1962, 1963; *Moody's Bank and Finance Manual*, 1963; *Moody's Transportation Manual*, 1962. Some of our samples are thus from 1962 and some from 1963.

TABLE 1—LIST OF INDUSTRIES

S.I.C. Number	Name	Code Name
1311	Crude Petroleum and Natural Gas	CRUDPET
6000	Banks (Massachusetts)	MASSBANK
6000	Banks (Illinois)	ILLBANK
6000	Banks (New York)	NYBANK
1511	General Building Contractors	BILDCONT
2011	Meat Packing Plants	MEATPACK
2082	Malt Liquors	MALTLICK
2421	Sawmills and Planing Mills	SAWMILL
2621	Paper Mills	PAPERMIL
2731	Books—Publishing and Printing	BOOKS
2818	Industrial Organic Chemicals	CHINDORG
2819	Industrial Inorganic Chemicals	INDINORG
2851	Paints, Varnishes, Lacquers, and Enamels	PAVALAC
2911	Petroleum Refining	PETROREF
3069	Fabricated Rubber Products	FABRUB
3141	Footwear	FOOTWEAR
3312	Blast Furnaces	BLASTFUR
3321	Gray Iron Foundries	GRIFOUND
3441	Fabricated Structural Steel	FABSTEEL
3522	Farm Machinery and Equipment	FRMEQUIP
3571	Computing and Accounting	CACTMACH
3621	Motors and Generators	MOTORGEN
4213	Trucking	TRUCKING
5064	Electrical Appliances, TV, Radio	ELECTAPP
1421	Crushed and Broken Stone	CRUBROST
2033	Canned Fruits, Vegetables, Preserves, etc.,	CANNING
3461	Metal Stamping	METSTAMP
3679	Electronic Components and Accessories	ELECTCOM
4011	Railroads, Line Haul Operating	RAILROAD
6300	Insurance Companies	INSUR

In particular, the selection criteria did not take into account possible relationships between the size of firm and the shape of the long-run marginal cost curve. It is possible, therefore, that some of the data are partially contaminated by the inclusion of firms which are too small. The particular industries chosen were not selected at random; there was at least a casual attempt to obtain a set of industries with differing numbers of firms in them, differing degrees of product homogeneity, and representing different types of production processes. The banking industry contains so many firms that, for computational ease, we consider as three separate industries banks in Massachusetts, Illinois, and New York.

Distributions Fitted. The statistical distributions mentioned perhaps most frequently as "explaining" the size distribution of firms (or of incomes, or of city sizes, or of any number of more or less peculiar socioeconomic quantities) are the Pareto distribution and the lognormal

distribution. Pareto himself advanced three distributions,[10] and the one commonly referred to as the Pareto distribution, more properly called the Pareto distribution of the first kind, is given by

$$F(x) = 1 - \left(\frac{k}{x}\right)^a, \tag{1}$$

where $F(x)$ is the probability of an observation having a value $\leq x$ and where k and a are parameters. We also employed the Pareto distribution of the second kind:

$$F(x) = 1 - \frac{K}{(x + c)^a}, \tag{2}$$

and a simplified form of the Pareto distribution of the third kind, known as the Champernowne distribution,[11] given by

$$F(x) = 1 - \frac{Ke^{-bx}}{x^a}. \tag{3}$$

Two additional distributions can be derived from equations (1) and (3), respectively, on the basis of the following—admittedly artificial—assumptions: (1) Imagine that we define a set of firms with outputs which are highly substitutable for each other in production as a "primitive" or "noncomposite" industry. We then form a "composite" industry by taking the largest firm in each of a number of noncomposite industries and adjoining them to each other.[12] (2) Each noncomposite industry has an identical number of members. (3) Firms in each noncomposite industry are distributed according to identical distributions (1) or (3). From these assumptions, it follows that firms in the composite industry are distributed approximately as

$$F(x) = e^{-m(k/x)^a}, \tag{4}$$

which we shall call the composite distribution, and

$$F(x) = e^{-m(k/x)e^{-bx}}, \tag{5}$$

which shall be called the double or iterated exponential distribution. Finally, as mentioned above, we fitted the lognormal distribution where for convenience we chose the two-parameter family given by

$$F(x) = \int_0^x \frac{e^{-1/2[(\log \xi - \mu)/\sigma]^2}}{\xi\sigma\sqrt{2\pi}}\, d\xi. \tag{6}$$

[10] See [9].
[11] See [15].
[12] This assumption may not be a priori as crude as it sounds for the two *Fortune* samples.

All of these distributions are, subject to specific subsidiary assumptions such as those used in deriving (4) and (5), broadly consistent with the Law of Proportionate Effect. In fitting all of these distributions to all our samples, we are in fact making a twofold query: (1) Does at least one of these distributions fit well in each case and is the prevalence of the Law of Proportionate Effect thus upheld? (2) Can we discriminate successfully among various distributions and the fits they produce if several distributions fit reasonably well, as is to be expected?[13]

The Criterion of Statistical Significance. It is important to note that this study concentrates attention on statistical significance. The acceptability of a hypothesis thus is based upon the absence of statistically significant discrepancies between the hypothesis and the data.

This is clearly not the only way to regard the matter. The hypothesis that the generation of industries is explained by the Law of Proportionate Effect or, indeed, the individual hypotheses represented by (1) to (6) are what may be called *extreme hypotheses*,[14] i.e., hypotheses that nobody would wish to accept literally. In analyzing the discrepancy between an extreme hypothesis and the facts it may be more relevant to concentrate on the degree to which the discrepancy *is of consequence* for, say, economic theory than the extent to which it is *statistically significant.* The notion of being of consequence is the more relevant one when we are interested in how good overall an approximation is. Thus a discrepancy may be statistically significant but of no consequence. The emphasis of this paper on statistical significance is justified in the following ways: (a) It is difficult to speak of consequential differences—as opposed to statistically significant ones—without specifying a utility function over the possible discrepancies between theory and facts; (b) relatively gross hypotheses such as the Law of Proportionate Effect can be embodied in a number of ways in particular different mathematical formulations among which it is desirable to discriminate; (c) successful statistical discrimination tends to yield reformulated theories of relatively greater sophistication.

III. *The Theory of Fitting Distributions*

In this section we briefly review standard methods of fitting distributions to samples and of judging the goodness of fit and introduce what, so far, promises to be a superior method.

Typically methods of fitting fall into the following categories: (1) The

[13] Some skewed distributions would not be expected to fit well a priori. An example is the exponential distribution which, it can be shown, has a theoretical value for the Lorenz measure of the inequality of the distribution which is independent of the parameter of the distribution. In fact, the exponential distribution produces abysmally bad fits to our samples.

[14] See [12, pp. 254–56].

method of moments or quantiles, according to which some theoretical sample moments (or quantiles) are set equal to their sample values, and the resulting equations are solved for the parameters of the distribution; (2) the method of maximum likelihood, which assigns to the parameters of the distribution such values as will maximize the likelihood of the sample actually obtained; (3) the method—employable only if some appropriate transform of the cumulative distribution can be expressed as a straight line—which consists of fitting a straight line to the sample cumulative distribution, appropriately transformed;[15] (4) qualitative methods such as inferring something about the class of admissible or inadmissible distributions from the shape—symmetry versus asymmetry—of the Lorenz curve.[16] Discounting the fourth class of methods as being generally too imprecise for purposes of fine discrimination, we have to admit that each of the other three methods may, under appropriate circumstances, have various desirable statistical properties such as providing estimates which are unbiased or consistent or have some other desirable feature.

Once the parameters of the distribution in question have been estimated, we still face the task of judging the goodness of fit. This is typically accomplished by the χ^2 test according to which data are grouped into r classes, the expected frequencies e_i and actual frequencies f_i in each class are obtained, and the statistic

$$G = \sum \frac{(e_i - f_i)^2}{e_i}$$

is calculated which has approximately the χ^2 distribution with $r-k$ degrees of freedom where k is the number of parameters fitted. The test is obviously unsuitable where fine discrimination is required for the following reasons:

1. The test criterion G is sensitive to the particular grouping selected for the observations and in the case of closely similar hypotheses the answer might depend on what grouping was chosen.

2. The test is not as powerful as it might be if its validity rested on the assumption of some specfic alternative hypothesis; but it is pre-

[15] H. S. Simon and C. P. Bonini [15] employ this technique.

[16] It can be shown quite easily that the Lorenz curve associated with the Pareto distribution of the first kind, defined only for values of $a>1$, is asymmetric. These methods are imprecise, at best, since this property is shared by many other distributions. If, however, the Lorenz curve were symmetric, we may be fairly sure that the sample does not fit the Pareto distribution well. An equally dubious qualitative method is to take sequentially increasing samples and examine the resulting sequence of sample first, second, etc., moments. If these sample moments show a tendency *not* to settle down, but to increase indefinitely with increasing sample size, we may be on fairly good ground to suggest that we are dealing with a sample from a distribution with infinite moments. Unfortunately this method does not tell us which of the many such distributions we may be dealing with.

cisely this kind of situation that is encountered when we wish to choose between, say, the Pareto and the lognormal distributions.

3. The test cannot be validly applied if expected frequencies are very small;[17] this, however, is precisely the case when we are interested in distributions with very long right tails.

4. The value of G may be small, indicating a good fit, but the fitted cumulative distribution and the sample cumulative distribution may still show very significant, systematic divergences from each other.

Points 2 and 4 can be equally well used as a critique of the Kolmogorov-Smirnov test, together with the added problem that critical values of the Kolmogorov-Smirnov statistic are not known in the case in which the parameters of the distribution have actually been estimated from a sample.[18]

Because of the variability in the properties of the estimates by the various methods mentioned earlier and because of the great difficulty of achieving subtle discrimination among alternative hypotheses, a new, unified method of estimating and of testing goodness of fit has been developed.[19]

This method considers the values of the cumulative distribution at the n sample points x_i, denoted by $F(x_i)$. We assume that the x_i's are order statistics; that is, that the sample has been so reordered that $x_i \leqq x_j$ if $i<j$. By convention $F(x_0)=0$ and $F(x_{n+1})=1$. We form:

$$S = \sum_{i=1}^{n+1} \left(F(x_i) - F(x_{i-1}) - \frac{1}{n+1}\right)^2$$

and choose as our estimates of the parameters of the distribution those values which minimize S.[20] The resulting estimates can be shown to be consistent.[21] It is to be noted that in general this method works for all distributions and is nonparametric in that the properties of the estimates do not depend upon the particular distribution in question.[22]

Goodness of fit is tested in two basic ways and a fit is pronounced bad if it fails by either test. The two criteria are the *closeness* of the fit as measured by the value of S at the minimum (the smaller is S, the better is the fit) and by the randomness of the fit which is measured

[17] There has been some debate as to what constitutes "small" expected frequencies. See [2].

[18] See, for example [1].

[19] See [10] and [11].

[20] Clearly this involves possibly tricky numerical minimization problems. For an effective solution of these problems which was not yet available when these computations were performed, see [4].

[21] See [10].

[22] The reason being that the quantities $F(x_i)-F(x_{i-1})$, called the coverages associated with the order statistics, have identical joint uniform distribution, irrespective of the form of the distribution of x.

by the randomness of the successive residuals $F(x_i)-F(x_{i-1})-1/(n+1)$, $F(x_{i+1})-F(x_i)-1/(n+1)$, etc. The rationale for testing the residuals for randomness is simple: under the null hypothesis, that is if the data really were generated by the distribution fitted, we would expect some residuals to be positive, some negative, in no predictable order. A systematic deviation from randomness, even with a small S, therefore indicates a bad fit. Randomness itself can be tested in several ways which will be discussed when we turn to an analysis of the data in the next section.

IV. *Results of Fitting*

It has been shown elsewhere that the standard methods are generally not capable of fine discrimination.[23] In this section we discuss only the results of our new method. Table 2 displays the estimated values of S. The first column gives the number of observations for each sample. Asterisks indicate that the value of S is so high that on the .05 level of significance we must reject the possibility of a good fit.[24]

Before we can interpret Table 2 we must note the anomaly that in a few instances the Pareto distribution of the first kind fits better than that of the second kind or that the composite distribution fits better than the double exponential. This may appear as paradoxical since these pairs of distributions are essentially of the same form, with one of them having one additional parameter to be fitted. The one with more parameters to be fitted should yield the better fit.[25] The answer is that the minimization of S was carried out by numerical methods; hence we cannot be sure that in each case we have reached a global minimum. In fact, during the computations there was occasional evidence of multiple minima—or at least of very flat plateau-type minima—in several instances. In these instances direct search of the parameter space was undertaken until a "reasonable" value of S seemed to have been produced.[26]

[23] See [11].

[24] Critical values for S were derived from sampling experiments in which the distribution was fitted when the null hypothesis was known to be true. See [10]. Linear interpolation was used for adapting the calculated critical values to the present values of N.

[25] Note also that these considerations affect the validity of using significance levels of, say, S, derived by fitting only two parameters for testing closeness of fit in a three-parameter case. It is quite likely, however, that when the number of observations is large, this will cause a negligible distortion.

[26] Clearly the procedure does not guarantee that a global minimum had been reached. This may cast some doubt on the statements of statistical significance made in the rest of the paper. On the whole, however, we may be confident that the results are reasonably accurate because (a) when direct search of the parameter space was undertaken, it was a fairly thorough search, consisting of picking a new starting point and performing a new set of iterations leading to a—hopefully—smaller minimum; (b) after an "acceptable" value of S was produced, the

TABLE 2.—ESTIMATED VALUES OF S

	N	Pareto 1	Pareto 2	Champer-nowne	Com-posite	Double	Log-normal
CRUDPET	159	.0185*	.0084	.0101*	.0068	.0065	.0075
MASSBANK	191	.0311*	.0106*	.0073	.0063	.0100*	.0068
ILLBANK	161	.0125*	.0906*	.0125*	.0063	.0206*	.0080
NYBANK	253	.0368*	.0149*	.0060*	.0041	.0092*	.0044
BILDCONT	30	.0656*	.0354	.0435*	.0344	.0337	.0345
MEATPACK	25	.0559*	.0639*	.0446*	.0426	.0417	.0450*
MALTLICK	26	.0570*	.0412	.0330	.0357	.0318	.0342
SAWMILL	24	.0383	.0146	.0153	.0135	.0109	.0115
PAPERMILL	48	.0663*	.0263*	.0334*	.0239	.0238	.0248
BOOKS	36	.0242	.0543*	.0145	.0170	.0148	.0151
CHINDORG	47	.0422*	.0173	.0185	.0173	.0178	.0154
INDINORG	56	.0321*	.0244*	.0177	.0217	.0170	.0180
PAVALAC	43	.0147	.0300*	.0140	.0224	.0222	.0250
PETROREF	55	.0565*	.0215	.0162	.0195	.0137	.0144
FABRUB	50	.0231	.0164	.0193	.0152	.0296*	.0179
FOOTWEAR	22	.0587*	.0536*	.0526*	.0469*	.0472*	.0537*
BLASTFUR	71	.0712*	.0098	.0106	.0092	.0092	.0097
GRIFOUND	36	.0872*	.0378*	.0278	.0330	.0255	.0292
FABSTEEL	36	.0181	.0186	.0158	.0148	.0157	.0234
FRMEQUIP	38	.0211	.0275	.0184	.0214	.0216	.0309
CACTMACH	27	.0243	.0443*	.0244	.0275	.0276	.0326
MOTORGEN	39	.0244	.0166	.0172	.0130	.0126	.0151
TRUCKING	41	.0588*	.0369*	.0220	.0260	.0168	.0216
ELECTAPP	18	.0495*	.0367	.0443*	.0271	.0327	.0343
CRUBROST	17	.0223	.0290	.0223	.0273	.0318	.0338
CANNING	24	.0447*	.0520*	.0272	.0369	.0310	.0358
METSTAMP	58	.0217	.0108	.0120	.0106	.0105	.0110
ELECTCOM	119	.0254*	.0118*	.0163*	.0089	.0088	.0115*
RAILROAD	168	.0285*	.0116*	.0104*	.0091	.0078	.0095*
INSUR	602	.0375*	.0019	.0023*	.0018	.0018	.0018
FORTUNE 5	500	.0020	.0020	.0019	.0029*	.0031*	.0041*
FORTUNE 6	500	.0021	.0019	.0020	.0026*	.0030*	.0038*

Table 2 provides us with the following conclusions:

1. The S-values tend to discriminate among the various distributions fairly well. Considering each sample to be a judge who ranks the six candidate distributions, we find Kendall's coefficient of concordance $W=.303$, which is significant on the .001 level. In particular, out of the 32 samples the Pareto distribution of the first kind is rejected in 20 cases, the Pareto distribution of the second kind in 15 cases, and the Champernowne distribution in 11 cases. The other three seem to do much better, being rejected not more than seven times. As potential candidates for general description we thus have only the composite,

search was continued until several new tries failed to produce a smaller minimum; (c) the search was continued until the results showed that the "acceptable minimum" could be achieved from various starting points.

iterated exponential and lognormal distributions left, and we can safely reject the hypothesis that all candidate distributions fit equally well.

2. One of the samples, the Footwear Industry, is not fitted well by any distribution.

3. The Pareto distributions of the first and second kinds and the Champernowne distribution do well for the two *Fortune* samples, for which the composite and iterated exponential distributions—the derivations of which were inspired by the conjectured construction of those samples—do rather badly. The assumptions underlying those derivations seem not to correspond to reality in those cases.

4. In spite of conclusion 1, it is interesting to note that each of the six distributions produces a "best fit" for at least one sample.

5. On a fairly casual basis one might argue that the S values for the last five distributions indicate discrepancies between hypothesis and facts which are of not great consequence; the same cannot be argued easily for the Pareto distribution of the first kind which exhibits much larger S values on the whole.

In the aggregative Table 3 the letter S signifies that the particular sample distribution pair failed the test of closeness at the .05 level of significance.

Three methods have been employed to test the randomness of the residuals. Clearly the results of the three are not independent of each other, but their relative sensitivities differ, and so all three were employed. We further note that nothing is known at present about the power of these tests. Therefore, since most instances in which the null hypothesis is false will tend to induce some nonrandomness of residuals, we cannot analyze questions about the probability that the hypothesis will be rejected when it is false. These methods can be described briefly as follows:

1. Runs of negative and positive residuals were counted and the standard-runs test was performed on each set of residuals at the .05 level of significance.[27] The letter R in Table 3 indicates that the particular sample-distribution pair failed the runs test of randomness.

2. Orthogonal polynomials of 15th degree were fitted to the various series of residuals.[28] If the residuals contain low-frequency oscillations only, the fit will be good, and the percentage reduction in the total sum of squares of residuals owing to fitting the polynomial will be large. If the series of residuals contain only very-high-frequency oscillations, the reduction in the sum of squares will tend to be small. For random series the percentage reduction in the sum of squares will be of intermediate size. The letter O in Table 3 indicates that, on the .05 level of signifi-

[27] See [8].

[28] See [3] and [5].

TABLE 3

	Pareto 1	Pareto 2	Champer-nowne	Com-posite	Double	Log-normal	N
CRUDPET	S OP	OP	S OP				159
MASSBANK	S OP	S OP	R		SROP	R	191
ILLBANK	SROP	SRO	SROP		SROP	O	161
NYBANK	SROP	SROP	SROP		SROP	O	253
BILDCONT	S O		S				30
MEATPACK	S O	S O	S O	O	O	S O	25
MALTLICK	SROP		ROP	O	O	O	26
SAWMILL	RO						24
PAPERMIL	SRO	S	S				48
BOOKS	R P	SRO					36
CHINDORG	SROP						47
INDINORG	S OP	S P					56
PAVALAC		SROP				R P	43
PETROREF	SROP	P		O			55
FABRUB	OP		OP		S OP		50
FOOTWEAR	S	S	S	S	S	S	22
BLASTFUR	SROP		RO			R	71
GRIFOUND	S O	S O					36
FABSTEEL		O		RO	O		36
FRMEQUIP							38
CACTMACH		S					27
MOTORGEN	P		P				39
TRUCKS	SROP	SR					41
ELECTAPP	S O	O	S O	O	O	O	18
CRUBROST	O	O	O	O	O	O	17
CANNING	S	S					24
METSTAMP	OP						58
ELECTCOM	S OP	S O	SROP			S OP	119
RAILROAD	S OP	S P	S OP			S P	168
INSUR	SROP	O	SROP			P	602
FORTUNE 5	OP	OP	P	SROP	S OP	SROP	500
FORTUNE 6	O			SROP	S OP	SROP	500

cance, the percentage reduction in the sum of squares deviated from randomness.[29]

3. The power spectrum of each series of residuals was calculated and examined by comparison with the power spectrum of series of comparable length generated when the null hypothesis was in fact true[30]—for significant deviations from the characteristics of the spectrum of white noise—which is indeed the appearance of all the spectra generated under the null hypothesis. The letter P is entered in Table 3 where significant amount of power was found at the low frequencies. It should

[29] Critical values for the percentage reduction were established by extensive sampling experiments in which the null hypothesis was in fact true. In all but five cases rejection in the above table occurred because of low-frequency oscillations. See [10].

[30] See [10].

be noted that the spectral representation is not very meaningful or informative for short series, say for series of length much less than 50. For illustrative purposes we show the spectra for the sample *Fortune* 6 in Figure 1. The first three of these spectra are indistinguishable from those of series of comparable length for which the null hypothesis is true. The last three exhibit noticeable peaks at the low frequencies, indicating nonrandomness.

Some basic conclusions that can now be drawn from Table 3 are as follows:

1. We disqualify a distribution as providing a good fit if it fails by either the criterion of closeness or the criterion of randomness. We shall base our conclusions concerning randomness on the test using orthogonal polynomials. On this basis we obtain the following ranking: (a) the composite distribution (23 good fits), (b) the iterated exponential and lognormal distributions (20 good fits), (c) the Champernowne distribution (17 good fits), (d) the Pareto distribution of the second kind (11 good fits), (e) the Pareto distribution of the first kind (6 good fits). The ordering of the composite and iterated exponential distributions is paradoxical and is probably explained by the computational artifact mentioned earlier. The Champernowne and the Pareto distribution of the second kind have the same number of parameters, hence the former is clearly better; the Pareto distribution of the first kind has one fewer parameter and one might attempt to explain its bad performance in terms of that fact. This would not be satisfactory, however, since both the lognormal and the composite distributions have the same number of parameters as the Pareto distribution of the first kind and yet perform markedly better.

2. Although there is considerable difference in the number of instances in which the various tests of randomness "catch" bad fits, the runs test catching 34 cases, the spectral test 50, and the orthogonal polynomial test 78, there is a very marked overlap in the cases they do catch. One way of verifying this is as follows: Consider a particular pair of tests, say those denoted by the letters R and P. Let the number of occurrences of these letters in Table 3 be N_R and N_P, i.e., $N_R=34$, $N_P=50$. On the null hypothesis that the N_R R's and N_P P's are distributed randomly in Table 3, the probability of k or more matches in Table 3 (instances in which R and P occur together) is

$$P(k) = \sum_{j=k}^{N_R} \frac{\binom{N_P}{j}\binom{192 - N_P}{N_R - j}}{\binom{192}{34}} .$$

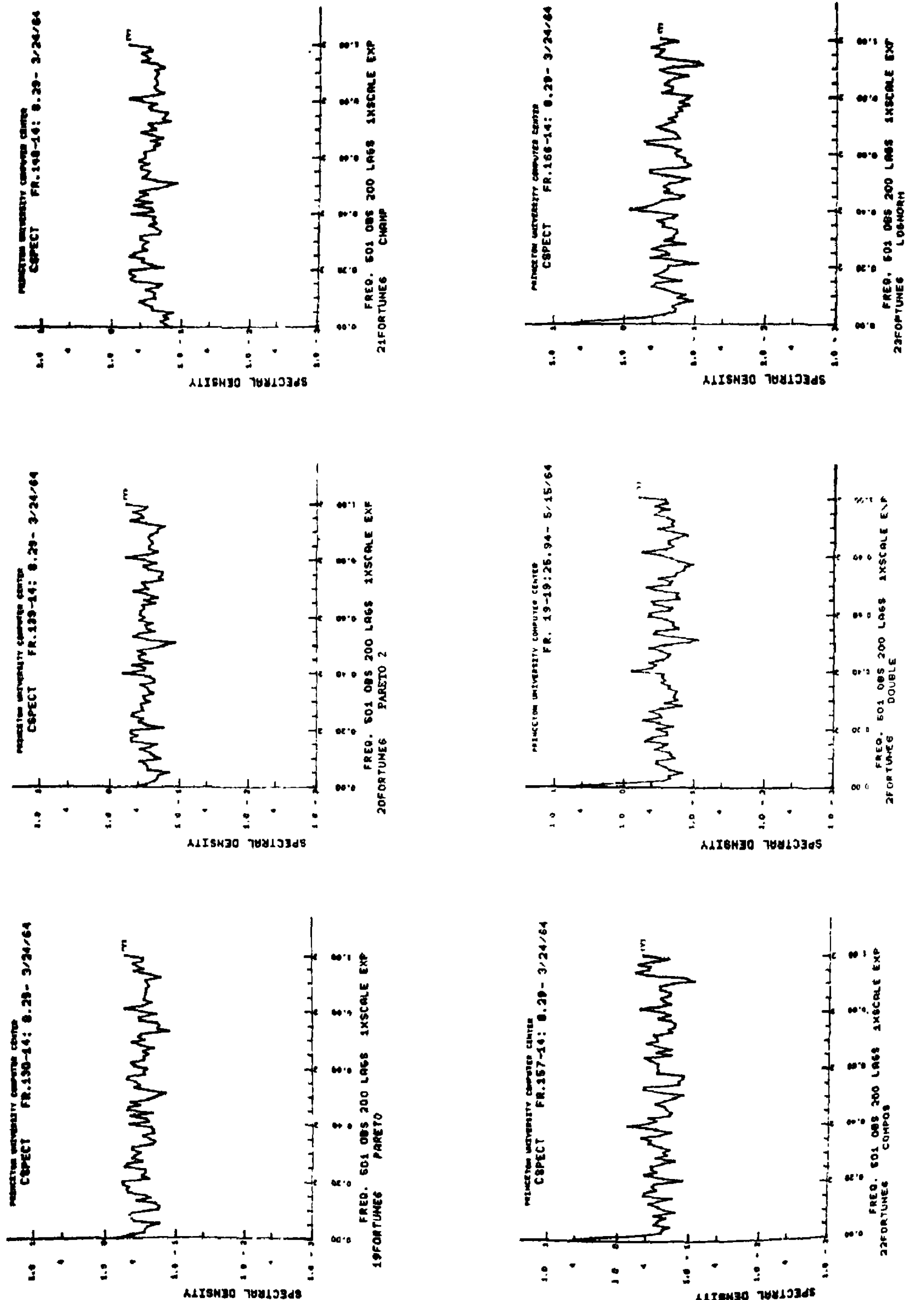


FIGURE 1

For all three possible comparisons this probability is less than 10^{-6}, and we emphatically reject the hypothesis that these tests of randomness catch cases at random.

3. There are four cases where none of the distributions fits well enough. In addition, the Pareto distribution of the first kind does not fit the sample *Fortune* 5 well on the grounds of nonrandom residuals.[31]

4. Discrimination is far from perfect. Basing our judgment of a good fit on the nonappearance of the symbols S and O in Table 3, we find 15 samples which are fitted well by four or more distributions. However, all of these are samples where the number of observations is less than 60. Discrimination becomes much greater when the number of observations is large. In fact, the number of times the symbols S, R, O, P occur in Table 3 is 224. One hundred ten of these occurrences are in the nine samples with more than one hundred observations. The unhappy conclusion from this is that we may not be able to test hypotheses involving subtle discrimination or we may not be able to test even grosser hypotheses—such as the Law of Proportionate Effect—without much larger samples than are typically available. The fact that good fits are fewer when the samples are large suggests that if we had large samples for all our industries, we would generally find many fewer good fits than is the case.

5. The composite distribution and the iterated exponential distribution, the derivation of which was inspired by the hypothetical construction of the *Fortune* samples, do not fit those samples well at all.

V. *Concluding Remarks*

Fitting distributions and testing the goodness of fit in the manner outlined in Sections II and III lead to fairly good discrimination in many cases among closely similar alternatives. In particular the Pareto distribution of the first kind generally was seen to provide very bad fits. This result contradicts in two ways the view that (a) firm sizes have the Pareto distribution, and (b) that the distributions are not very sensitive to the methods by which the samples are obtained [8, p. 425].

Although the analysis does not suggest that discrepancies between the predictions of the (unadorned) Law of Proportionate Effect are of great consequence, there is statistical evidence that we can discriminate between alternative guises in which the Law may appear. In all likelihood the Law of Proportionate Effect operates together with such complicated conditions on birth and death as to be incapable of yielding a pure test of the law itself. Finally, it is very likely that in

[31] This sample had been found to agree well with the Pareto distribution earlier. See [15].

many instances we shall be unable to test hypotheses about the distribution of the data, and thus about the mechanism generating them, because of the smallness of the number of observations. In these instances an unavoidable agnosticism may previal.

REFERENCES

1. D. G. CHAMPERNOWNE, "A Model of Income Distribution," *Econ Jour.*, June 1953, *63*, 318–51.
2. W. G. COCHRAN, "Some Methods for Strengthening x^2 Tests," *Biometrics*, 1954 (10), 417–51.
3. R. A. FISHER, *Statistical Methods for Research Workers*, 12th ed. London 1954.
4. S. M. GOLDFELD, R. E. QUANDT, and H. F. TROTTER, "Maximation by Quadratic Hill-Climbing," *Econometric Research Program*, Research Memo. No. 72, Princeton University, January 1965; also in *Econometrica*, forthcoming.
5. C. H. GOULDEN, "*Methods of Statistical Analysis*, 2nd ed. New York 1952.
6. P. E. HART AND S. J. PRAIS, "An Analysis of Business Concentration," *Jour. Royal Stat. Soc.*, Ser A., 1956, *119*, Pt. 2, 150–91.
7. M. KALECKI, "On the Gibrat Distribution," *Econometrica*, April 1945, *13*, 161–70.
8. B. MANDELBROT, "New Methods in Statistical Economics," *Jour. Pol. Econ.*, Oct. 1963, *71*, 421–40.
9. V. PARETO, *Corso di Economia Politica*, ed. by G. Einaudi, 1949.
10. R. E. QUANDT, "Old and New Methods of Estimation and the Pareto Distribution," *Metrika*, 1966, *10*(1), 55–82.
11. ———, Statistical Discrimination Among Alternative Hypotheses and Some Economic Regularities," *Jour. Reg. Sci.*, Winter 1964, *5*, 1–23.
12. L. J. SAVAGE, *The Foundations of Statistics*. New York 1954.
13. S. SIEGEL, *Nonparametric Statistics*. New York 1956.
14. H. A. SIMON, "On a Class of Skew Distribution Functions," *Biometrika*, 1955, *42*, Pts. 1 and 2, 425–40.
15. ——— AND C. P. BONINI, "The Size Distribution of Firms," *Am. Econ. Rev.*, Sept. 1958, *48*, 607–17.

DYNAMIC INCOME ADJUSTMENT TO MONEY-SUPPLY CHANGES

By DONALD P. TUCKER*

Recent empirical studies have provided evidence that aggregate investment spending responds to changes in the level of interest rates, but that it responds only gradually, with a considerable distributed lag. Some writers have drawn from this the conclusion that monetary policy, to the extent that its effectiveness comes from its power to influence investment through interest rate changes, works very slowly and cannot be used for cyclical stabilization. This conclusion is not justified, for long distributed lags in investment response can exist without causing comparable lags in the response of aggregate demand to changes in the money supply. The lags and parameter values in other sectors of the economy have fully as much influence as those in investment in determining the dynamics of the system. In particular, lags in the money-demand function tend to counteract, rather than reinforce, the investment lags. In this paper, a dynamic Keynesian income model characterized by distributed lags in the behavioral equations is analyzed to demonstrate this point.

One of the crucial questions for monetary policy is the question of speed of adjustment, of how rapidly production and income respond to changes in the money supply. At issue is whether attempts at countercyclical management of the money supply are stabilizing or destabilizing, for monetary policy may, in fact, aggravate business cycles if its effects are mostly felt many months after money-supply changes occur. One of the economic mechanisms through which a change in the money supply is thought to influence real national income operates through the interest rate. Investors base their spending decisions in part, according to this Keynesian argument, on the current and expected future levels of interest rates. When these change in response to a money-supply change, then investment also changes, inducing a corresponding change in consumption spending. Clearly, then, one factor of major importance to the question of the speed of monetary policy is the speed with which investment spending responds to interest rate changes.

In recent econometric studies of investment the most satisfactory results have often been obtained with models in which investment re-

* The author, assistant professor of economics at the University of Chicago, is indebted to the members of the Workshop in Econometrics and Mathematical Economics at the University of Chicago for their suggestions on an earlier draft of this paper.

sponds with a distributed lag to changes in the variables on which depends. Because of its simplicity, the form

$$I_t = a + b_1X_{1,t} + b_2X_{2,t} + \cdots + cI_{t-1}$$

(where X_i is the ith independent variable and t is a time subscript) commonly employed. This form, as is well known, is implied by ε investment function in which current investment depends on all cu rent and past values of the independent variables with geometrical declining coefficients. The coefficient of some variable t periods ago proportional to c^t, where $0 \leq c < 1$. Thus, the lag coefficient, c, is ε indicator of the speed of adjustment. If c is close to zero, investmeı depends hardly at all on the values of variables more than a couple ‹ periods back, and adjustment is rapid. A large c means slower adjus ment.

Several investment studies utilizing such a lag model have foun both a significant coefficient for some interest rate variable and moderately large lag coefficient [3] [5] [7, Ch. 7], thus indicating thε investment responds directly to monetary stimuli, but only with distributed lag. It is an easy and intuitively appealing application ‹ the Keynesian framework to judge from such evidence that monetar policy works slowly on the economy, and conclusions of this sort hav occasionally appeared in the literature [1, Ch. 12] [2] [3] [6]. Th deduction required to reach this conclusion is not a valid one, howeve: The existence of lags in investment response is not a sufficient conditio for monetary policy to work slowly, and it is the burden of this pape to establish this point.

The missing link in the argument is the behavior of the interest rate The impact of a monetary policy change is transmitted to investmen indirectly by inducing changes in the interest rate, and it is clear tha the dynamic response of investment spending to a money-suppl change cannot be determined without knowledge of how the interes rate responds. If it adjusts only gradually, then this means anothe lag to be added to the distributed lag in the investment function However, not only is a vigorous initial interest rate response to be ex pected, as we shall see, but, in fact, we can not rule out the possibilit of interest rate fluctuations sharp enough to shove investment spendin immediately to or beyond its new equilibrium level, without any lag at all.

What, then, is the implication for monetary policy of the investment lags so far observed, and how can we find the interest rate behavior they imply? The answers require a framework that is explicitly dynamic, and an obvious approach is to remold the static Keynesian model into a dynamic one by incorporating into it behavioral equations containing

distributed lags. The model, using linear equations for mathematical simplicity, is the following:

$$I_t^* = (1 - j)(a_1 + b_1 Y_t + dr_t) + jI_{t-1}^* \tag{1}$$

$$C_t^* = (1 - j)(a_2 + b_2 Y_t) + jC_{t-1}^* \tag{2}$$

$$Y_t = C_t^* + I_t^* \tag{3}$$

$$M_t^* = (1 - m)(e + fY_t + gr_t) + mM_{t-1}^* \tag{4}$$

$$M_t = M_t^* \tag{5}$$

where

C^* = desired consumption;
I^* = desired investment;
M^* = desired money holdings;
M = money supply;
Y = income;
r = interest rate;
t = time subscript;
j and m are lag parameters, with $0 \leq j, m < 1$; and
all other symbols are constants.

Equations (1)–(3) specify product demand and the condition that the product market is cleared in every period. Equations (4) and (5) specify the demand for money and the condition that the money market is cleared in every period.

Price and wage changes are ignored. No labor market is considered, and income can change freely without encountering a labor-supply constraint. Thus, the model deals with the impact of monetary policy on real income and employment rather than its impact on the price level.

The money supply is a policy parameter, and it is the only exogenous variable of the system. The model is assumed to have a stable static equilibrium position,[1] the properties of which are obtained by setting each variable equal to its own lagged value. Notice that the lag parameters disappear when this is done; the other constants thus specify the relationships between steady-state values of the variables. Varying j and m thereby permits us to vary the lags in the system without affecting its static properties.

This model represents a dynamic generalization of only one aspect of a Keynesian model. No dynamic process has been formulated to de-

[1] This stability assumption imposes a condition on (b_1+b_2). See equation (13) and the remarks immediately preceding it.

scribe the response of markets to excess supply and demand. Instead the static equilibrium conditions that the money and product markets be cleared have been converted into the dynamic assumptions that these markets are cleared *in every period*, and the results of the model are very dependent on these assumptions. For this reason, and for many others, the model is far too simple to be regarded as a serious representation of the real world. Rather, it is simply an illustration, an expositional device to help bring out in the conciseness of mathematics the properties of one particular economic adjustment mechanism. The general applicability of the economic principle involved will, I think, become apparent.

I. *Income Response*

A difference equation specifying directly the relationship between Y and M is obtained by combining equations (1)–(5) and eliminating C^*, I^*, M^*, and r by substitution. For our purposes, the essential characteristics of the resulting relationship are given by

$$Y_t - AY_{t-1} = B + D(M_t - mM_{t-1}), \tag{6}$$

where

$$A = \frac{j}{1 - (1 - j)\left(b_1 + b_2 - \frac{df}{g}\right)}, \tag{7}$$

$$D = \frac{Ad(1 - j)}{gj(1 - m)}, \tag{8}$$

and B is a constant of no importance.

In order to obtain an explicit solution for Y from equation (6) we must specify an initial value for Y and the complete time path of M. Yet, the answer will have no general relevance if we determine the behavior of Y only for the obvious but special case in which Y is at static equilibrium before we change the money supply and is permitted to settle down completely to a new static equilibrium after we change the money supply once. In general, we expect the money supply to change frequently as the monetary authorities react to changing economic conditions, and we would like a result that is still valid if the money supply changes again before the dynamic response to the first change has worked itself out. The question we are asking is this. What portion of the dynamic changes in Y going on continuously can be attributed to any one particular money-supply change that we choose arbitrarily? In other words, suppose we superimpose a permanent change in the money supply, occurring all at once, on top of the time path of M

(fixed or fluctuating) that would otherwise prevail. How will Y now deviate from the time path it would otherwise have followed?

Conceptually, then, we divide M and Y each into two parts. One part of M is the time path the money supply would have followed if the single monetary change whose effect we are analyzing had not taken place. The other part of M represents this change. Likewise, one part of Y is the time path income would have followed if the monetary change we are considering had not occurred, and the other part represents the deviation from this path caused by the monetary change. It is the dynamic pattern of this deviation in Y that we wish to find.

Let $Y_t = Y_t' + Y_t''$ and $M_t = M_t' + M_t''$ for all t. These are substituted into equation (6). Let Y' and M' be the unspecified underlying values to which we are adding a change. The relationship between Y' and M' clearly must be specified by an equation identical to (6) containing only single-primed variables.

This leaves

$$Y_t'' - AY_{t-1}'' = D(M_t'' - mM_{t-1}''). \tag{9}$$

Furthermore, the point of dividing our variables into two parts was in order that we might specify, without loss of generality, that

$$M_t'' = \begin{cases} 0, & t \leq 0 \\ \Delta M, & t = 1, 2, \cdots \end{cases}$$
$$Y_t'' = 0, \qquad t \leq 0. \tag{10}$$

The condition on Y'' specifies that output cannot be affected by a change in M before that change has occurred.

In analyzing the dynamic properties of this model, we are concerned more with the shape of the path along which equilibrium is approached than with the absolute magnitude of each adjustment. Therefore, it is simpler to express the values of Y'' as ratios to their final equilibrium value (Y_∞''). Define $y_t = Y_t''/Y_\infty''$, and substitute

$$Y_t'' = Y_\infty'' y_t = \frac{D(1-m)}{(1-A)} y_t \Delta M \tag{11}$$

into (9), where the expression for Y_∞'' has been obtained from (9) and (10) by setting $Y_{t-1}'' = Y_t'' = Y_\infty''$.[2] Given the conditions (10), we can now solve the resulting difference equation sequentially for y_1, y_2, y_3, etc., and it is readily seen by induction from these results that

[2] We assume $A \neq 1$. See the following paragraph.

$$(12) \qquad y_t = 1 + \frac{m - A}{1 - m} A^{t-1} \qquad \text{for} \qquad t = 1, 2, \cdots,$$

a remarkably simple solution with some very interesting properties.

The magnitude of A, defined by equation (7), clearly determines whether the model is convergent. We know that $0 \leq j < 1$, that $d, g \leq 0$, and that $b_1, b_2, f \geq 0$. Therefore, if $(b_1 + b_2)$ lies within the appropriate range, a range extending from zero somewhat beyond one, then $0 \leq A \leq 1$, and y_t converges monotonically to one. Specifically, the stability condition is

$$(13) \qquad b_1 + b_2 < 1 + \frac{df}{g} \cdot$$

The lag parameter of the demand for money, m, appears directly in the solution (12), but j, the investment and consumption lag parameter, does not appear. However, its place is taken by A. When the stability condition (13) is satisfied, then A is a monotonically increasing function of j. In addition, $j = 0$ implies $A = 0$, and $j = 1$ implies $A = 1$. Thus, we may treat A as a proxy for the investment and consumption lag parameter.

First of all, note from equation (12) that $m = A$ implies $y_t = 1$ for all t. Income adjusts instantly to its new equilibrium value when $m = A$, despite the lags in the demand functions. One lag has canceled the other.

The more general pattern becomes apparent when we examine the first-period response,

$$(14) \qquad y_1 = 1 + \frac{m - A}{1 - m} = \frac{1 - A}{1 - m} \cdot$$

For $m < A$, we have $y_1 < 1$; income only adjusts partially at first and must complete its adjustment gradually over the next several periods. As m increases or as A decreases, the first-period response becomes larger, eventually reaching 1.0 when $m = A$. If $m > A$, then income overshoots its equilibrium in the first period and must adjust back in the opposite direction. In either case, y approaches equilibrium asymptotically in succeeding periods, the speed of convergence being determined by the magnitude of A. See Figures 1 and 2 for illustrations of the response of y for different parameter values.

In all cases, investment is the volatile component of income and consumption the stable one. Consumption depends only on income, with a distributed lag, and, for this reason, it inevitably must adjust more gradually than income. This leaves investment as the component that

responds more vigorously to money-supply changes, just as we expect, and the reason is because of its dependence on the interest rate. The underlying forces will become more apparent in the next two sections.

It is clear from equation (12) that the speed with which income converges to its equilibrium is closely related to A. We expect a smaller A to mean more rapid adjustment. However, the relationship is not so simple. In the first place, $m=A$ gives instantaneous adjustment for any A. If $m \neq A$, then y only approaches 1.0 asymptotically, and

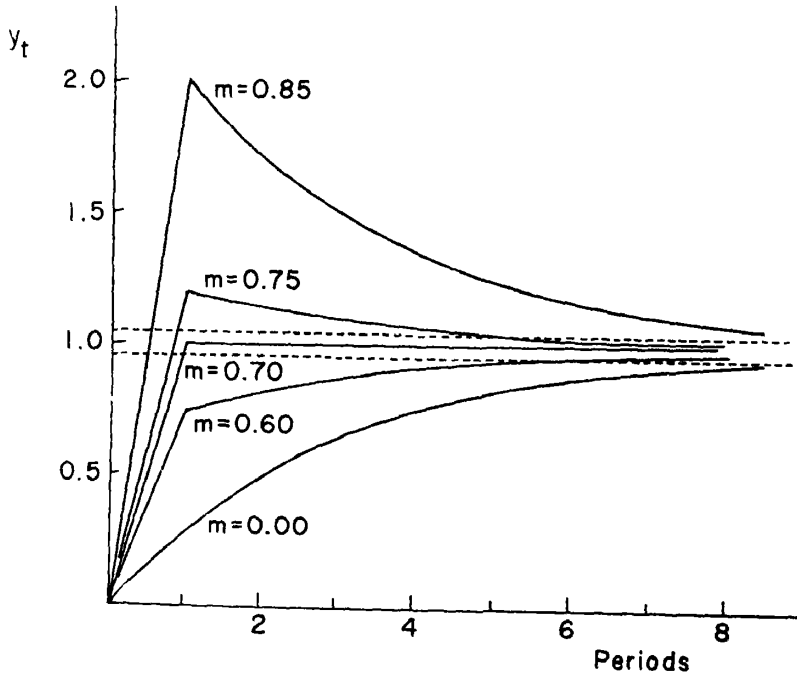


FIGURE 1. TIME PATH OF INCOME ADJUSTMENT FOR DIFFERENT MONEY-DEMAND LAGS
(Ratio of accumulated change to total equilibrium change, with $A = 0.7$)

adjustment is never entirely complete. In order to enable ourselves to talk of the speed of adjustment, we may arbitrarily say that adjustment is "complete" when the asymptotic approach of y has brought it into the range $0.95 < y < 1.05$ (or some other such narrow band around one). In Figures 1 and 2, the dotted lines mark the boundaries of this range.

In Figure 1, A has been held constant, and yet the speed of adjustment clearly varies with m. When $m < A$, adjustment becomes more rapid as m approaches A. Then it begins to take longer again as m exceeds A. The explanation is that A governs the speed of approach to equilibrium only after the first period. The farther income is from

equilibrium at the end of one period, for given A, the longer it is going to take the series of geometric terms to reach the range between the dotted lines. Yet, equation (14) shows that the distance of y_1 from equilibrium depends on the difference $(m-A)$. In a general sense, then, it is the *difference* between product-demand lags and money-demand lags, as much as it is the inherent character of the lags themselves,

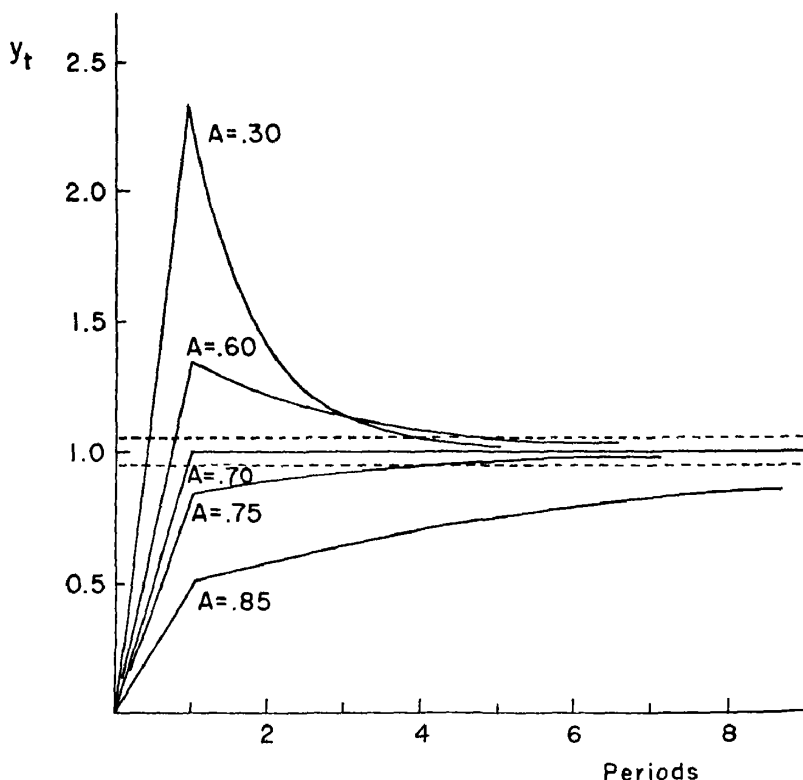


FIGURE 2. TIME PATH OF INCOME ADJUSTMENT FOR DIFFERENT PRODUCT-DEMAND LAGS
(Ratio of accumulated change to total equilibrium change, with $m=0.7$)

that determines the speed of income adjustment. If the difference is small, then adjustment is rapid.

II. *Interest Rate Response*

The preceding section presents the mathematical solution, but it offers no explanation in economic terms for this surprising result. The key lies in the behavior of the interest rate, so it is necessary first to find the solution for r in this model.

Equations (1), (2), and (3) are combined to give an equation connecting r and Y. As in the preceding section, we divide the variables

into two parts, and this produces an expression in r'' and Y'' from which the constant terms have disappeared. When this is converted into an equation in y, using equations (11) and (8), we obtain

$$r_t'' = \frac{A}{gE(1-A)}(y_t - Ey_{t-1})\Delta M \tag{15}$$

where

$$E = \frac{j}{1-(1-j)(b_1+b_2)} \tag{16}$$

and where A is given by equation (7). The solution (12) for y, together with the condition that $y_0=0$, are used in (15) to obtain directly the solution

$$r_1'' = \frac{A}{gE(1-m)}\Delta M \tag{17a}$$

$$r_t'' = \frac{A}{gE(1-A)}\left[(1-E) - \frac{(m-A)(E-A)}{(1-m)}A^{t-2}\right]\Delta M \tag{17b}$$

for $t = 2, 3, \cdots$

Comparison of equations (7) and (16) by eye shows that $E>A$ for all relevant parameter values.[3] Therefore,

$$\frac{r_\infty}{r_1} = \frac{(1-m)(1-E)}{(1-A)} < (1-m)$$

(the double prime is dropped for simplicity), and this expression may even be zero or negative because $E \gtreqless 1$ according as $(b_1+b_2) \gtreqless 1$.[4] In other words, the initial response of the interest rate is to overshoot its new equilibrium (r_∞) if $(b_1+b_2)<1$, and, if m is moderately close to one, it takes a big jump far beyond equilibrium. When $(b_1+b_2)>1$, on the other hand, it moves away from equilibrium at first. Equations (17) show that the direction of movement of r_1 is the same in either case; it is the sign of r_∞ that is different.

In the second period, the interest rate reverses direction, moving back toward (or beyond) equilibrium. We find

$$\frac{r_2}{r_1} = (1-m) - (E-A) < (1-m),$$

[3] $E=A$ if j, d, or f is zero. We may ignore these extreme cases as uninteresting.

[4] The possibility that r_∞ may be zero is the reason the solution (17) is not expressed as a ratio to r_∞ as was the solution (12) for y_t.

and this expression may also be negative. A negative value implies that r moves back farther than its initial starting position. Note that r_2 and r_∞ both bear roughly the same relation to r_1. Regardless of how wildly the interest rate jumps in the first period, its readjustment in the second period returns it to the vicinity of equilibrium.

After its second-period reaction, the interest rate approaches equilibrium asymptotically, and its rate of convergence also depends on the magnitude of A. Note that the sign of $(m-A)$ determines the sign of the second term in equation (17b); the sign of every other factor in that term is independent of the parameter values. It is this term that governs the asymptotic approach to equilibrium, and thus, just as in the case of Y, the sign of $(m-A)$ determines whether r approaches equilibrium from above or below. From the third period on, as it turns out, both Y and r are approaching equilibrium from the same side (both either rising or falling), even though their equilibrium values may lie in opposite directions from their respective starting points.

The striking thing about the interest rate is its behavior in the first two periods. Its movement is by no means simply a mirror image of the movement of income. If m is significantly greater than zero, r takes a big jump in the first period regardless of how smoothly Y adjusts. This point stands out clearly when $m=A$. In this case, Y moves directly to equilibrium in the first period, but r does not. Instead, it overshoots (or moves away from) equilibrium in the first period, returning to equilibrium only in the second period.

Figure 3 illustrates the behavior of r for an increase in the money supply when $(b_1+b_2)<1$. The adjustment is shown there as a downward adjustment because this direction of change corresponds to the upward adjustment of Y shown in the preceding two figures. In general, the behavior of r is very similar if $(b_1+b_2)>1$. The only essential difference is that the dotted equilibrium line lies above the origin; the interest rate still moves down at first and then back toward the equilibrium line.

III. *Economic Explanation*

This surprising behavior of income and the interest rate in a simple lag model has, in fact, a very simple economic explanation. The key lies in the function of the interest rate as the variable that clears the money market.

Assume for the moment that there is no distributed lag in the money-demand function; $m=0$ in equation (4). Even in this case, the interest rate must overshoot its new equilibrium position as soon as the money supply changes if there is any lag in the adjustment of Y. We have assumed that the money market is continuously cleared, and therefore aggregate money demand, M^*, must always change immediately by the same amount as any change in the money supply. Changes in Y and in

both contribute independently to the adjustment of M^*, but if Y hanges gradually, its effect on money demand is felt only gradually. nitially, it has a small impact on money demand because only a portion f the total adjustment in Y has occurred. This throws onto r a part f the adjustment burden that will later be borne by Y, and thus it vershoots at first in compensating for the lag in Y.[5]

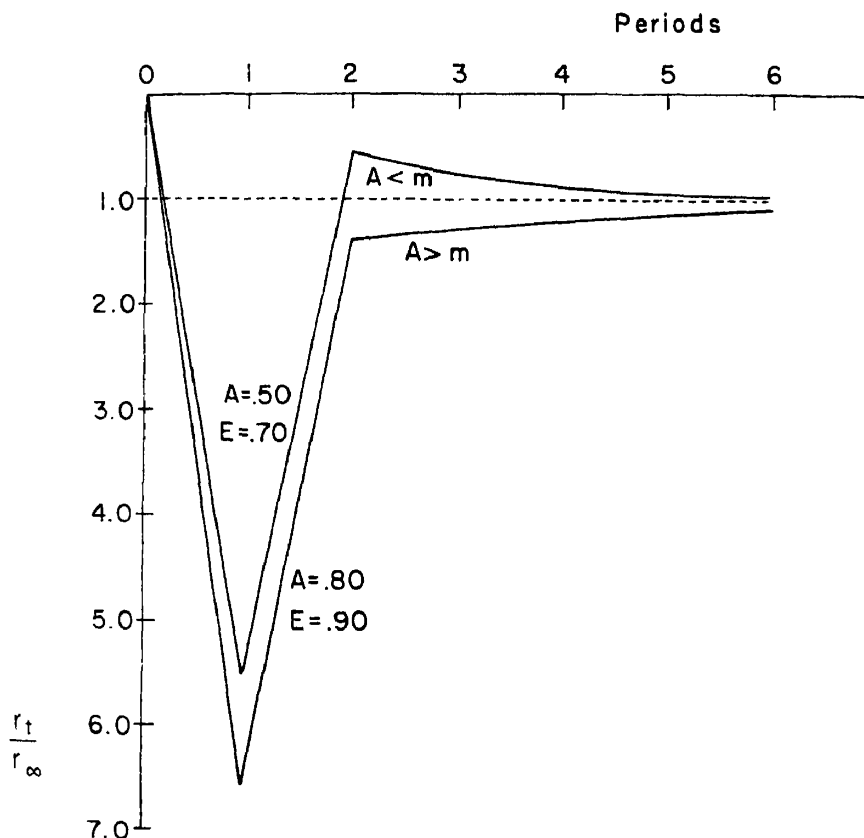


FIGURE 3. TIME PATH OF INTEREST RATE ADJUSTMENT
(Ratio of accumulated change to total equilibrium change, with $m=0.7$)

A lag in the demand for money has the independent effect of magnifying the movements of r. The coefficient of r in equation (4) is smaller, the greater is m. In other words, the *current* value of r has a smaller and smaller influence on the demand for money as the lag coefficient increases. Therefore, a given change in r this period causes less change in M^* if there are lags than if there are none. But M^* must in any case change by the amount of the change in the money supply, and there-

[5] When $(b_1+b_2)>1$, r moves away from equilibrium at first rather than overshooting. This is identical both in cause and in consequences to the overshooting that occurs when $(b_1+b_2)<1$.

fore if there are lags, r must change by a greater amount initially in order to clear the money market. Similarly, lags reduce the coefficient of Y, and this reduces the effect a given change in current Y has in adjusting M^* and throws a greater adjustment burden initially on r, thereby increasing still more the amount it must change.

Changes in the interest rate influence income through their impact on investment. Of course, vigorous overshooting of the interest rate need not necessarily imply any corresponding immediate response by investment and income, in general. However, a crucial property of the lag formulation of investment demand being studied here lies in the fact that the lag is distributed, not discrete. Investment may depend relatively more heavily on past values of r, but at the same time it depends, to some small degree, on the current value of r. This small linkage gives r the direct tie it needs.

In this model, the initial response of I^* to any change in r is strictly proportional to the change in r. Thus, if r overshoots sufficiently far beyond its new equilibrium, it can cause I^* to overshoot immediately as well, operating solely through this small tie between I^* and current r. A sufficiently vigorous response by I^* will, in turn, carry Y to or beyond its equilibrium.

IV. *Empirical Relevance*

Despite the model's obvious inadequacies, simple curiosity demands that we plug into it some representative parameter values from empirical studies of U. S. data just to see what comes out. The theoretical validity of these results is independent of any data, but their empirical significance depends on whether there is a genuine possibility that we may have to revise our conclusions about the speed of monetary policy in the real world. This, in turn, hinges on whether there may exist a substantial distributed lag in the demand for money. If m is close to zero, then previous conclusions about the speed of monetary policy, although shown to be based on an incorrect argument, will be vindicated in fact, and this analysis will have little empirical significance.

The elementary textbook Keynesian view is that a speculative demand for money stocks provides much of the interest responsiveness of the demand for money, and this by its very nature implies a very rapid adjustment to changes in interest rates. This explanation must be rejected as entirely inadequate. The speculator who operates regularly with significant quantities of money will find it unprofitable to hold money for more than a very short period of time. The rate of return on short-term debt instruments will pay the transaction costs of buying and selling them even if they are only held a few days, and the risk of capital loss on them is essentially zero. Thus, very little money is held for genuinely speculative purposes at any point of time in the real world.

The more modern theories of liquidity preference present a rather different and more convincing picture. One's cash balance is an inventory, and each economic unit has a decision rule (very often only vague and implicit) specifying the inventory of cash to be held for future expenditures, both foreseen and unforeseen. The storage cost of money is the opportunity cost from interest income foregone, and the gain from holding money is savings in transaction costs and lower risk of capital loss. The optimal inventory of cash is a decreasing function of the interest rate (the storage cost). The question of speed of adjustment of the demand for money is the question of how frequently economic units consult their decision rules on cash inventory and how quickly they can modify institutional arrangements that determine to some extent the minimum quantity of cash required. There is nothing in this theory to suggest instantaneous or even rapid response to interest rate changes that, from the point of view of potential lenders, may be entirely transitory.

A substantial distributed lag in the demand for money thus appears reasonable in principle, and, in addition, there is empirical evidence of its existence. In his recent estimates of money-demand functions based on quarterly data for the United States, Ronald Teigen [8] finds that aggregate demand for money only completes about 31 per cent of its adjustment to equilibrium in one quarter.[6] In a more recent paper, de Leeuw [4] finds even greater lags in money demand. Without attempting to evaluate their reliability, I have used Teigen's results and the investment function estimates of Kareken and Solow in [3] to derive the following parameter values for my model:[7,8]

$$d = -10.1 \qquad j = 0.550$$
$$f = 0.178 \qquad m = 0.686$$
$$g = -3.39$$

Their data are quarterly, and thus I take one quarter as the length of each time period in my model. Note that investment demand responds

[6] Teigen uses a lag function for the demand for money that is similar to equation (4) in this paper, and he finds a coefficient of 0.686 for M_{t-1} [8, p. 489, equation 3.1a]. This implies that adjustment is 31.4 per cent complete after one quarter.

[7] The static equilibrium equation implied by Teigen's model [8, p. 491] has the property that $dM/dY = .1958 - .0080\, r$, where r is the rate on 4–6 month prime commercial paper. The 1948–58 average of r is 2.25, whence it follows that $f = 0.178$. In the same equation, $dM/dr = -.0080Y$. The 1948–58 average of GNP in billions of 1963 dollars is $Y = 424$, and making this substitution yields $g = -3.39$.

[8] Kareken and Solow report [3, p. 38] an interest elasticity of equilibrium investment demand of approximately 0.5. Therefore, we have $dI/dr = -0.5I/r$. The 1948–58 average of gross private domestic investment in billions of 1963 dollars is $I = 64.6$. Kareken and Solow used Moody's industrial bond yields for their interest variable, and the 1948–58 average of this variable is 3.21. Accordingly, $d = -10.1$. Their estimate of the coefficient of I_{t-1} is approximately 0.55 [3, p. 37].

rather slowly, also, although not so slowly as the demand for money. Rather than find estimates for b_1 and b_2, I have tried several possible values for their sum. The parameter values are substituted into equations (7) and (12), and the results are shown in Table 1.

In spite of the long lags in the investment and money-demand functions, it turns out that aggregate income adjusts to within 10 per cent of equilibrium within one quarter if (b_1+b_2) lies between 0.89 and 1.05, and for any smaller value it overshoots equilibrium by more than 10 per cent. The range between 0.89 and 1.05, furthermore, is about where we would expect to find (b_1+b_2). Any value greater than 1.0 implies that the equilibrium interest rate is raised by expanding the money

TABLE 1—DYNAMIC ADJUSTMENT OF INCOME TO MONEY-SUPPLY CHANGES IN U.S.

Accumulated change in Y expressed as a ratio to equilibrium change in Y

Value of (b_1+b_2)	.70	.80	.90	.95	1.00	1.10	1.20
Implied value of A	.596	.626	.660	.678	.698	.740	.787
Accumulated adjustment after:							
1 quarter	1.288	1.190	1.084	1.025	.963	.829	.677
2 quarters	1.172	1.119	1.056	1.017	.975	.874	.746
3 quarters	1.102	1.075	1.037	1.012	.982	.907	.800
4 quarters	1.061	1.047	1.024	1.008	.987	.931	.843
5 quarters	1.036	1.029	1.016	1.005	.991	.949	.876
6 quarters	1.022	1.018	1.011	1.004	.994	.962	.902

supply and is lowered when M is reduced (the case of an upward-sloping IS curve in the Hicksian LM-IS diagram), and this is not particularly plausible. Yet, we must assign to b_2, the marginal propensity to consume, a minimum value of at least 0.70, and only a moderate value for b_1 brings their sum close to 0.89.

The results are sensitive to the values of the lag coefficients j and m, and the standard errors of the estimates I have used are not small. Increasing j and decreasing m respectively by only one standard error, for example, leads to a model in which income does not overshoot for any of the values of (b_1+b_2) shown in Table 1, and with $(b_1+b_2)=1.0$, it takes seven quarters for income to approach within 10 per cent of equilibrium.

Thus, this little numerical exercise proves nothing, but it should certainly whet some appetites. The lag coefficients appear to be of the right order of magnitude, at least, to suggest the possibility that the money-demand lag may offset completely the influence of distributed lags in investment and consumption demand. Can more sophisticated methods confirm this? If so, the theoretical explanation for the apparent

slowness of monetary policy in the real world will have to be found elsewhere from now on, perhaps in such things as production lags or gradual revisions of expectations in response to monetary changes. Or perhaps monetary policy works more quickly than we thought.

V. *Summary and Conclusion*

The speed with which the economy responds when the monetary authority changes the money supply clearly depends directly on the speed with which investment responds to changes in the interest rate and other variables. The relation between them is not simple, however. In a linear model in which the behavioral equations have distributed lags and in which the money and product markets are cleared in every period, the interest rate, in performing its function of clearing the money market, will overshoot its new equilibrium as soon as the money supply changes. Furthermore, the longer the distributed lag in the demand for money, the greater the amount by which the interest rate will overshoot. If it overshoots far enough, it will cause both investment and income to overshoot, as well. The speed with which income adjusts to equilibrium depends partly on the length of the investment lag, but it also depends, in a general sense, on the difference between product-demand and money-demand lags. If there is little lag in the demand for money, then a long investment lag implies slow adjustment. No matter how long the product demand lags, however, adjustment is very rapid if the money-demand lag is comparably long.

The model is an unrealistic one, and this analysis clearly, then, does not show that monetary policy works quickly. Its significance, instead, is in pointing out the fallacy in a common argument purporting to show that monetary policy works slowly. Slow lagged response of investment to interest rate and income changes is not a sufficient condition for monetary policy to work slowly.

Yet, although the model as a whole is specific and unrealistic, the underlying economic mechanism that accounts for the results is completely general. A distributed lag in the demand for money will amplify the dynamic response of both the interest rate and income in almost any model in which the relations between variables are smooth and in which investment depends on the interest rate. The function of the interest rate as the variable that adjusts to reduce excess demand or supply in the lending market causes this behavior regardless of the shapes of the functions and regardless of whether markets are cleared in every period. This, basically, is why the results of the specific model in this paper may be accepted as an insight into the workings of monetary policy in the real world.

"The" interest rate may, of course, be interpreted broadly as an index

or representative of the many different interest rates and other terms of lending contracts. In the real world, the components of this index may adjust at different rates, and, in particular, some could adjust very slowly without preventing the index as a whole from behaving as does "the" interest rate in the model. For example, Kareken's and Solow's finding [3, p. 5] that interest rates on bank loans to business adjust very slowly to equilibrium, a finding they use as additional evidence that investment must respond slowly to monetary policy changes, is logically consistent with rapid investment and income response. Major lags in the demand for money, such as found by Teigen [8], could cause market yields on government and corporate bonds and the nonmonetary terms of bank lending contracts to change rapidly, leaving quoted bank lending rates and time deposit dividend rates to follow behind months later. The rapid changes in bond yields and nonmonetary lending terms might be enough, by themselves, to cause rapid adjustment of investment spending and income.

These results provide an interesting commentary on the subject of indicator and target variables for monetary policy. Even when all behavioral parameters and exogenous variables other than the money supply itself remain fixed, the money supply and the interest rate do not move together in a simple way. Large interest rate fluctuations may be associated with monotonic money-supply adjustment, and they need not be taken as evidence of excessively abrupt or destabilizing monetary policy action. Also, a policy of linear adjustment of the interest rate to a new level is distinctly different from a policy of linear adjustment of the money supply to a new level.

Two additional lessons may be drawn from this study. In a general way, it shows that having the current value of some variable y depend on a weighted average of the values assumed by another variable x in several periods—rather than on the value of x in only one period, current or past—does not necessarily smooth out or damp the dynamic behavior of the system. The interaction of distributed-lag behavioral relations in a simultaneous system cannot be assumed to lead in a simple way to gradual lagged adjustment. The intuition that expects gradual adjustment is a poor guide.

Finally, this work should demonstrate that analysis and estimation of dynamic investment response cannot be granted any position of unique importance in empirical work on the speed of monetary policy. The portfolio behavior of holders of liquid assets plays a role equally as influential, and major research efforts should be concentrated in the future on understanding the monetary side of individual and firm behavior.

REFERENCES

1. H. G. JOHNSON, *The Canadian Quandary*. Toronto 1963.
2. ——— AND J. W. L. WINDER, *Lags in the Effects of Monetary Policy in Canada*, staff study for the Royal Commission on Banking and Finance. Toronto 1962.
3. J. KAREKEN AND R. M. SOLOW, "Lags in Monetary Policy," in Commission on Money and Credit, *Stabilization Policies*. Englewood Cliffs, N. J. 1963.
4. F. DE LEEUW, "The Demand for Money—Speed of Adjustment, Interest Rates, and Wealth," Staff Economic Studies Series, Board of Governors of the Federal Reserve System, 1965.
5. T. C. LIU, "An Exploratory Quarterly Econometric Model of Effective Demand in the Postwar U. S. Economy," *Econometrica*, July 1963, *31*, 301–48.
6. T. MAYER, "The Inflexibility of Monetary Policy," *Rev. Econ. Stat.*, Nov. 1958, *40*, 358–74.
7. J. R. MEYER AND R. R. GLAUBER, *Investment Decisions, Economic Forecasting, and Public Policy*. Boston 1964.
8. R. L. TEIGEN, "Demand and Supply Functions for Money in the United States: Some Structural Estimates," *Econometrica*, Oct. 1964, *32*, 476–509.

THE EFFECTIVENESS OF AUTOMATIC STABILIZERS

By PETER EILBOTT*

Automatic stabilizers are fiscal or monetary mechanisms which automatically reduce the flow of income or money to individuals and corporations during periods of expansion and which increase such flows (relative to what they would have been in the mechanisms' absence) in times of recession. The corporate and personal income tax and the unemployment compensation program are examples of automatic stabilizers. Tax receipts rise and fall automatically as incomes fluctuate, and unemployment compensation benefit payments increase automatically in response to rising unemployment. (The fact that individuals must apply for benefit payments does not change their basic automaticity.)

The stabilizers represent an important countercyclical force, since they avoid the difficulties which beset discretionary policy—difficulties arising out of problems of timing and errors of judgment. There have been numerous attempts to measure their quantitative impact in the postwar period as a guide to determining their role in countercyclical policy. With few exceptions, however, these investigations determined only the response of changes in taxes and transfer payments to given increases or decreases in national income.

Thus Hart [7], Clement [1], Lewis [9], and Duesenberry, Eckstein, and Fromm [5] all evaluated the built-in flexibility of the automatic stabilizers (that is, the response of the various stabilizing items to changes in national income), the first three by examining historical data, the last by performing a simulation study. Similarly, Lester [8], Pechman [15], and others cited in the bibliography [2] [3] [16] investigated the response of individual stabilizers to changes in national income. Finally, Creamer [4] computed the value of recession-generated unemployment benefit payments and recession-induced personal income tax reductions, and Ida Merriam [11] computed the value of recession-generated social insurance payments—that is, the amounts paid out over and above the amounts which would have been paid in the absence of the recession.

* The views expressed in this article are those of the author, and do not necessarily reflect the views of the New York Stock Exchange, where he is employed as a research economist. He wishes to acknowledge the very helpful comments of Professor James W. Angell, both on the draft of this article and on the dissertation [6] from which the article is drawn.

A change in income will lead to a change in taxes, and it is clearly useful to know how income changes affect both the sums paid out in benefit payments and the taxes collected from individuals and corporations. It seems even more important, though, to determine how tax and transfer payment changes affect private spending and therefore movements in national income—in other words, to determine the percentage of potential income changes prevented by the stabilizers during recessions and expansions.

While a few studies have examined the effectiveness of the stabilizers from this point of view, these studies were based on limited historical data. Thus, Musgrave and Miller [12] employed a rather simplified multiplier model to investigate the effectiveness of the stabilizers in 1947-48 (Musgrave [13] later employed the same model to measure the stabilizers' impact in the mid-1950's). Lusher [10] employed a different model to examine data from the 1930's and to evaluate the stabilizers' effectiveness in a hypothetical recession occurring in the mid-1950's. Finally, Pearse [14] developed a model somewhat similar to ours to measure the impact of the British tax stabilizers.[1]

In this article we evaluate the effectiveness of the stabilizers in reducing income changes during the postwar period. Specifically, we will estimate, on the basis of a multiplier model, by what percentage the stabilizers reduced the potential change in income in each of the three recessions and three expansions between 1948 and 1960.[2] We are primarily concerned with the stabilizers' countercyclical role and not with their effect on economic growth. Consequently, a tax which restricts a rise in income is designated a stabilizer, though it may be inhibiting from the point of view of growth. However, as pointed out in Section II. A, while the model depicts the stabilizing (or restrictive) effect of taxes and transfer payments during the postwar expansions, in practice rising government expenditures largely offset this effect. As a result, growth was generally not inhibited by the stabilizers.

I. *The Model and Its Application*

Our model is basically an elaboration of the one first developed by Musgrave and Miller (and repeated by Musgrave in *The Theory of Public Finance* [13 pp., 509-11]). In their formulation

[1] Pearse's article appeared in 1962 and was apparently written at approximately the same time as the dissertation on which this article is based.

[2] While it was also our intention to evaluate the impact of the stabilizers in the 1960-61 recession, the decline in national income was too limited to permit meaningful results. Thus, Lewis found that taxes decreased by 97 per cent of the decline in GNP occurring during the contraction, while unemployment benefit payments increased by 25 per cent of that decline [9, p. 30]. The stabilizers therefore had a combined "swing" of 122 per cent of the reduction in GNP, and this result must be considered atypical.

$$\alpha = \frac{cEr}{1 - c + cEr}$$

where E is the combined income elasticity of personal and corporate taxes, r the ratio of these taxes to national income, c the marginal propensity to consume out of disposable income plus retained corporate earnings, and α the percentage of a potential change in income prevented by the stabilizers.

By introducing a new term allowing for a change in consumption in response to a change in transfer payments and, more important, by separating the corporate from the personal sector, we have developed the following model (whose derivation is available on request):

$$\alpha = \frac{c(E_r r_1 - E_\mu \mu_1) + i(E_t t_1)}{1 - c\frac{\Delta X}{\Delta Y} - i\frac{\Delta Z}{\Delta Y} + c(E_r r_1 - E_\mu \mu_1) + i(E_t t_1)} .$$

E_r, E_μ, and E_t are, respectively, the income elasticities of personal taxes, transfer payments, and corporate taxes; r_1, μ_1, and t_1 are the ratios of personal taxes, transfer payments, and corporate taxes to national income at the beginning of the period under analysis; c is the marginal propensity to consume out of disposable income; i is the marginal propensity to invest out of retained corporate earnings; $\Delta X/\Delta Y$ and $\Delta Z/\Delta Y$ are the personal and corporate shares of a change in national income; and α is the percentage of a potential change in income prevented by the stabilizers. In contrast to Musgrave and Miller's model, our formulation relates consumption directly to personal taxes and the noncorporate part of national income, while investment is related only to corporate taxes and the corporate share of national income.

The combined impact of the following items, which are commonly designated automatic stabilizers, is tested by our model: individual income taxes, corporate taxes, excise taxes, unemployment compensation benefit payments and contributions, and Old Age and Survivors Insurance benefit payments and contributions.[8] Because of the difficulty of adjusting some of the data for changes in tax rates and lags in collections, certain components of these taxes and transfer payments

[8] Results which exclude the impact of the OASDI program are also presented, since its characterization as an automatic stabilizer may de disputed. In addition, benefit payments and contributions could not be adjusted for the extensive increases in coverage occurring in the postwar period. As a result, they both show a fairly continuous growth, even when adjusted for the effects of rate and benefit payment changes and for increases in the taxable wage base.

are excluded from the analysis. As a result, only the following data are employed:

Individual taxes. Personal income tax withholding collections, federal excise taxes (considered personal income taxes assessed on an expenditure basis), and personal OASDI contributions (excluding payments by the self-employed).

Corporate taxes. Corporate income tax accruals (excluding excess profits tax accruals in 1950-53), unemployment contributions to state unemployment trust funds, and corporate OASDI contributions.

Transfer payments. Unemployment benefit payments (excluding payments under temporary programs) and OASDI benefit payments.

Our basic data consist of the quarterly values of these taxes and transfer payments during the years 1948 through 1960, covering three complete recessions and three complete expansions. We determine the effectiveness of the stabilizers during each recession and each expansion, and also measure their average effectiveness. In addition, we evaluate the impact of the tax stabilizers both on the basis of historical data and, where rates changed over time, on the basis of data adjusted to 1965 tax rates.

Before the available tax and transfer payment data can be employed in our model, several adjustments are necessary (a detailed explanation of these adjustments is available on request, as are the final adjusted data).

1. *Adjustment for a collections lag.* The individual bears the impact of a tax payment when he makes that payment. Most published data, however (except for accrual data), reflect receipt of the funds by the government. This may occur months after the taxes are withheld by an employer or collected by a business establishment. If the impact of the stabilizers is to be attributed to the proper time period, an adjustment must be made for this delay.

2. *Adjustment for the effects of statutory rate and benefit payment changes.* The analysis involves comparisons of tax collections and transfer payments at the beginning and end of specific recessions and expansions. If no adjustment is made for a tax rate increase occurring during an expansion, the rise in national income would appear to have caused a sizable increase in tax collections, and the effectiveness of the stabilizers would be overstated. The impact of the OASDI program would also be overstated if no allowance were made for a statutory increase in benefit payments occurring during a recession. The data must therefore be adjusted for the effects of rate and benefit payment changes.

3. *Adjustment of the tax series to 1965 tax rates.* The various tax series are also adjusted to 1965 rates in an attempt to appraise the quantitative importance of the present set of automatic stabilizers. For example, since corporate tax rates have been increased considerably since 1948, estimates of the stabilizers' current effectiveness cannot be based on the response of corporate taxes to the decline in national income during the 1948-49 recession. We can, however, estimate how corporate taxes would have changed in 1948-49 had present rates then been in effect, and use these data in the model.

Unemployment and OASDI benefit payments and contributions are not brought up to recent levels, however. Benefit payments are expressed in absolute amounts, and not in percentages, as are tax rates. These payments have risen steadily in response to the inflation and the rise in real wages which have characterized the postwar period, and they now bear no reasonable relation to payments made in previous years. It therefore seems meaningless to estimate how effective benefit payments would have been ten or fifteen years ago had current payment levels then been in effect. Since changes in unemployment and OASDI contribution rates have reflected, in part, the rise in benefit levels, contributions are also not adjusted to present rates.

4. *Adjustment for the effects of seasonal influences.* The data are seasonally adjusted in order to establish the relationship between changes in national income and changes in taxes and transfer payments in a form independent of a particular time of year. The seasonal factors are based on data from years not marked by rate changes, and on data adjusted for these changes where tax rates were revised frequently.

The data are not adjusted for changes in the price level. An increase in tax revenues, assuming a given tax structure, is the same whether the increase in national income which gave rise to it reflects inflation or is a result of real factors (ignoring the possibility of differential effects on the distribution of income under the two sets of circumstances).

By applying the model to the adjusted data we obtain our estimates of the effectiveness of the stabilizers in restricting changes in income. As a first step, we derive income elasticities by computing the percentage changes in individual taxes, corporate taxes, and transfer payments between the initial and terminal quarters of each recession and each expansion, and by determining the ratios of these percentages to the percentage changes in national income occurring within the same periods.

The resulting elasticities are arc elasticities, which apply only to a

period as a whole. Consequently, if the calculations indicate that the stabilizers prevented 40 per cent of a potential rise in income during an upswing, the percentage measures the stabilizers' effectiveness only over the complete expansion. It may also, however, be considered a measure of the average effectiveness of the stabilizers during the upswing.

As a second step, we calculate the ratios of tax collections and transfer payments to national income in the initial quarters of each recession and each expansion. The use of these quarters follows from the derivation of the model, in which the stabilizing impact of a tax is a function of its arc elasticity from the initial to the terminal quarter of a particular period, and of its ratio to national income in that initial quarter.

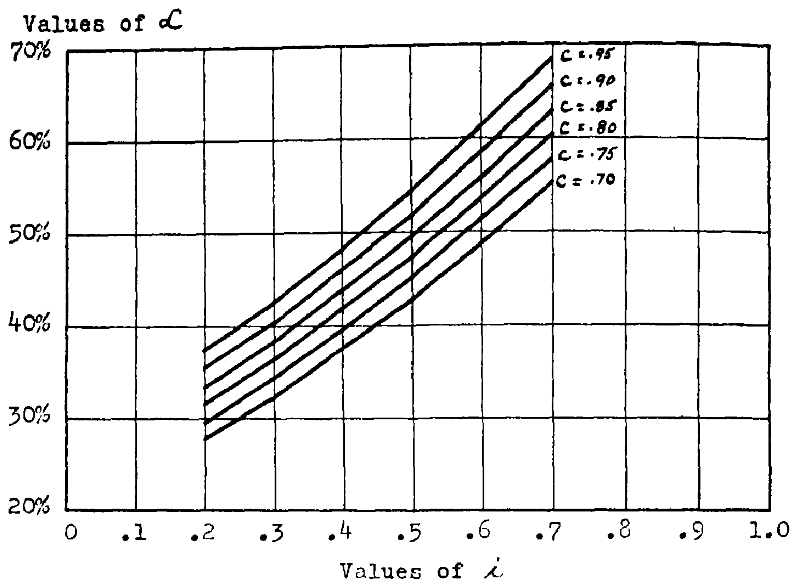
Third, the ratios $\Delta X/\Delta Y$ and $\Delta Z/\Delta Y$ are derived by relating the change in pretax retained corporate profits (net of inventory profits and losses) and in corporate social insurance contributions during each upswing or downswing to the change in national income occurring within the same period. This calculation yields $\Delta Z/\Delta Y$, and $\Delta X/\Delta Y$ is derived simply by subtracting $\Delta Z/\Delta Y$ from 1.

Finally, the values of the various income elasticities, the ratios of taxes and transfer payments to national income, and the ratios $\Delta X/\Delta Y$ and $\Delta Z/\Delta Y$ are employed with several combinations of assumed values c and i, in the manner prescribed by the model, to derive the estimates of α. The resulting values represent the percentages by which the stabilizers reduced the potential change in income during each recession and expansion.

Since estimates of the values of c and i in the postwar period vary, several combinations of values of these variables are employed in the calculations. These values (0.8, 0.85, and 0.9 for c; and 0.3, 0.4, and 0.5 for i) span the range of existing estimates, and their use in different combinations permits a determination of the stabilizers' effectiveness under varying assumptions. Estimates of α resulting from the use of other values of c and i are presented in Chart 1.

The use of fixed values of c and i does not imply that these variables were stable during the postwar business cycles. Since the results obtained by using the model represent the *average* effectiveness of the stabilizers during a given period, the values assigned to c and i may be considered averages of the values these variables assume during that period.

Table 1 illustrates the calculations involved in deriving the values of α (using data from the 1957-58 recession). Table 2 presents these values for each contraction and each expansion, on the basis of data adjusted to tax rates existing at the beginning of each period and



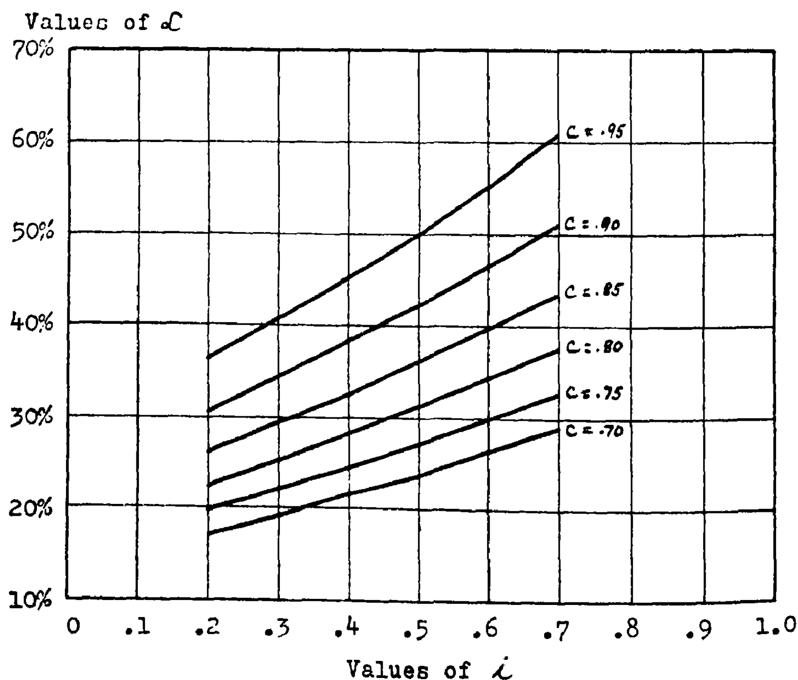


CHART 1—ESTIMATES OF α BASED ON THE AVERAGE VALUES OF THE ELASTICITIES AND RATIOS OF TAXES AND TRANSFER PAYMENTS TO NATIONAL INCOME, 1948-60

TABLE 1—DERIVATION OF THE VALUES EMPLOYED IN OBTAINING ESTIMATES OF α FOR THE 1957–58 RECESSION

	(All Values in Billions of Dollars, at Annual Rates)			
	Data Adjusted to Tax Rates and Benefit Levels Existing at the Beginning of the Recession		Data Adjusted to 1965 Tax Rates and to Benefit Levels Existing at the Beginning of the Recession	
	III 1957	I 1958	III 1957	I 1958
National Income	$371.6	$357.4	$371.6	$357.4
$\Delta X/\Delta Y$	4.4/14.2		4.4/14.2	
$\Delta Z/\Delta Y$	9.8/14.2		9.8/14.2	
Withholdings Collections	27.70	26.50	21.54	20.61
Excise Taxes	11.06	10.73	10.98	10.65
OASDI Contributions	3.81	4.16	3.81	4.16
Total Personal Taxes	$ 42.57	$ 41.39	$ 36.33	$ 35.42
Unemployment Benefits	1.68	3.37	1.68	3.37
OASDI Benefit Payments	8.09	8.70	8.09	8.70
Total Transfer Payments	$ 9.77	$ 12.07	$ 9.77	$ 12.07
Corporate Income Taxes	21.20	16.30	19.60	15.00
Unemployment Contributions	1.42	1.58	1.42	1.58
OASDI Contributions	3.81	4.16	3.81	4.16
Total Corporate Taxes	$ 26.43	$ 22.04	$ 24.83	$ 20.74
E_r	.73		.66	
E_μ	−6.16		−6.16	
E_t	4.35		4.30	
r_1	.115		.098	
μ_1	.026		.026	
t_1	.071		.067	

adjusted to 1965 tax rates. In addition, the table presents estimates of α derived from the average values, over the three recessions and three expansions, of the various income elasticities and ratios of taxes and transfer payments to national income. These estimates, based only on data adjusted to 1965 tax rates, may be assumed to represent the best appraisals of the present effectiveness of the stabilizers. They are portrayed in Chart 1, though on the basis of a considerably wider range of values of c and i than are employed in Table 2.

Table 2 also presents results which exclude the impact of both OASDI contributions and benefit payments. Only selected combinations of c and i are employed in the calculations, which are based on data adjusted to 1965 tax rates.

TABLE 2—EFFECTIVENESS OF THE AUTOMATIC STABILIZERS

(Values of α)

A. Recessions		IV 1948–IV 1949	II 1953–II 1954	III 1957–I 1958	Average of Three Recessions
1. Data Adjusted to Tax Rates and Benefit Levels Existing at the Beginnings of the Recessions					
$c=0.9$	$i=0.5$	40.3%	57.7%	49.9%	
	$i=0.4$	35.1	51.4	43.5	
	$i=0.3$	30.5	45.5	37.8	
$c=0.85$	$i=0.5$	37.1	55.7	48.0	
	$i=0.4$	32.2	49.6	41.8	
	$i=0.3$	28.0	43.7	36.2	
$c=0.8$	$i=0.5$	34.2	53.8	46.2	
	$i=0.4$	29.7	47.7	40.1	
	$i=0.3$	25.8	41.9	34.6	
2. Data Adjusted to 1965 Tax Rates and to Benefit Levels Existing at the Beginnings of the Recessions					
$c=0.9$	$i=0.5$	42.4%	56.3%	47.9%	51.8%
	$i=0.4$	36.8	50.1	41.6	45.8
	$i=0.3$	31.8	44.4	36.0	40.3
$c=0.85$	$i=0.5$	39.2	54.4	46.1	49.5
	$i=0.4$	34.0	48.3	39.9	43.7
	$i=0.3$	29.3	42.6	34.4	38.3
$c=0.8$	$i=0.5$	36.3	52.5	44.3	47.2
	$i=0.4$	31.5	46.4	38.3	41.5
	$i=0.3$	27.1	40.7	32.8	36.3
3. Data from Section 2 Excluding the Impact of the OASDI Program					
$c=0.9$	$i=0.5$	39.5%	51.8%	48.6%	48.6%
$c=0.85$	$i=0.4$	31.2	43.5	40.5	40.2
$c=0.8$	$i=0.3$	24.4	35.9	33.2	32.8

II. *Interpretation of the Results*

A. *The Impact of the Stabilizers*

Table 2 indicates that, during the three recessions between 1948 and 1960, the stabilizers would have prevented an average of from 36 to 52 per cent of the income declines which would have occurred in their absence had 1965 tax rates been in force. According to the tax changes which actually occurred, they were slightly more effective, since tax rates were higher than they are now in two of those recessions. To discuss the average impact of the stabilizers in terms of historical tax rates is meaningless, though, since rate revisions were partly responsible for the changes in their effectiveness between the various recessions.

TABLE 2 (*Continued*)

B. Expansions

	IV 1949– II 1953	II 1954– III 1957	I 1958– II 1960	Average of Three Expansions
1. Data Adjusted to Tax Rates and Benefit Levels Existing at the Beginnings of the Expansions				
$c=0.9$ $i=0.5$	44.4%	44.3%	44.6%	
$c=0.9$ $i=0.4$	41.2	41.0	39.8	
$c=0.9$ $i=0.3$	38.1	37.8	35.2	
$c=0.85$ $i=0.5$	37.6	37.1	39.7	
$c=0.85$ $i=0.4$	34.9	34.3	35.3	
$c=0.85$ $i=0.3$	32.3	31.6	31.3	
$c=0.8$ $i=0.5$	32.2	31.5	35.4	
$c=0.8$ $i=0.4$	29.9	29.1	31.5	
$c=0.8$ $i=0.3$	27.7	26.7	27.8	
2. Data Adjusted to 1965 Tax Rates and to Benefit Levels Existing at the Beginnings of the Expansions				
$c=0.9$ $i=0.5$	44.8%	39.2%	40.6%	42.2%
$c=0.9$ $i=0.4$	41.3	35.8	35.7	38.1
$c=0.9$ $i=0.3$	38.0	32.4	31.1	34.2
$c=0.85$ $i=0.5$	38.0	32.4	35.8	36.0
$c=0.85$ $i=0.4$	35.1	29.5	31.5	32.6
$c=0.85$ $i=0.3$	32.3	26.8	27.4	29.3
$c=0.8$ $i=0.5$	32.6	27.3	31.8	31.2
$c=0.8$ $i=0.4$	30.1	24.8	27.9	28.2
$c=0.8$ $i=0.3$	27.7	22.5	24.3	25.3
3. Data from Section 2 Excluding the Impact of the OASDI Program				
$c=0.9$ $i=0.5$	44.9%	47.3%	44.8%	45.9%
$c=0.85$ $i=0.4$	35.2	37.3	35.6	36.3
$c=0.8$ $i=0.3$	27.9	29.6	28.3	28.8

Table 2 also shows that the impact of the stabilizers varied considerably from recession to recession. They were most effective during the 1953-54 downswing, when they reduced the potential decline by from 42 to 58 per cent, and would have reduced it by from 41 to 56 per cent had 1965 tax rates been in force.

The discussion is framed in terms of ranges of effectiveness, since we do not attempt to determine the actual values of c and i during the period covered by the analysis. Nevertheless, we assume that these values did in fact fall between the limits employed in the calculations.[4]

[4] As noted previously, Chart 1 portrays estimates of the stabilizers' effectiveness based on a considerably wider range of values of c and i than are employed in Table 2.

The findings indicate that, even if one assumes fairly low values for the two marginal propensities, the stabilizers prevented an average of over one-third of the potential income declines during the first three postwar recessions. On the other hand, if the average values of c and i were at the upper limits employed in the calculations, those declines would have been twice as severe in the absence of the stabilizers.

The stabilizers were somewhat less effective during the three expansions between 1948 and 1960 than they appear to have been during recessions (measuring effectiveness in terms of a dampening of a rise in income). Table 2 indicates that they would have reduced the potential increases in income by an average of from 25 to 42 per cent had 1965 tax rates been in force, while their impact on the basis of actual tax changes was slightly greater. The stabilizing percentages show relatively little variation from expansion to expansion.

It must be pointed out that the values of α for expansions do not necessarily indicate the actual effectiveness of the stabilizers during the particular periods. The model assumes that the stabilizers have no impact on government spending on goods and services; this assumption is implicit in the derivation of α. Consequently, if government expenditures increased in response to (or along with) the stabilizer-induced rise in revenues (as they generally did during the postwar expansions), the data would only depict how effective the stabilizers might have been had government spending on goods and services remained unchanged, and not their actual effectiveness during the particular upswings.

The data indicate that the stabilizers have an impact in recessions which differs only moderately from their impact in expansions. Previous studies, which measured the response of taxes and transfer payments to changes in national income, showed that the stabilizers were substantially more effective during recessions. The conflict may be resolved in the following terms.

How effective the stabilizers are depends on their impact on the economy's over-all marginal propensity to save out of national income (considered as some kind of weighted average of the individual, corporate, and government sectors' marginal propensities to save). That is, because of the stabilizers, the government's budget (including trust fund operations) is pushed toward a surplus during periods of rising national income. As a result, the government becomes a net saver, thereby raising the economy's marginal propensity to save out of national income. Similarly, the budget is pushed toward a deficit during recessions, and the over-all marginal propensity to dissave is increased.[6]

[6] For the rest of this article, the terms marginal propensity to save and to spend will be aplied both to expansions and to contractions, though it would be more appropriate to refer to disspending and dissaving during periods of income decline.

Since the value of the system's multiplier is a reciprocal of the marginal propensity to save (hereafter referred to as the m.p.s.), the stabilizers, through their impact on the m.p.s., dampen the multiplier.

The reduction in the value of the multiplier depends not only on the absolute increase effected by the stabilizers in the m.p.s., but also on that propensity's initial value. For example, the stabilizers reduce the value of the multiplier from 3⅓ to 2½, and diminish potential income changes by 25 per cent if they increase the m.p.s. from 0.3 to 0.4. If they raise its value from 0.1 to 0.2, they lower the multiplier from 10 to 5 and dampen income changes by 50 per cent. Consequently, the lower the m.p.s., the greater the impact of a given set of stabilizers.[6]

The evidence indicates that a considerably higher marginal savings rate has characterized the postwar recessions than the postwar expansions. The corporate sector absorbed the major share of declines in national income, and maintained dividend payments virtually unchanged in the face of these declines. This corporate absorption led to a fairly high over-all marginal savings rate, since the marginal propensity to invest out of retained earnings is assumed to be considerably lower than the marginal propensity to consume out of disposable income.

During expansions, on the other hand, the corporate sector absorbed a much smaller share of the over-all increases in income, and also raised its dividend payments substantially. The marginal savings rate was therefore considerably lower, *on average*, permitting the stabilizers, which produced a much smaller absolute increase in that savings rate,[7] to have a sizable impact.

The above argument may be reformulated in the following way. Assuming that additional forces would not have depressed the economy had the stabilizers been inoperative, the postwar recessions would have been relatively mild even in the absence of the stabilizers (because the corporate sector would have provided a cushioning impact until the forces stimulating growth reasserted themselves). Therefore, recessions which would not have been very severe in the stabilizers' absence were made even milder by their presence. During expansions, on the other hand, the absorption by the individual sector of the bulk of changes in national income implies a sizable potential response to autonomous changes in investment (or in some other factor). The impact of what seems to be, on the basis of an analysis of built-in flexibility, a less effective set of stabilizers is thereby enhanced.

[6] This explains why the effectiveness of the stabilizers varies directly with the assumed values of c and i—the higher these values, the lower the m.p.s.

[7] The finding that the stabilizers caused a smaller increase in the m.p.s. during expansions than during recessions derives from those previous studies which measured the response of taxes and transfer payments to changes in national income.

B. *The Impact of the 1964 Tax Revision*

A comparison of the results based on historical data with those based on data adjusted to 1965 tax rates indicates that the 1964 tax revisions should have only a slight impact on the effectiveness of the stabilizers during recessions, but a somewhat greater impact during expansions. The values of α during the 1957-58 recession and the 1954-57 and 1958-60 expansions are employed in this comparison, since these were periods with an 18 per cent withholding rate and a 52 per cent corporate tax rate.

Table 2 shows that the stabilizers reduced the income decline by 34.6 to 49.9 per cent during the 1957-58 recession. They would have reduced the decline by 32.8 to 47.9 per cent had 1965 tax rates been in force, lowering their effectiveness by about 5 per cent in relative terms. During the 1954-57 and 1958-60 expansions, however, the stabilizers would have been up to one-sixth less effective had 1965 tax rates been in force. Thus, while they prevented from 26.7 to 44.3 per cent of the potential rise in income during the 1954-57 expansion, when the withholding rate was 18 per cent and the corporate tax rate 52 per cent, they would have prevented from 22.5 to 39.5 per cent of that rise had the withholding rate been 14 per cent and the corporate tax rate 48 per cent (as already noted, the percentages only show the stabilizers' potential impact, since government expenditures were not held constant during the period).

The 1964 tax revisions affected mainly the individual income tax, and this explains their greater impact on the stabilizers during expansions. As long as the corporate sector bears the brunt of an income decline, the substantial personal tax reduction cannot lead to more than a small diminution of tax sensitivity during downswings.

From the point of view of economic growth it is desirable that tax changes should lessen the impact of the stabilizers during expansions and not during contractions. Growth considerations therefore suggest that future tax reductions should be concentrated in the individual rather than in the corporate sector.

C. *Exclusion of the OASDI Program*

When the effects of the OASDI program are excluded from the calculations, the effectiveness of the stabilizers is reduced during recessions and enhanced during expansions. This reflects the steady rise in benefit payments throughout the postwar period, even when payments are adjusted for statutory increases in benefit levels. Consequently, the OASDI program has been stabilizing during contractions and destabilizing during expanisons (taking into account both payments and con-

tributions). This pattern is likely to persist as long as the number of persons receiving benefits continues to increase, in response to both statutory changes and the rapid growth in the number of older persons.

III. *Summary and Conclusions*

Whether the average values of *c* and *i* are at the upper or at the lower limits employed in the calculations, the stabilizers are clearly a powerful tool for dealing with cyclical fluctuations. Other things being equal, they will reduce declines in national income by about 50 per cent if the values of *c* and *i* are close to 0.9 and 0.5, respectively. They are in fact likely to have a somewhat greater impact however. In their absence the economy might be subject to even greater downward pressures than the model indicates, since psychological factors (which are not taken into account in the model) might then become adverse. In addition, a stabilizer-induced budget deficit will probably be financed in an expansionary manner.

The effectiveness of the stabilizers would be further increased by a strengthening of the unemployment compensation system. If all employees were covered, if benefit levels were raised to 50 per cent of the average weekly wage of covered workers, and if payments were extended to a uniform period of 26 weeks, benefit payments during recessions of postwar severity would be increased by about 100 per cent.[8] The effectiveness of the stabilizers would then be almost 10 per cent greater in absolute terms, enabling them to prevent 60 per cent of a potential decline in national income.

During expansions, the stabilizers would prevent over 40 per cent of potential increases in income if the average values of *c* and *i* were close to 0.9 and 0.5, respectively, and if government spending on goods and services remained unchanged. In fact, though, much of the stabilizers' impact was offset by rising government expenditures during the postwar period, though there is no indication that this reflected deliberate government policy.

If the average values of *c* and *i* are at the lower limits employed in the calculations, the stabilizers would prevent about 35 per cent of a potential income decline during recessions and close to 45 per cent if the unemployment compensation system were strengthened. During periods of expansion they would reduce a potential rise in income by about 25 per cent.

[8] For the calculations on which this conclusion is based, see my unpublished dissertation [6, pp 221-25].

Provisions for the automatic extension of the duration of unemployment benefits, while also desirable, would have a very limited impact on the stabilizers. They would tend to go into effect only towards the end of a recession.

In general, while different observers will judge the effectiveness of a given countercyclical tool by different standards, the stabilizers do appear to provide, under all but unusual circumstances, a reasonably satisfactory defense against downward pressures on the economy.[9] They do not suffer, as do discretionary policies, from problems of timing and errors of judgment, and they seem to be powerful enough (especially if the unemployment compensation system is strengthened considerably) to prevent a substantial percentage of an income decline. Their operations can easily be supplemented by antirecessionary monetary policies, and by cuts in the withholding rate if a recession gives no sign of slackening some specified time period after the previous peak.

The stabilizers do not appear to be as useful a tool during periods of expansion, however. They exercise a fairly strong restraining influence, which must be offset, at least in part, by tax reductions or by increased government spending.

[9] They obviously would not be able to deal, though, with the problems created by the existence of depressed areas or technological unemployment. These problems would require specific measures—such as job retraining and relocation allowances—for their solution

REFERENCES

1. M. O. CLEMENT, "The Quantitative Impact of Automatic Stabilizers," *Rev. Econ. Stat.*, Feb. 1960, *42*, 56-61.
2. L. COHEN, "An Empirical Measurement of the Built-In Flexibility of the Individual Income Tax," *Am. Econ. Rev., Proc.*, May 1959, *49*, 532-41.
3. ———, "A More Recent Measurement of the Built-In Flexibility of the Individual Income Tax," *Nat. Tax Jour.*, June 1960, *13*, 122-26.
4. D. CREAMER, *Personal Income During Business Cycles*. Princeton 1956.
5. J. DUESENBERRY, O. ECKSTEIN, AND G. FROMM, "A Simulation of the United States Economy in Recession," *Econometrica*, Oct. 1960, *28*, 749-809.
6. P. EILBOTT, *The Effectiveness of Automatic Stabilizers*. Unpublished doctoral dissertation, Columbia Univ., 1964.
7. A. G. HART, *Money, Debt, and Economic Activity*, 1st ed. New York 1948.
8. R. A. LESTER, "The Economic Significance of Unemployment Compensation, 1948-1959," *Rev. Econ. Stat.*, Nov. 1960, *42*, 349-72.
9. W. LEWIS, JR., *Federal Fiscal Policy in the Postwar Recessions*. Washington 1962.
10. D. LUSHER, "The Stabilizing Effectiveness of Budget Flexibility," *Policies to Combat Depression*, Princeton 1956, pp. 77-89.
11. I. MERRIAM, "Social Security Programs and Economic Stability," *Policies to Combat Depression*, Princeton 1956, pp. 205-35.
12. R. A. MUSGRAVE AND M. H. MILLER, "Built-In Flexibility," *Am. Econ. Rev.*, March 1948, *38*, 122-28; reprinted in American Economic Association, *Readings in Fiscal Policy*, Homewood 1955, pp. 379-86.

13. ———, *The Theory of Public Finance.* New York 1959.
14. P. H. PEARSE, "Automatic Stabilization and the British Taxes on Income," *Rev. Econ. Stud.*, Feb. 1962, *29*, 124-39.
15. J. A. PECHMAN, "Yield of the Individual Income Tax During a Recession," *Nat. Tax Jour.*, March 1954, *7*, 1-16.
16. P. E. SMITH, "Built-In Flexibility of the Individual Income Tax; Quarterly Estimates," *Nat. Tax Jour.*, June 1962, *15*, 194-97.

COMMUNICATIONS

Tariff Reductions and Trade in Manufactures among the Industrial Countries

This paper uses empirical evidence to examine the proposition advanced by Lawrence B. Krause that the establishment of the Common Market will seriously harm U.S. exports because "dominant suppliers"[1] in EEC countries receive increased protection from the averaging of national tariffs [5] [6]. Subsequently, the results are used to reinterpret the traditional conclusions regarding the effects of tariff reductions on the domestic economy, and additional tests are offered to indicate the predominance of intraindustry—as against interindustry—specialization in trade in manufactures among the industrial countries.

I

Upon finding that, in the case of dominant suppliers, national duties are generally lower than the common external tariff, Krause claims that "a comparison of the new EEC tariff for each commodity group with the former national tariff of the leading exporting country in intra-Community trade of each product gives the vivid impression that the new EEC tariff is much more protective than the old national tariff" [6, p. 101-2]. Now, under the assumption that "the expansion of the output of these suppliers will not significantly change their average costs" [6, p. 101], Krause reaches the conclusion that the dominant suppliers present a threat to foreign—and particularly to U.S.—exporters in the markets of the partner countries.

I have elsewhere criticized Krause's argument and noted that the effects of the Common Market on imports from third countries should properly be considered in two steps: (a) the implications of the averaging of national tariffs, and (b) the discriminatory effects of eliminating duties on intra-EEC trade. The averaging of tariffs *by itself* is likely to reduce, rather than increase, protection in the European Common Market: low-cost dominant suppliers who compete in the world market will receive greater—but largely unnecessary—protection while the lowering of duties will expose high-cost producers to foreign competition.[2] Further, I have expressed doubts about the possibility of dominant suppliers being able to expand output at constant costs to exploit the possibilities offered by tariff discrimination and to replace third-country exporters in the markets of partner countries [1].

With data available for several years since the creation of the EEC, it is now possible to test empirically the validity of Krause's hypothesis. Since, after the lowering of tariffs in the partner countries, dominant suppliers would first increase their sales at the expense of partner-country producers, Krause's

[1] The expression "dominant supplier" denotes the largest exporter in intra-area trade within each commodity category prior to the formation of the EEC.

[2] A similar argument has recently been made in [4].

TABLE 1—MARKET SHARES OF DOMINANT SUPPLIERS[a] IN INTRA-EEC EXPORTS FOR 91 MANUFACTURING INDUSTRIES

(Per cent)

Intra-EEC exports in 1958	Common Market	Belgium	France	Germany	Italy	Netherlands
Combined share of dominant suppliers[b]	51.0 (91)[c]	46.2 (22)	15.5 (8)	84.2 (50)	18.1 (5)	12.6 (6)
Combined share of second largest suppliers	21.8 (91)	33.8 (23)	36.3 (21)	5.3 (17)	18.3 (9)	40.5 (21)
Combined share of two largest suppliers	72.8 (182)	80.0 (45)	51.8 (29)	89.5 (67)	36.4 (14)	53.1 (27)
Increment in intra-EEC exports between 1958 and 1963						
Combined share of dominant suppliers	39.5	33.1	6.9	78.6	8.8	13.7
Combined share of second largest suppliers	17.0	31.7	18.9	5.6	13.7	36.7
Combined share of two largest suppliers	56.5	64.8	25.8	84.2	22.5	50.4

[a] The largest exporters in intra-EEC trade within each commodity category in the year 1958

[b] The combined share of the dominant suppliers in the intra-EEC exports of the individual member countries, or the six countries taken together.

[c] Numbers in parentheses refer to the number of suppliers in each category.

Source: Office Statistique des Communautés Européennes, *Commerce Extérieur*, 1958 and 1963.

hypothesis should be accepted or rejected, depending on whether the share of dominant suppliers in intra-EEC trade has increased or declined following the Common Market's establishment.

For purposes of the investigation, I have divided the Common Market manufacturing sector into 91 industries and have considered changes in intra-EEC trade between 1958 and 1963. The general principle of classification has been to group together products that have a high degree of substitutability in production, although the limited availability of data has often constrained our choices. In 56 cases the categories utilized correspond to three-digit SITC items, in 28 instances a four-digit breakdown has been used, and seven categories involve combining two or more three-digit items.[8]

The results shown in Table 1 indicate that the share of dominant suppliers in intra-EEC trade declined in the period 1958-63. Thus, while the dominant suppliers in individual industries, taken together, accounted for 51.0 per cent of trade among the Common Market countries in 1958, their share in the *in-*

[8] A similar classification has been used in [3].

crement of intra-area trade between 1958 and 1963 was only 39.5 per cent. It should be emphasized that this decline is not explained by changes in the identity of the dominant suppliers between 1958 and 1963, since the largest exporters in intra-area trade in 1963 provided 45.9 per cent of exports in that year, as compared to 43.4 per cent furnished by the dominant suppliers of the year 1958.

The conclusions hardly change if industry-by-industry comparisons are made. Dominant suppliers accounted for over one-half of trade among the Common Market countries in 52 out of the 91 industries in 1958, but their share in the increment of trade between 1958 and 1963 exceeded 50 per cent in 20 industries only. Moreover, among the 84 industries where intra-EEC trade expanded during this period, an absolute reduction in the exports of any one country is observed in only twelve cases; of these, the decline pertains to more than one country in two instances. Finally, considering the two largest suppliers within each industry, we find that their combined share declined from 72.8 per cent in 1958 trade to 56.5 per cent in the increment of intra-EEC trade during the period under consideration.

Similar results are obtained if we examine the export performance of the dominant suppliers in each of the five member countries. The data of Table 1 show a decline in the share of these suppliers within the intra-EEC exports of every country, the only exception being the Netherlands. The Dutch sample is rather small, however, since in 1958 the Netherlands was ahead in only six industries. And Dutch producers, too, follow the general pattern if the two largest exporters are considered in every industry, or if changes in the shares of dominant suppliers in the intra-area exports of individual commodities are calculated.[4]

Thus, dominant suppliers have apparently lost ground since the establishment of the Common Market and we are led to reject Krause's hypothesis. This conclusion is not affected if the data are regrouped according to the industry classification employed by Krause. We find, then, that in the 20 industries he considered [6, p. 103][5] the dominant suppliers accounted for 56.4 percent of intra-area trade in 1958, and their share in the increment of this trade between 1958 and 1963 declined to 40.9 per cent. Accordingly, it would appear that Krause has overestimated the ability of dominant suppliers to expand production at constant costs, and hence the chances of trade diversion.

II

Aside from their usefulness in judging the validity of Krause's hypothesis, these results have more general implications both for the theory of international specialization and for commercial policy. According to the familiar textbook exposition, multilateral reductions in duties would lead to a reallocation of resources from import-competing industries to export industries, accompanied

[4] In the latter case, adjustment has been made for intercountry differences in the expansion of the total exports of manufactured goods.

[5] We have excluded Miscellaneous chemicals and Manufactured goods, n.e.s., because these could not be identified with SITC categories.

by a contraction in the activity of the former and an expansion of the latter. Correspondingly, despite the ensuing improvements in productive efficiency, the welfare consequences of a move toward free trade are not unambiguous since—in the absence of appropriate compensation—the factors used more intensively in the production of importables would experience a decline in their real income.

The losses that import-competing industries might sustain in countries participating in multilateral tariff reductions have also received considerable attention in public discussions. The possible adverse consequences of lowering tariffs for particular industries have been emphasized by the opponents of entry into the European Common Market, especially in France and Italy, and arguments along these lines have recently been invoked in connection with the Kennedy-round of tariff negotiations. It would also appear that such fears have often slowed down the process of negotiations held under the auspices of GATT, and have limited the extent of reductions in duties.

Since, according to the traditional explanation, tariff reductions would be followed by interindustry specialization, the validity of this hypothesis requires that within each industry the largest supplier, or suppliers, of the preintegration period have the lion's share in the expansion of intra-area trade. Our results do not reveal such a tendency; instead of concentration, an increasing diversification in export patterns is indicated. Rather than increasingly specializing in industries where they had been leading exporters prior to the establishment of the European Common Market, the member countries have lost ground in these industries and have reduced reliance on them in expanding their exports.

It is suggested here that the failure of the traditional explanation stems from the inadequacies of conventional models that deal exclusively with standardized commodities. In the case of standardized goods, cost differences are the main determinants of trade, and a country cannot protect and export the same commodity. In such instances, the traditional conclusions on the reallocation of resources from import-competing to export industries follow: reductions in tariffs lead to a contraction in the former and an expansion in the latter.

Only a few manufactured goods (e.g., steel ingots, nonferrous metals, paper) traded among the industrial countries are standardized commodities, however, while the large majority are differentiated products that can be protected *and* exported. In the presence of national product differentiation, multilateral tariff reductions may lead to an increased exchange of clothing articles, automobiles, and other consumer goods, for example, without substantial changes in the structure of production. Further, the expansion of trade in machinery and in intermediate products at a higher level of fabrication, following all-round reductions in duties, may entail specialization in narrower ranges of products rather than the demise of national industries. These changes, then, would involve intraindustry rather than interindustry specialization.

While the results of the calculations constitute a rejection of the traditional explanation, they are consistent with the hypothesis that tariff reductions

would result in intraindustry specialization.[6] This hypothesis receives further support from evidence pertaining to the pattern of intra-area exports of the Common Market countries and the relationships between exports and imports in the various industries of these countries.

To begin with, the calculations reported in Table 2 show a positive correlation between the structure of intra-area exports of the individual EEC countries. It is also apparent that the export patterns of these countries have become more uniform since the establishment of the Community. Thus, the rank correlation coefficients calculated with regard to 91 industries ranked by the value of intra-EEC exports are larger in 1963 than in 1958 for all pairs of

TABLE 2—RANK CORRELATION COEFFICIENTS FOR THE STRUCTURE OF INTRA-EEC EXPORTS OF MANUFACTURED GOODS[a]

		Belgium	France	Germany	Italy	Netherlands
Belgium	1958	x	.576	.433	.403	.539
	1963	x	.791	.595	.485	.682
France	1958	.576	x	.643	.528	.651
	1963	.719	x	.760	.716	.782
Germany	1958	.433	.643	x	.416	.566
	1963	.595	.760	x	.592	.682
Italy	1958	.403	.528	.416	x	.549
	1963	.485	.716	.592	x	.688
Netherlands	1958	.539	.651	.566	.549	x
	1963	.682	.782	.682	.688	x

[a] Spearman rank correlation coefficients calculated from data for 91 industries ranked by the value of intra-area exports. All coefficients are statistically significant at the .01 level.

Source: See Table 1.

countries, with the differences amounting to at least one-fifth of the value shown for 1958. A shorthand expression of the results can be given if we consider that the unweighted average of the rank correlation coefficients rose from .53 to .67 in the period under consideration.

The increasing uniformity of export patterns in the EEC countries provides evidence that the creation of new trade among these countries as a result of the 50 per cent reduction in intra-area tariffs accomplished by 1963 has taken the form of intraindustry specialization.[7] Another approach to the problem is to consider the relative magnitudes of the export (import) balances of individual countries for the 91 commodity categories. For this purpose, I have expressed the absolute difference between exports and imports in each category

[6] Similar conclusions have been derived for the Benelux union by P. J. Verdoorn [7] and for trade in machinery and precision instruments among the major industrial countries by the present author [3].

[7] For an analysis of trade creation and trade diversion in the Common Market, see [2].

as a ratio of the sum of exports and imports and have calculated, for every country, an unweighted average of these ratios.[8] Should interindustry specialization predominate, one would expect the resulting "representative ratios" to approach unity since a country would either export or import a commodity. By contrast, in the case of intraindustry specialization, the ratios would tend toward zero because exports and imports would tend toward equality within each category.

In the countries under consideration the relevant ratios were approximately in the .4 to .6 range in 1958 and .3 to .5 in 1963, indicating the relative importance of intraindustry specialization (Table 3). And while the figures

Table 3—Representative Ratios of Trade Balances for 91 Commodity Categories

	Representative ratios of trade balances[a]	
	1958	1963
Belgium	.458	.401
France	.394	.323
Germany	.531	.433
Italy	.582	.521
Netherlands	.495	.431

[a] Calculated as unweighted averages of the ratio of the absolute difference of exports and imports to the sum of exports and imports for 91 industries by the use of the following formula:

$$\frac{1}{n} \sum \frac{|X_i - M_i|}{X_i + M_i}.$$

Source: See Table 1.

shown for a given country and a particular year are difficult to interpret, changes in these ratios over time permit us to draw inferences regarding the effects of tariff reductions on specialization. With the "representative ratios" falling in every country following the Common Market's establishment, one may conclude that reductions in duties have led to increasing intraindustry specialization in the EEC. Further, the higher values shown for Italy may be taken to indicate that intraindustry specialization increases with the development of manufacturing industry.

The results point to the importance of intraindustry specialization in trade among industrial countries and provide support to the hypothesis that trade liberalization would result in intraindustry rather than interindustry specialization. It follows that we have to revise the conclusions derived from traditional theory regarding the welfare consequences of tariff reductions.

[8] The following formula has been used in the calculations:

$$\frac{1}{n} \sum \frac{|X_i - M_i|}{X_i + M_i}$$

where X_i and M_i refer to the intra-EEC exports and imports of commodity category i, and n is the number of the commodity categories considered.

According to the traditional explanation, the reallocation of resources from import-competing to export industries would improve productive efficiency and result in a redistribution of incomes from the former to the latter. Different conclusions are reached for the case of intraindustry specialization: the welfare effects of an increased exchange of consumer goods may now consist largely of improvements in the efficiency of exchange (the satisfaction of consumer wants) whereas specialization in narrower ranges of machinery and intermediate products will permit the exploitation of economies of scale through the lengthening of production runs. Correspondingly, in the absence of declining industries, the income redistributional effects of trade liberalization are expected to be smaller than in the traditional case.

It would further appear that the difficulties of adjustment to freer trade have been generally overestimated. It is apparent that the increased exchange of consumer goods is compatible with unchanged production in every country while changes in product composition can be accomplished relatively easily in the case of machine building, precision instruments, and various intermediate products. These considerations may explain why the fears expressed in various member countries of the Common Market concerning the demise of particular industries have not been realized. There are no examples of declining manufacturing industries in any of the member countries, nor have they experienced a wave of bankruptcies. Indeed, the number of bankruptcies has fallen since the Common Market's establishment, and there is little evidence of frictional unemployment.[9]

III

In the present note, I have attempted to derive certain conclusions regarding the effects of tariff reductions on trade among the industrial countries on the basis of information about changes in trade flows since the Common Market's establishment. The experience of the EEC is of particular interest in this regard, in part because—aside from Italy—these countries are on similar levels of industrial development, and in part because the relatively large reductions in duties within a short period of time make it possible to derive conclusions regarding the effects of trade liberalization on specialization. Although further research would be necessary to explore the experience of other countries, the findings of the paper support the hypothesis that trade among the industrial countries is characterized by intraindustry rather than interindustry specialization, and the results augur well for the consequences of multilateral tariff reductions in the Kennedy-round negotiations.

BELA BALASSA*

[9] According to national statistics on bankruptcies, after a temporary increase to 17,000 in the recession year 1958, the number of business failures in the countries of the EEC declined from slightly over 16,000 in 1956 and 1957 to 14,500 in the early sixties. At the same time, while data on frictional unemployment are not reported, it is questionable that this could have been substantial in a period when total unemployment in the Common Market fell by two-thirds.

* The author is associate professor of economics at Yale University. Data collection and calculations were carried out in the framework of the Atlantic Trade Project, directed by the author and sponsored by the Council on Foreign Relations. Comments by Richard Cooper, Donald Hester, and Stephen Hymer are gratefully acknowledged.

REFERENCES

BELA BALASSA, "The Future of Common Market Imports," *Weltwirtschaftliches Archiv*, 1963, *90*(2), 292-316.

———, "Trade Creation and Trade Diversion in the European Common Market," *Econ. Jour.*, forthcoming.

———, "Trade Liberalization and 'Revealed' Comparative Advantage," *Man. School Econ. Soc. Stud.*, May 1965, 99-123.

M. JUNE FLANDERS, "Measuring Protectionism and Predicting Trade Diversion," *Jour. Pol. Econ.*, April 1965, 165-69.

LAWRENCE B. KRAUSE, "European Economic Integration and the United States," *Am. Econ. Rev., Proc.*, May 1963, 185-96.

———, "The European Economic Community and the United States Balance of Payments," in W. S. Salant ed., *The United States Balance of Payments in 1968*, Washington, The Brookings Institution, 1963, pp. 95-118.

P. J. VERDOORN, "The Intra-Bloc Trade of Benelux," in E. A. G. Robinson, ed., *Economic Consequences of the Size of Nations*, Proceedings of Conference held by the International Economic Association, London 1960, pp. 291-318.

The Weighted Average Marginal Tax Rate on Dividends Received by Individuals in the U.S.

The purpose of this study is to determine the weighted average marginal tax[1] ate on dividends received by individuals in the United States. The question o be answered is, to put it simply, "If U.S. corporations paid $1 million more n dividends to individual stockholders, how much of this $1 million would be aid to the Internal Revenue Service by these stockholders?"

This tax rate was desired for a number of reasons. First, it could be useful n the formulation of dividend policy by publicly-owned corporations. Second, t could measure the effect of personal taxation on the cost of retained earnngs in cost-of-capital calculations.[2] Third, it could be useful in forecasting the nacroeconomic effects of changes in total dividend payments. Fourth, it might e of interest to those formulating national income tax laws.

A second objective of this study is to determine the *trend* in the marginal ax rate. It was not clear to the author whether increasing dividends and increasing incomes would raise the weighted average, or whether these would be offset by a slowly increasing proportion of dividends being paid to stockholders in the lower tax brackets.

Third, during this study the United States Congress revised the tax laws, eliminating the 4 per cent dividend tax credit and reducing income tax rates,

[1] Throughout this article "tax" refers to federal income tax. State income taxes are ignored.

[2] Some students of finance maintain that income tax paid on dividends is not pertinent to the determination of cost of capital.

so it was decided to estimate the effect of the new tax laws on the weighted average marginal tax rate on dividends received by individuals.

Results

The weighted average marginal tax rate on dividends included in individual tax returns was calculated to be:

Year[a]	*Marginal Tax Rate*
1955	42%
1957	41%
1959	40%
1960	40%
1961	39%
1962	39%

Thus, under the 1954 tax code rates, the weighted average marginal tax rate on dividends received by individuals was approximately 39 per cent in 1962. The declining trend indicates that a slowly increasing proportion of dividends was being paid to stockholders in the lower tax brackets.

A separate calculation using the 1961 distribution of dividends with the new 1965 income tax rates, and eliminating the 4 per cent dividend tax credit, showed the expected weighted average marginal tax rate on dividends received by individuals for 1965 to be approximately 36 per cent. Although the marginal tax rate will rise by 4 per cent because of the elimination of the dividend tax credit, this will be more than offset by a reduction of about 8 per cent in the weighted average marginal tax rate by reason of reduced personal income tax rates. A continuation of the observed declining trend would further reduce the 1965 rate to approximately 35 per cent.

Prior Estimates by Other Researchers

The author is aware of only two other estimates of the weighted average marginal tax rate on dividends received by individuals. The first estimate was made by D. M. Holland: his figure for 1952 was 55.6 per cent [3, p. 120]. The second was made by G. Donaldson using data for 1959: his estimate was a weighted average tax "range" of from 52 per cent to 63 per cent [2, p. 128]. Details of how these two estimates were obtained are lacking, but it would appear that they were very rough and did not take into consideration differences in tax rates paid by different types of individual stockholders. The author feels, therefore, that the validity of the present results is much greater than the validity of these prior estimates. The author hastens to add that in the case of both of the other studies the calculation of marginal tax rate was incidental and not the focus of the study as is the case here.

[a] The last year for which the calculations were made was 1962 because the necessary data published by the Internal Revenue Service appear two to three years after the year in question.

'ource of Data

The data used in the calculations were obtained from the *Statistics of In-ome: Individual Income Tax Returns* published for the years 1955-1962 by he U.S. Treasury Department, Internal Revenue Service. These published 'ata are based upon stratified systematic samples consisting of 100 per cent of eturns showing adjusted gross income of $150,000 or more, and various lesser ercentages of returns with adjusted gross income under $150,000. The total ize of these samples ranged from almost 250,000 returns (0.42 per cent of all eturns filed) for the year 1955, to a total sample of 484,952 returns (0.77 per ent of the total number filed) for the year 1962.[4]

The data used understate dividends received by reason of the so-called 'dividend gap," described by Holland as made up partly of dividends that did ıot have to be reported under the law, and partly of dividends that legally hould have been reported but were not. Holland made different estimates of his gap using a variety of assumptions; in general it appears to be of the ırder of 10 per cent of total dividends received by individuals [3, Ch. 2]. One an only speculate as to the effect these dividends would have on the weighted ıverage marginal tax rate, but it seems clear to the author that the direction ıf the change would be toward lowering the rate. This is because dividends egally unreported would have entered the calculation at a zero per cent marginal tax rate; the illegally undeclared dividends must have been distributed oward the lower income tax brackets by reason of the vigilance of the IRS in hecking the tax returns of individuals in high income brackets.

˜alculations

The general calculations used are illustrated in Table 1. The amounts of otal dividends received by each category were obtained by rearrangement of he IRS data. The weighted average marginal tax rates for each of the first three categories were obtained by rather lengthy calculations, the details of which are available from the author. Basically these calculations involved the onversion of adjusted gross income classes to taxable income classes (with adjustments for capital gains income where appropriate), and the weighting of the marginal tax rate of each class by the dividends received by that class.

Use of Rate by Individual Corporations

The figure obtained for the marginal tax rate might not always be applicable directly to individual corporations for two reasons. First, not all stockholders are individuals; there are also corporate stockholders, nonprofit foundations, and others not taxed as individuals. Since a corporation can segregate the ownership of its stock by type of stockholder without much difficulty, however, and since the marginal tax rate of stockholders other than individuals can be estimated easily, the individual corporation could calculate the

[4] The relative sampling variability of these data can be obtained from the individual *Statistics of Income*. The author believes that their effect is insignificant in this study.

TABLE 1—GENERAL CALCULATIONS FOR 1962

Category	Total Dividends Received (millions)	Weighted Average Marginal Tax Rate for Category	
1. Joint returns with taxed dividend income	$ 6,559	42.5%	2,788
2. "Single" returns with taxed dividend income	$ 3,016	42.2%	1,273
3. Other returns[a] with taxed dividend income	$ 676	44.8%	303
4. Nontaxable returns	$ 780	0%	0
5. Taxable returns in which dividend income was completely covered by dividend exclusion	$ 48	0%	0
	$11,079		4,364

$$\text{Over-all weighted average marginal tax rate} = \frac{4,364}{11,079} = 39.4 \text{ per cent}$$

[a] "Other returns" include separate returns of husbands and wives, heads of households, and surviving spouses.

weighted average marginal tax rate paid by *all* of its stockholders given this rate for its individual stockholders.

A more serious difficulty is that the composition of the particular corporation's individual-stockholder group may differ from the national average, making the national rate inapplicable. This is particularly true in closely-held corporations, but in this case it is often possible to obtain the marginal tax rates of the stockholders directly. In widely-held corporations, there is the possibility that to a certain degree investors in high tax brackets will avoid dividend-paying stocks and seek capital gains more actively. Some vague evidence of this was found by J. K. Butters, L. E. Thompson, and L. L. Bollinger in their 1949-1950 surveys of investment policies followed by individuals.

> Except in very high income classes only a minority of individuals profess to have taken taxes into account in formulating their investment policies. Such shifts as have occurred appear to have been on the whole moderate in degree. Moreover, they have been opposite in direction, though the shifts toward more conservative investment holdings have substantially outnumbered those toward more venturesome holdings [1, pp. 178-79].

Butters, Thompson, and Bollinger, as might be expected, also found differences in investment objectives related to age, sex, and area (major metropolitan vs. nonmetropolitan areas). Obviously the composition of the stockholder group of an individual corporation can be expected to differ to some degree from the national average with regard to distribution among the various income tax brackets, but in the case of widely-held corporations this difference is not likely to be large.

Thus, the use of the 1965 rate determined in this study should enable many managements of individual corporations to estimate with reasonable accuracy

the weighted average marginal tax rate applicable to dividends paid to their own stockholders. This information is of use in dividend decisions and the calculation of cost of capital.

VINCENT JOLIVET*

* The author, professor of finance and statistics at the University of Washington, is indebted to Professor J. T. S. Porterfield for reviewing the original manuscript and to Mr. P. French for assistance with the computations.

REFERENCES

1. J. K. BUTTERS, L. E. THOMPSON, AND L. L. BOLLINGER, *Effects of Taxation: Investments by Individuals.* Boston 1953.
2. G. DONALDSON, "In Defense of Preferred Stocks," *Harvard Bus. Rev.*, July-Aug. 1962, *40*, 123-36.
3. D. M. HOLLAND, *Dividends Under The Income Tax.* Princeton 1962.

The Graduated Fair Return: A Regulatory Proposal

An examination of the regulated industries reveals a broad spectrum of rationales for government's deviation from the path of a *laissez faire* policy. One justification offered is that the industry is a "natural monopoly." A second rationale is the need to protect consumers in the sellers' markets of the services of public utilities. While these and other justifications for interference with the natural course of the market's operation are given, there still exists no precise line separating regulated from unregulated industries. Neither economists nor courts have drawn a strict line of demarcation. Despite this inability or unwillingness to delimit the two classes, the general goal of regulation is to improve the performance of the economy, in welfare terms, as compared with what it would be if the areas now regulated were uncontrolled.

It would seem, then, that public regulation should take a form that would enable the economy to come as close as possible to the achievement of a welfare optimum. Harvey Averch and Leland L. Johnson [1] have demonstrated, however, that the regulatory control most commonly used today, the "fair rate of return" criterion, induces a serious deviation from the optimal allocation of inputs. A welfare optimum would require that the marginal rate of substitution between any two factors equal the inverse of the ratio of their marginal costs [3, p. 140] [4, p. 41]. The fair rate of return does not lead to such an optimal state. Instead, it induces the firm subject to such regulatory control to use "noncapital" (call it labor) to a point where marginal revenue product equals marginal cost but to use capital at a level at which its marginal cost exceeds its marginal revenue product.

Nevertheless, regulatory commissions seem wedded to the notion of an equitable rate of return for the regulated industries. It seems logical that if we are to regulate by means of a fair-rate-of-return criterion, the input-allocation efficiency of regulation within this framework should be improved. To this end, this note proposes a regulatory scheme—the "graduated fair return" criterion—which remains within the framework of an equitable

return but which is more efficient in allocating inputs than is the present form of control.

The firm we are concerned with is a monopoly employing two inputs, capital (K) and labor (L) to produce a single homogeneous output (Y). The production function of the firm is assumed to be of the following form:

$$(1) \qquad \begin{cases} Y = Y(K, L), \quad K \geqq 0, \quad L \geqq 0; \\ \dfrac{\partial Y}{\partial K} > 0, \quad \dfrac{\partial Y}{\partial L} > 0, \quad Y(0, L) = Y(K, 0) = 0. \end{cases}$$

The output of the firm is a function of the inputs of capital and labor, each of which has a positive marginal product. Positive quantities of both factors are required if any output is to result. The price (P) of the product is related to the output of the product by the inverse of the demand function. That is,

$$(2) \qquad P = P(Y), \quad P'(Y) < 0.$$

We further assume that while the firm is a monopolist, it has no control over the price of its inputs. Hence, the costs of its inputs, r for capital and w for labor, are assumed fixed for all levels of purchase. Denoting profit by Π, we define it as

$$(3) \qquad \Pi = PY - rK - wL.$$

It will be assumed that this profit function is concave.[1]

For the sake of simplicity, without seriously diminishing the insights to be derived, let us assume that the value of depreciation of plant and equipment in the time period being considered and the cumulative value of depreciation are both zero.[2] In addition, we take the acquisition cost of a unit of capital to be unity. Hence, the value of the capital stock is equal to the physical quantity of capital (K). It should be noted that while we have posited that the acquisition cost of capital is unity, we have made no assumption beyond its constancy about the cost of capital, r, which is the interest cost involved in holding plant and equipment.

Having specified the model, it remains to define the efficiency in input allocation of a particular regulatory control. Under an optimal allocation of resources, with pure competition in buying factors, the marginal rate of substitution between any two factors will be equal to the inverse of the ratio of their prices. That is, in the present instance, the marginal rate of substitution of capital for labor ($\partial L/\partial K$) would equal the ratio of the cost of capital to the cost of labor (r/w). We shall define the efficiency (E) of a regulatory control as an input allocator to be the ratio

$$(4) \qquad E = \frac{\partial L/\partial K}{r/w}.$$

[1] This enables us to limit our concern to the first-order conditions for profit maximization, since the second-order conditions will be fulfilled if the function to be maximized is concave.

[2] For a discussion of this assumption, see [1, p. 1054].

The efficiency of a particular control increases as E increases from zero to one, the latter being the point of maximum efficiency. Once E exceeds unity, the control is once again inefficient, its efficiency now decreasing as E increases.

The fair-rate-of-return criterion imposes the following constraint upon the regulated firm: after subtracting its operating costs from its gross revenue, the firm's net revenue should not exceed a given percentage (the fair rate of return) of the value ascribed to its property net of depreciation (the rate base). Since depreciation has been assumed away, the operating expenses of the firm in the present model are only labor costs, and the rate base is equal to the original value of the firm's capital stock. Moreover, with the cost of acquiring capital set at unity by assumption, the firm's rate base is equal to the physical quantity of capital it possesses. If we denote by s the fair rate of return determined by the regulatory agency, the regulatory constraint may be written as

$$\frac{PY - wL}{K} \leqq s \quad \text{or} \tag{5}$$

$$PY - sK - wL \leqq 0. \tag{6}$$

To determine the firm's optimal position when it operates subject to this regulation, Averch and Johnson make use of the Kuhn-Tucker conditions for a constrained maximum. These conditions are sufficient as well as necessary under our assumption of concavity of the profit function [2]. The firm attempts to maximize (3) subject to the constraint (6). The following Lagrangian function is formed:

$$\phi(K, L, \lambda) = PY - rK - wL - \lambda(PY - sK - wL). \tag{7}$$

As Averch and Johnson demonstrate [1, pp. 1055–56] the realm of interest is that in which $0<\bar{\lambda}<1$ where $[\bar{K}, \bar{L}, \bar{\lambda}]$ is the constrained maximum point. Under these conditions, the constraint is effective ($\bar{\lambda}>0$) and the fair rate of return exceeds the market cost of capital ($\bar{\lambda}<1$). Using the Kuhn-Tucker conditions, it can be shown [1, pp. 1055–56] that at the point of constrained maximization, with $\bar{L}>0$,

$$w = \left(P + Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial L}. \tag{8}$$

Labor is used up to the point where its marginal cost is equal to its marginal revenue product. At the same time, with $\bar{K}>0$,

$$r - \bar{\lambda}s = (1 - \bar{\lambda})\left(P + Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}. \tag{9}$$

We may, however, rewrite (9) as

$$r - \frac{\bar{\lambda}(s - r)}{1-\lambda} = \left(P + Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}. \tag{10}$$

Since $0<\bar{\lambda}<1$ and $s>r$, it is clear that

$$\frac{\bar{\lambda}}{1-\bar{\lambda}}(s-r)>0.$$

Consequently, capital is used up to a point at which its marginal cost exceeds its marginal value product,

$$r>\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}. \tag{11}$$

The monopolistic firm maximizing profits subject to a fair-rate-of-return regulatory constraint uses labor efficiently, but it uses capital inefficiently.

The efficiency of this regulatory constraint can be found easily by dividing (10) by (8). We find

$$\frac{\partial L}{\partial K}=\frac{r}{w}-\frac{\bar{\lambda}}{1-\bar{\lambda}}\left(\frac{s-r}{w}\right). \tag{12}$$

Since

$$\frac{\bar{\lambda}}{1-\bar{\lambda}}(s-r)>0$$

and since $w>0$, we have

$$\frac{\partial L}{\partial K}<\frac{r}{w}. \tag{13}$$

By the definition of input efficiency, E, it is clear that $E<1$ for the fair-rate-of-return constraint. It is, in fact, equal to

$$E=1-\frac{\bar{\lambda}}{1-\bar{\lambda}}\left(\frac{s-r}{r}\right). \tag{14}$$

The inefficiency (*as an input allocator*) of the present fair-rate-of-return criterion derives from the constancy of the maximum allowable rate of return. The owners of the monopolistic firm considered could earn a net return of $s-r$ per cent on every unit of capital employed. The true marginal revenue product of capital may be, and most probably is, changing with the amount of capital and labor used. But the regulated firm's concern is not confined to relating this changing marginal revenue product to the marginal cost of capital when it determines the amount of capital to use. Instead, the firm must also weigh the constant net return of $s-r$ per cent it receives on the extra unit of capital for just bringing it into the production process. If the maximum allowable rate of return itself changed as capital was added, the effect of this inefficiency on the allocation of inputs could be diminished. This variable maximum allowable rate of return is the heart of the proposal made in this paper.

The fair rate of return available to a firm would change as the size of its capital stock changed. Either the equitable rate of return would be a decreasing function of the capital stock's size as soon as capital accumulation began or it would begin to decline only after the capital stock reached a certain threshold level. The essence of the proposal is that after some point the maximum allowable rate of return on capital would decline with increases in the amount of capital used. The fair return would, after some point, be graduated downward.

The graduated fair return could be implemented in one of two ways. The function relating the rate of return to the capital stock's size could be a continuous one or it could be a step function. In the first instance, after the fair return has begun its decline with further increments to the capital stock, each addition to that stock would call forth a new lower level of the equitable rate of return. On the other hand, the step-function relationship would involve the establishment of brackets of capital stock levels with declining rates applying to each bracket and with a constant rate applying within each bracket. For example, the firm might be allowed a maximum return of 8.0 per cent on the first \$100,000 of its capital stock, a maximum of 7.5 per cent on the next \$80,000, a maximum of 6.8 per cent on the next \$65,000, and so on. Obviously, in either case, the regulatory agencies which presently dictate the constant fair rate of return would be required to determine, after careful study by their research staffs, the functional relationship that should exist between the rate of return and the capital stock's size. This schedule, and I presume the step-function form would be the one adopted if either was employed, would undoubtedly vary from one regulated industry to the next.

To demonstrate that this graduated-fair-return criterion can, with a properly constructed function, constitute an improvement upon the present fair-rate-of-return method of control as an allocator of inputs, assume that the maximum allowable rate of return does begin to decline from the start. That is, for simplicity, suppose that the threshold level is zero. In order further to simplify the analysis, we posit the introduction of the first type of graduated-fair-return regulation, the continuously declining fair return. The constraint now imposed upon the monopolistic firm of the model is

$$\frac{PY - wL}{K} \leqq s(K) \quad \text{or} \tag{15}$$

$$PY - Ks(K) - wL \leqq 0, \tag{16}$$

where $s(K)$ is the continuous function relating the maximum allowable rate of return, s, to the size of the capital stock, K, and $s'(K) < 0$ for $K > 0$.

The monopolistic firm subject to the graduated-fair-return criterion maximizes its profit (3) subject to (16). The relevant Lagrangian expression is

$$\psi(K, L, \eta) = PY - rK - wL - \eta(PY - Ks(K) - wL). \tag{17}$$

The Kuhn-Tucker conditions for a maximum at the level of output given by K^0, L^0, η^0 are

$$(18)\quad (1-\eta^0)\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}-r+\eta^0[s(K^0)+K^0s'(K^0)]\leqq 0,\ K^0\geqq 0$$

$$(19)\quad (1-\eta^0)\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial L}-(1-\eta^0)w\leqq 0,\quad L^0\geqq 0,$$

$$(20)\quad K^0\left\{(1-\eta^0)\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}-r+\eta^0[s(K^0)+K^0s'(K^0)]\right\}=0,$$

$$(21)\quad L^0\left\{(1-\eta^0)\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial L}-(1-\eta^0)w\right\}=0,$$

$$(22)\quad \eta^0(PY-Ks(K)-wL)=0,\quad \eta^0\geqq 0.$$

Our concern is with the case in which the regulatory constraint is effective that is, when $\eta^0>0$. If $\eta^0=0$, then the strict inequality can obtain in (16) so that $PY-wL<Ks(K)$. Since $\Pi=PY-rK-wL$, this means that $\Pi+rK<Ks(K)$ or $\Pi<[s(K)-r]K$. In short, when the constraint is ineffective, $\eta^0=0$ and the regulatory commission has designed its graduated-fair-return function so that at the firm's optimum $[s(K)-r]K$ exceeds the value of unconditionally maximized profit.

Turning to the case where the constraint is effective, (19) and (21) show that if any labor is used (which must be the case if any output is to be produced),

$$(23)\qquad w=\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial L}\cdot$$

Labor is employed up to the point where its marginal revenue product equals its marginal cost. The conditions of (18) and (20) indicate that with $K^0>0$,

$$(24)\qquad r-\eta^0[s(K^0)+K^0s'(K^0)]=(1-\eta^0)\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}\cdot$$

But this can be rewritten as

$$(25)\qquad r-\frac{\eta^0}{1-\eta^0}[s(K^0)+K^0s'(K^0)-r]=\left(P+Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}\cdot$$

The sum $s(K^0)+K^0s'(K^0)$ is the value of the derivative of $Ks(K)$ with respect to K at the point of optimal output. It is the marginal return to the firm for the use of capital. Clearly, the schedule of equitable rates of return could be constructed so that at some point this marginal return is equal to the cost of capital, r. However, this involves removing entirely the decision concerning the quantity of capital to be used from the realm of the firm

and placing it, instead, completely with the firm's regulators. It is the case where $\eta^0 = 1$. Consequently we must confine our attention to the case where $\eta^0 < 1$ if we are intent upon examining the behavior of the *firm* subject to the constraint.

Attention is now centered upon the case in which $0 < \eta^0 < 1$, so that the constraint is effective and the marginal return on capital always exceeds the market cost of capital, allowing the firm to make its own decisions. Under these conditions, the second term on the left-hand side of (25),

$$\frac{\eta^0}{1 - \eta^0}\,[s(K^0) + K^0 s'(K^0) - r], \tag{26}$$

is positive. We find, as was the case with the constant-fair-rate-of-return control,

$$r > \left(P + Y\frac{dP}{dY}\right)\frac{\partial Y}{\partial K}\,. \tag{27}$$

Capital is used inefficiently, its marginal value product lying below its marginal cost. But it is used less inefficiently under a properly constructed graduated-fair-return criterion than under a constant one. The relevant comparison is the one in which the respective constraints are equally effective. This is the case in which the marginal value products of the two constraints (their dual prices) are equal. It occurs when the corresponding Kuhn-Tucker multipliers are equal, when $\bar{\lambda} = \eta^0$. Then, for equal fair rates of return, the graduated-return criterion is more efficient than the constant-return one. With $s = s(K^0)$, we find $s - r > s(K^0) + K^0 s'(K^0) - r$ since $s'(K^0)$ is negative by construction and K^0 is positive. Hence,

$$\frac{\bar{\lambda}}{1 - \bar{\lambda}}\,(s - r) > \frac{\eta^0}{1 - \eta^0}\,[s(K^0) + K^0 s'(K^0) - r].$$

A comparison of (10) and (25) now shows that r is closer to the marginal value product of capital under the graduated-return criterion.

The marginal rate of substitution between the factors under the graduated-return criterion is

$$\frac{\partial L}{\partial K} = \frac{r}{w} - \frac{\eta^0}{1 - \eta^0}\left(\frac{s(K^0) + K^0 s'(K^0) - r}{w}\right). \tag{28}$$

Dividing equation (28) by r/w, we find that the efficiency, as defined in (4), of the graduated-return method of control is

$$E = 1 - \frac{\eta^0}{1 - \eta^0}\left(\frac{s(K^0) + K^0 s'(K^0) - r}{r}\right). \tag{29}$$

This contrasts with the efficiency of the constant-fair-rate-of-return criterion given in (14). Once again, for $\bar{\lambda} = \eta^0$ and $s = s(K^0)$, which are the relevant conditions for a comparison, it can be seen that the negativity of $s'(K^0)$ ensures

that regulation under a graduated-fair-return scheme is more efficient as an allocator of inputs: E is closer to unity for the graduated criterion.

It is not asserted that the method of control proposed here is flawless. It requires careful planning by the staff economists of the many public utility commissions. It does not necessarily achieve an optimal allocation of inputs as judged by the relevant welfare criterion. In fact, we have seen that its efficiency is generally less than unity if input allocation is left within the realm of the firm's decision-making process. However, if it is desired to remain within the confines of a fair-rate-of-return regulatory program, it is incumbent upon us to look for alternative ways to control regulated industries so as to improve upon the inefficient criterion presently in use. The graduated-fair-return criterion is put forth here as such an alternative method of control.

ALVIN K. KLEVORICK*

* The author is a graduate student at Princeton University. He is grateful for the helpful comments and suggestions of Professors Jesse W. Markham and F. M. Scherer.

REFERENCES

1. H. AVERCH AND L. L. JOHNSON, "Behavior of the Firm Under Regulatory Constraint," *Am. Econ. Rev.*, Dec. 1962, *52*, 1052–69.
2. H. W. KUHN AND A. W. TUCKER, "Nonlinear Programming" in J. Neyman (ed.), *Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability*, Berkeley 1951, pp. 481–92.
3. A. P. LERNER, *The Economics of Control*. New York 1949.
4. M. W. REDER, *Studies in Welfare Economics*. New York 1947.

Omissions in the Treatment of the Law of Variable Proportions

The purpose of this paper is to clarify two points with respect to the treatment of the Law of Variable Proportions (hereafter referred to as the Law) in most economics textbooks. The first concerns cost and product relationships in Stage III, commonly called the "irrational" region[1] of production, and the second deals with the symmetry of Regions I and III.

Although Stage III is an irrational area of production, it is not uncommon for firms lacking perfect knowledge actually to produce in that region, particularly in agriculture. As Bishop and Toussaint note [1, p. 41]:[2]

> . . . evidence of production in Region III is often noted. For example, during the late summer and fall months, we frequently have evidence of too many cattle on a given quantity of pasture, resulting in overgrazing of pastures and less production than could have been obtained with fewer cattle. Also, we find evidence of overcrowding of broilers and layers in poultry houses.

[1] The terms "stage," "region," and "phase" are used interchangeably in this paper.

[2] For additional examples, see Heady [3, pp. 94 and 170].

egardless of the empirical incidence, however, the chief reason for examining st curves in Stage III lies in presenting a complete and consistent treatment the product-cost relationships in all three phases of the Law.

Most of the textbooks on economics discuss the various product functions id their interrelationships for all three phases of the production function in nsiderable detail. However, the corresponding cost functions are derived ily for the first two phases of the Law and the relationship between product id cost functions in Stage III has been neglected, stated ambiguously, or isinterpreted.

The common practice is to draw a standard Sigmoid-type total cost function id its corresponding average and marginal cost functions. This tends to leave e reader either wondering as to the product-cost relationships in the third iase or under the erroneous impression that the total, average, and marginal st functions as drawn are for all phases of the Law and that they continue have a positive slope, even though the total product function assumes a gative slope in the third region.[3] The writer has found both undergraduate id graduate students curious and confused about this.

The cost-product diagram (Figure 1) reproduced from a recent textbook on ricultural economics by Snodgrass and Wallace [6, p. 235] will show that e confusion is not confined to students alone. In providing a vivid illustra- on of the cost-product relationships, the authors completely overlook the int that the AVC function cannot continue to rise with a positive slope, as own in Figure 1, for the output beyond point *C* on the TPP function or on e MPP function, because from *C* onwards the TPP is falling and the MPP is gative. It is true that the authors recognize "that the horizontal and vertical es in each of the three figures [panels a, b, and c, in Figure 1] . . . are not mparable on a quantitative basis [but] the importance is to show the rela- onship between various points on the physical product curves and the cost irves . . . " [6, p. 235]. However, it is in this effort to show the relationship etween various points on the product and cost functions that they either ake the said error or at least fail to warn the reader anywhere in the book iat such an error is due to the lack of exact quantitative correspondence be- veen the horizontal and vertical axes of the three panels of the figure.

I. *Interrelationships Between Product and Cost Functions*

Figures 2 and 3 illustrate product-cost relationships for all phases of the aw. The product functions in Figure 2 are based on the production func- on:[4]

[3] Heady's book [3, p. 94] is one of the few texts which advises the reader explicitly iat the stages of increasing average and diminishing total returns are not drawn in ibsequent chapters of the book. To the best of my knowledge, Vickrey [8, pp. 159-60] the only author who makes a brief reference to and draws very tiny portions of the ackward-rising AVC and TVC functions. However, he does not show the complete in- rrelationships between various product and cost functions.

[4] This production function is taken from Heady's book [3, p. 39, n.1], except that the onstant term, 40, has been dropped in order to keep our TPP function similar to the imiliar textbook TPP which usually starts from the origin.

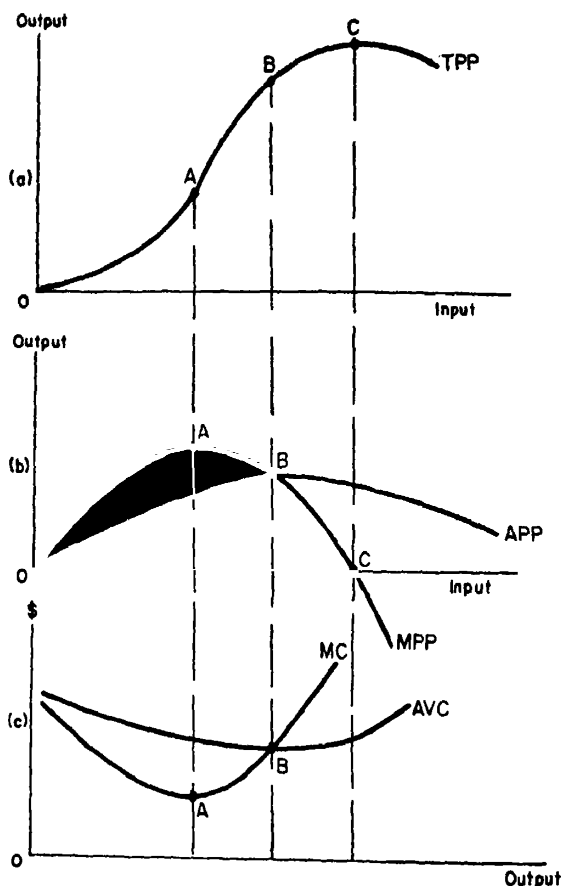


FIGURE 1. PRODUCT AND COST CURVES

Source: Reproduced from Fig. 13-2 (p. 235) *Agriculture, Economics, and Growth* by Milton M. Snodgrass and Luther T. Wallace. Copyright © 1964 by Meredith Publishing Company. Reproduced by permission of Appleton-Century-Crofts, Division of Meredith Publishing Company.

$$(1) \qquad Y = 4.0X + .5X^2 - .15X^3$$

where Y represents total physical product (TPP), and X stands for the variable input which is being applied to a fixed amount of another input. This fixed input (not explicitly included in the production function) is assumed to be perfectly divisible and adaptable. The marginal physical product (MPP) function, therefore, is given by

$$(2) \qquad dY/dX = 4 + 1.0X - .45X^2.$$

In deriving cost functions (Figure 3) from product functions (Figure 2) it is assumed that the input market is perfectly competitive, so that the variable factor of production can be purchased at a constant price per unit

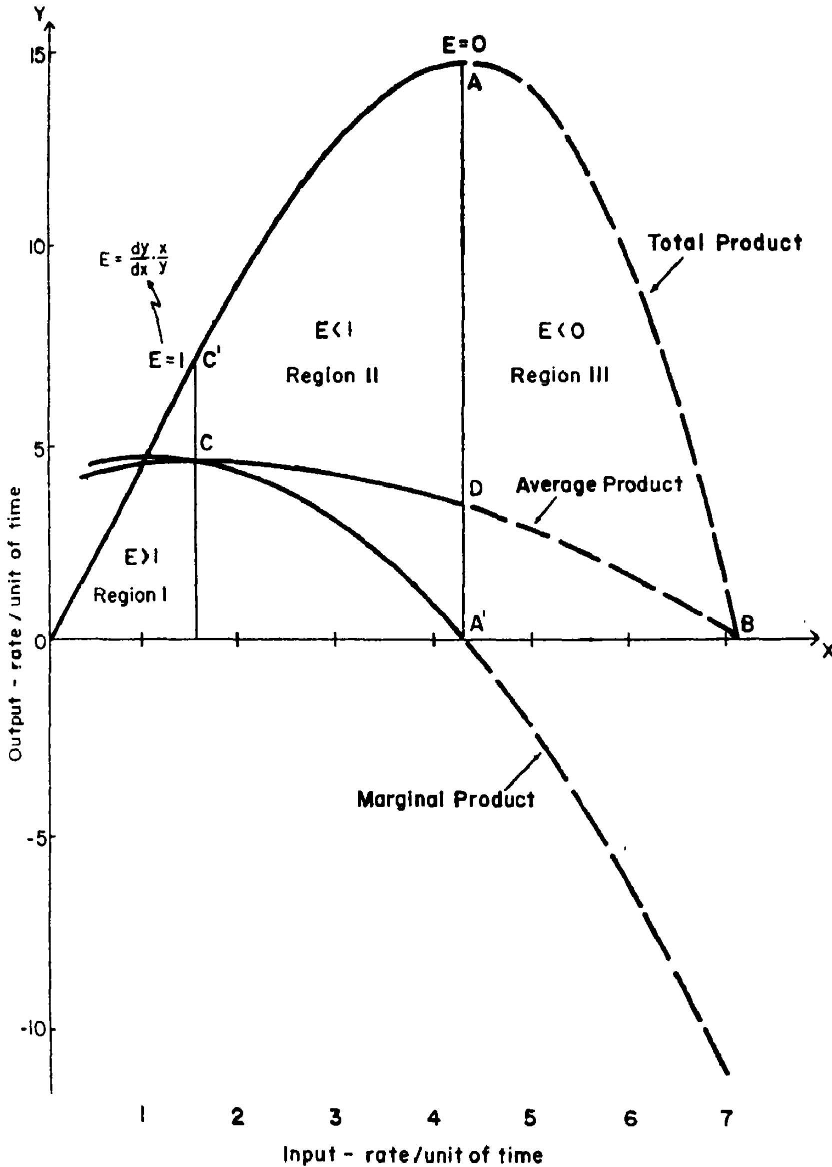


FIGURE 2. FACTOR-PRODUCT RELATIONSHIPS

of the input, irrespective of its quantity bought. The assumed input price, $P_x = \$3.00$ per unit.

From Figures 2 and 3 it will be noted that when Region III, the area of diminishing *total* returns, is included in the cost analysis, the various standard cost functions take on some peculiar attributes. In Figure 3, the total variable cost function (TVC) no longer remains single-valued; instead

it becomes bi-valued. The TVC for all phases of the Law is denoted by *OAB;* the broken segment *AB* represents TVC for Stage III. Notice that in this region, TVC continues to rise until point *B*, a point representing a high positive cost but zero output, and has a negative slope because output is continuously falling, a phenomenon quite consistent with Stage III.

The average variable cost (AVC) function in Figure 3, represented by *aDa′*, develops a cusp at *D*, a point representing the end of Stage II and beginning of Stage III. After this point, the broken line *Da′* denotes AVC for Stage III. In this region, since output begins to fall after point *D, Da′*

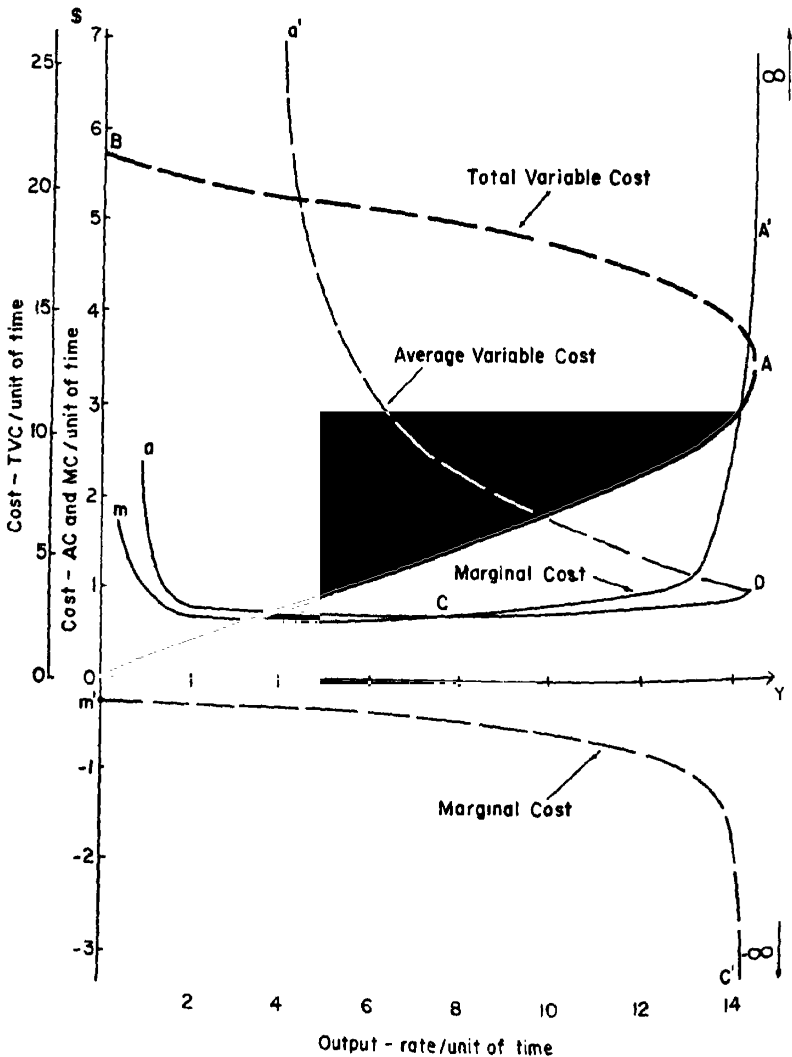


FIGURE 3. COST-PRODUCT RELATIONSHIPS

assumes a negative slope. It is of the same general shape as the AVC function, aD, in Stages I and II, but is at a higher cost level.

Again, diminishing *total* returns in production cause a discontinuity in the marginal cost (MC) function. The complete MC function for all regions of the Law is shown by $mCA'C'm'$ (Figure 3), where mCA' represents the MC function for Stages I and II, and $C'm'$ denotes MC for Stage III. It will be noted from Figure 3 that at point A' (maximum output), the MC function attains a value of $\pm\infty$. Therefore after A', the MC function for Stage III, $C'm'$, is (1) a discontinuous function at the maximum output level, and (2) is negative over the entire region (III) because the associated MPP function for this region in Figure 2 is negative.

In our traditional treatment of costs, we usually do not think in terms of negative costs. Negative MC is not to be construed to mean that for a certain rate of output the cost rate is negative (i.e., the entrepreneur is saving) but implies, instead, that after a certain point (A') if an effort is made to increase output, cost changes are positive even though output changes are negative, a phenomenon quite consistent with the input-output behavior, and the total cost-total product relationship in Stage III.

II. *Symmetry of Regions I and III*

This paper will conclude with clarification and elaboration of two final properties of the Law, viz., (1) interfactoral symmetry[5] (or reversibility) in Regions I and III, and (2) the relationship, if any, between factor symmetry and product symmetry in Regions I and III. As early as 1936, Cassels in his celebrated article [2, p. 108] remarked: "The most important thing to observe about this law is that it is symmetrical and consequently the third phase is simply the converse of the first." Yet, most textbooks make scanty or no reference to this point.[6] Furthermore, of the writers who do discuss the symmetrical nature of the Law, few, including Cassels, bring out the practical significance of the Law. Except for suggesting one operational aspect, namely, that total output can be increased in either Region I or Region III by reducing the relative intensity of the excessive factor, most of the writers leave the reader with the impression that symmetry of the Law is a mere theoretical nicety.

In our analysis of factor-product relations, Figure 2, it would be recalled that factor X (explicitly included in the production function) was treated as a variable factor of production. It was applied in equal incremental units to a given amount of some other factor (not explicitly included in the production function) which was treated as a fixed factor of production. Thus, in Region I, where the ratio of the variable factor to the fixed factor is relatively low, the marginal physical product for the variable factor is positive and greater than the average physical product (until $MPP_X = APP_X$

[5] The term "symmetry" is used here to imply reversibility of factor proportions (or factor position) rather than strict mathematical symmetry.

[6] Of some 15 texts consulted, only a few, viz., Heady [3, pp. 76-78], Leftwich [4, pp. 116-21], and Stigler [7, p. 125] discuss this point adequately. Even as good a text as Samuelson's [5, pp. 518-19] devotes only 5½ lines to this point.

at the outer boundary of Region I).[7] In Region III, however, where the ratio of the variable factor to the fixed factor is relatively high, MPP of the variable factor is negative and below the APP of this factor.

By reversing the positions of these factors, one can easily see that a low variable-fixed factor ratio in Region I means a high fixed-variable factor ratio in the same region. Accordingly, when the MPP for our variable factor is positive in this region, it is negative for the other input in the same region. Likewise, in Region III a high variable-fixed factor ratio means a low fixed-variable factor ratio in the same region. Thus, when the MPP for our variable factor is negative, it is positive for the other factor. In short, Region I for one input is Region III for some other input, and vice versa.[8]

The importance of factor reversibility lies not only in deciding *when* a particular factor of production should be decreased to increase production but also in deciding *what* factor of production should be treated as variable or fixed in a given economic environment. These are not easy decisions. They confront not only entrepreneurs at the individual level but also policy makers at the national level whenever they are concerned with optimum resource allocation. An example from Heady [3, p. 77] will illustrate the point.

> Within a cropping year, a farmer may consider hog numbers as fixed and corn as variable; he can either sell the year's corn crop or feed it in varying amounts to a given number of spring-farrowed pigs. Or, he can consider the corn crop as fixed and vary the number of hogs in proportion to it; the corn can be stored into the next year and opportunities will be open for farrowing more pigs. At the national level, food emergencies may arise which call for either feeding more or less grain to a given number of hogs within a year or storing the corn and increasing the number of hogs to which it might be fed. Which system will give greatest profits to the farmer or food to the nation?

Answers to such complicated problems can be determined only by an analysis of both the physical and economic relationships. The important point is that reversibility of the technical inputs not only allows the interchange of factor positions but also provides the necessary, though not sufficient, basis on which this choice is exercised. The usual textbook discussion of symmetry and optimum factor proportions then becomes meaningful.

The second point concerns the relationship, if any, between factor symmetry and product symmetry in Regions I and III. Having read the class assignment on the symmetry of the Law in standard sources, many students ask: If Regions I and III are symmetrical, why doesn't the TPP curve in Region III have an inflection point corresponding to the inflection point in Region I? In terms of the MPP function, why doesn't it achieve a maxi

[7] It is recognized that the location of the boundary between Regions I and II at the point where MPP = APP holds true only when the production function is homogeneous of the first degree. If either diminishing or increasing returns to scale are present, the boundary between Regions I and II will not be marked by the equality of MPP and APP.

[8] For an excellent graphic and tabular illustration of this point, see Leftwich [4, pp. 116-21].

mum negative value in Region III corresponding to the maximum positive value in Region I, and reverse its direction toward zero (*B*, Figure 2) as it does toward the zero value (*A′* Figure 2)?

These questions reveal a confusion between factor symmetry and product symmetry in Regions I and III. Even though factor symmetry makes Regions I and III the converse of each other, it does not necessarily mean that

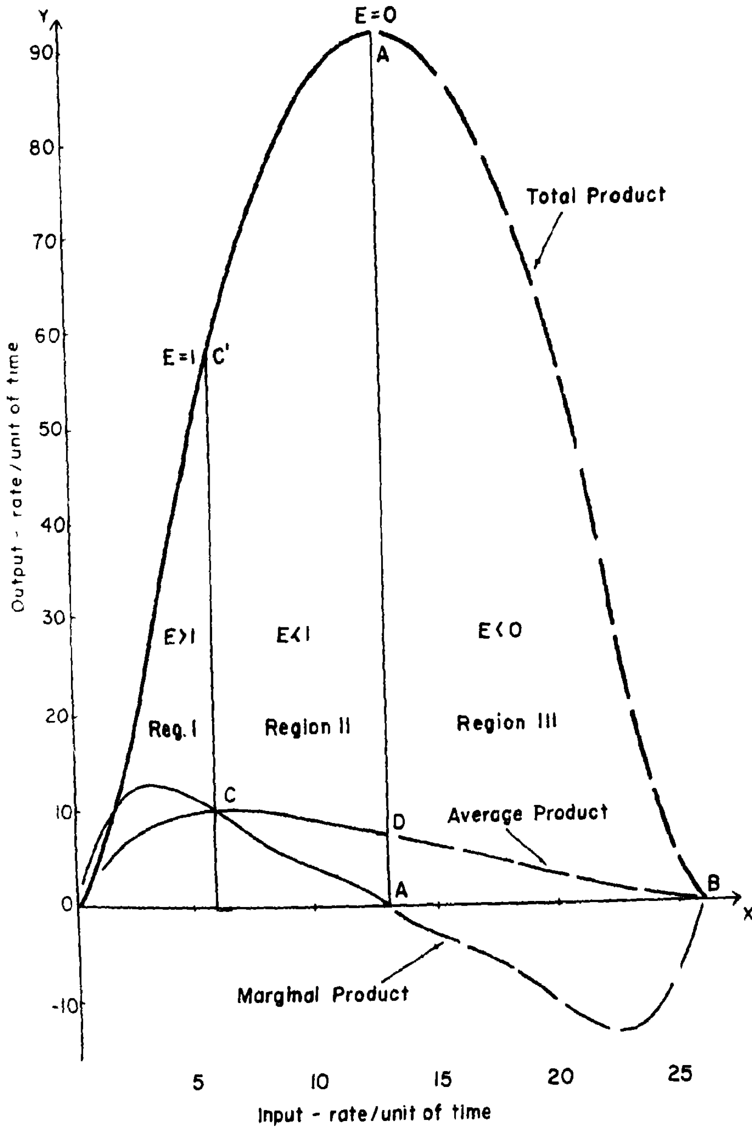


FIGURE 4. FACTOR-PRODUCT RELATIONSHIPS

the *rate* of increase in total product in Region I must be symmetrical to the *rate* of product loss in Region III. Hence the difference in the exact behavior and shapes of the TPP and MPP curves in Regions I and III.

The MPP curve in Region III would achieve a maximum negative value and reverse its direction to join *B* only if the TPP curve in Region III, un-

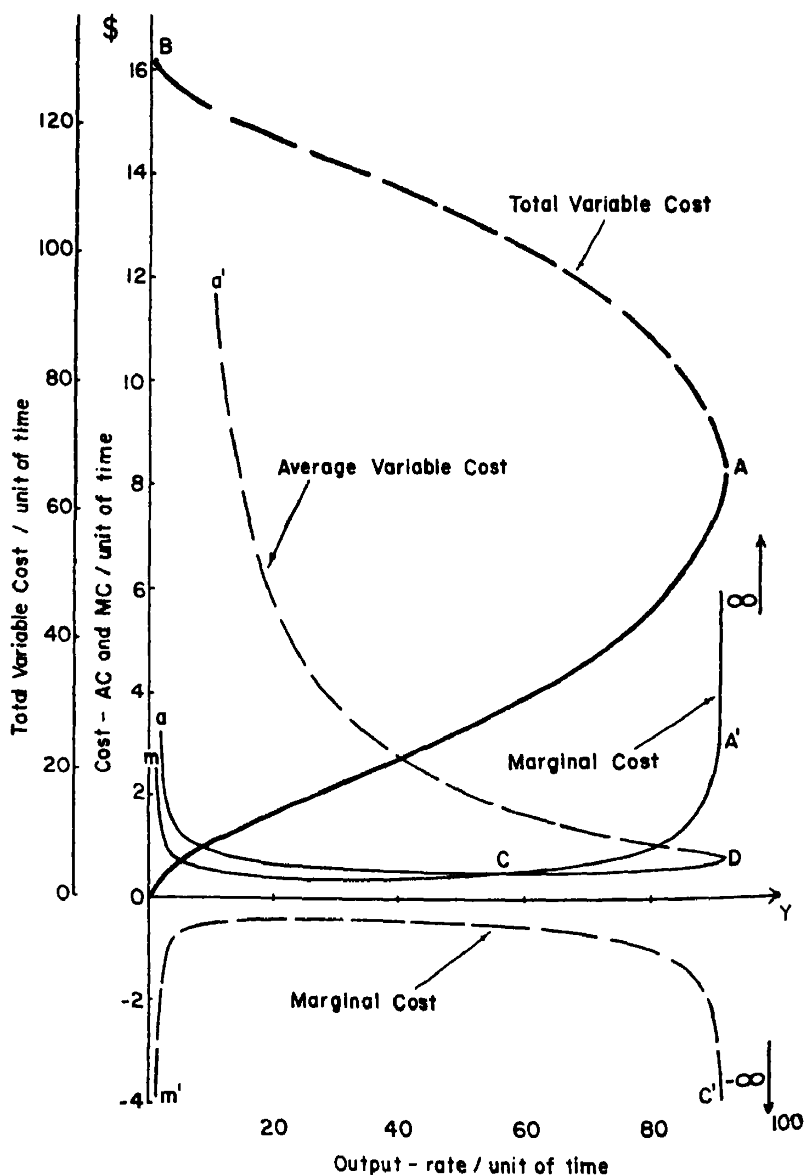


FIGURE 5. COST-PRODUCT RELATIONSHIPS

der some special circumstances, were exactly symmetrical (used here in the mathematical sense) to the TPP curve in *both* Regions I and II, i.e., if the TPP were symmetrical about the input value corresponding to the maximum TPP value. This is illustrated in Figure 4 where the TPP is symmetrical about input (X) value of 13. The MPP in Region III, therefore, is an exact mirror image of the MPP in Regions I and II. The corresponding cost functions are shown in Figure 5. Here the MC function in Region III, corresponding to the MPP behavior, is also an exact mirror image of the MC function in the first two regions. But, this situation, as remarked earlier, is rather unusual. Theoretically, there is more reason to believe that the TPP curve would be nonsymmetrical than symmetrical. As Cassels stated [2, p. 109, n. 10], "since the *proportions* of the factors are less affected by each additional unit of the variable factor as we move to the right along the X-axis it is clear that *in general* the third phase must be more prolonged than the first."

OM P. TANGRI*

* Assistant professor, department of agricultural economics and farm management, University of Manitoba, Canada.

Martin Ulrich, a graduate student in my seminar, started me on this paper by expressing skepticism on the standard treatment of cost-product relationships. My thanks to him and to Surendra Kulshrestha, both of whom assisted me by running the necessary input-output computations (not included in this paper). Thanks also to the referee, my colleagues at the University of Manitoba, and my brother at Wayne State University.

REFERENCES

1. C. E. BISHOP AND W. D. TOUSSAINT, *Agricultural Economic Analysis*. New York 1958.
2. J. M. CASSELS, "On the Law of Variable Proportions," Am. Econ. Assoc., *Readings in the Theory of Income Distribution*, Vol. 3. Philadelphia 1946.
3. E. O. HEADY, *Economics of Agricultural Production and Resource Use*. New York 1952.
4. R. H. LEFTWICH, *The Price System and Resource Allocation*. New York 1958.
5. P. A. SAMUELSON, *Economics, An Introductory Analysis*. New York 1964.
6. M. M. SNODGRASS AND L. T. WALLACE, *Agriculture, Economics and Growth*. New York 1964.
7. G. J. STIGLER, *The Theory of Price*. New York 1947.
8. W. S. VICKREY, *Microstatics*. New York 1964.

Interindustry Wage Change Dispersion and the "Spillover" Hypothesis

Since the advent of unionism as a quantitatively important economic force, the economics literature has been replete with references to the quantitative relevance of equitable or coercive comparisons in the wage determination process. The terminology used to describe this process has varied

considerably—spillover, key bargains, wage leadership, pattern wage adjustment, imitation, and diffusion. The basic notion underlying what may generally be called the "spillover" hypothesis is that wage movements in the *i*th sector not only reflect traditional market forces in that sector but also reflect wage movements in some *j*th sector.[1] The wage relationship between the *i*th and *j*th sectors is "derived" from a political influence theory which suggests that traditional economic variables are to be supplemented, or in some cases supplanted, by additional spillover variables if a complete explanation of wage movements is to be obtained. The theory is sometimes restricted to wage determination under collective bargaining, but it is also applied to cases where monopsonistic employers administer wages and seek simple decision rules.

The interindustry dispersion of wage changes computed over different time periods, for groups of industries alleged to be part of a common spillover domain, is commonly cited as evidence to support this model. From these data it is argued that the dispersion is either absolutely "small," a fact allegedly consistent with the spillover hypothesis, or smaller than the dispersion in some other sector or in some earlier time period in which spillovers were inoperative.[2] The purpose of this paper is to comment on the theoretical and statistical appropriateness of this procedure. The following questions will be considered: (1) Does the spillover hypothesis imply that the interindustry dispersion of wage changes within a spillover domain will be absolutely small in some sense? (2) Does it imply that the dispersion will be smaller than that which would be generated by market forces alone? (3) Is the test statistically appropriate?

I. *Market Wage Determination and Wage Change Dispersion*

The implications of the traditional market hypothesis for interindustry wage change dispersion will first be examined, and then compared with those of a spillover model of wage determination. The market theory of the wage determination process asserts that, for a particular industry, wage changes will reflect variation in underlying labor demand and supply conditions. According to traditional economic theory, the demand by the *i*th industry for any factor of production, including labor, is a function of the prices of all other inputs, technology, and the relevant variables of the product-demand schedule. The supply of labor to the *i*th industry is a function of the wage rate in that industry and the alternative wage rates for labor of comparable skill offered by other industries. Adding the equilibrium condition that labor supplied equals labor demanded, a reduced-form expression relating the equilibrium wage rate and the exogenous de-

[1] The term wages can mean many things—straight time hourly earnings, average hourly earnings, average hourly earnings plus fringe benefits, etc. And wage changes may be computed in percentage, absolute, or, for that matter, any units. Although the term "wage changes" will be interpreted to mean percentage changes in average hourly earnings, the arguments in this paper are independent of this choice.

[2] The formal and informal use of this kind of evidence is found in many studies as well as in some labor economics textbooks. See, for example, [1, pp. 386–92] [4] [5] [7, pp. 193–94] [8.]

mand and supply variables can be obtained. In the neighborhood of equilibrium this expression can be approximated as

$$\dot{W}_i^* = B_0 + B_1\dot{X}_i + B_2\dot{W}_i^A + u_i, \tag{1}$$

where $\dot{W}_i^*$ is the equilibrium percentage change in the wage rate paid by the ith industry; $\dot{X}_i$ is the percentage change in a labor demand shift variable;[3] $\dot{W}_i^A$ is the percentage change in alternative wages for labor of skill comparable to that used in the ith industry; and u_i is a random-error term summarizing omitted factors and random disturbances in the structural equations. This equation gives the percentage change in wages for the ith industry as a function of the percentage change in the relevant demand and supply shift variables.

Given a sample of n industries for which the percentage change in wages is computed over a particular time period, the sample variance of wage changes is defined as

$$S^2_{\dot{W}^*} = \frac{\sum_{i=1}^{n} (\dot{W}_i^* - \overline{\dot{W}})^2}{n-1}, \tag{2}$$

where $\overline{\dot{W}}$ is the mean of the $\dot{W}_i^*$'s.[4]

If the explanatory variables are uncorrelated with the error term, the sample variance of $\dot{W}^*$ is given by the formula,

$$S^2_{\dot{W}^*} = B_1^2 S^2_{\dot{X}} + 2B_1B_2S^2_{\dot{X}\dot{W}^A} + B_2^2 S^2_{\dot{W}^A} + S^2_u,\text{[5]} \tag{3}$$

where $S_{\dot{X}\dot{W}^A}$ is a covariance, while all other S^2 terms are variances.

The sample variance of $\dot{W}^*$ will depend on the variance of the error term (S_u^2) and the sample variance of the explanatory variables. If the sample of industries chosen is such that each industry exhibits similar changes in demand and supply conditions, the only source of variance in the sample will be the variance of the error term, which may be either "large" or "small." On the other hand, if the industries diverge widely in their demand and supply change characteristics, this divergence will be an additional source of variation in the sample $\dot{W}^*$'s. In any event, the theory does not specify that S_u^2 will be small, and there is no basis for forming an a priori expectation concerning the sample variance (2). In short, while the market theory of wages names the relevant explanatory variables and specifies the signs of the structural coefficients, it implies nothing about the variance of interindustry wage changes.

[3] It is assumed that the demand and supply shift variables can be summarized by the variables $\dot{X}_i$ and $\dot{W}_i^A$ but the argument is unaffected if these variables and their coefficients, B_1 and B_2, are interpreted as vectors.

[4] In equation (2) and elsewhere throughout this paper, sample means and variances are computed as unweighted averages rather than as weighted averages. Since this simplification does not affect any results qualitatively, it was adopted to eliminate notational complexity which would only obscure the main points we are making.

[5] See, for example, P. Hoel [3, p. 231].

II. *The Competitive Hypothesis: A Special Case of the Market Hypothesis*

If the change in wage rates is computed over a sufficiently long period of time, the competititve hypothesis asserts that the supply of labor to the *i*th industry will be infinitely elastic.[6] Under these conditions, changes in the demand for labor will affect employment but not wages. Wages will change only in response to changes in the supply price of labor, which will depend on changes in alternative wages available for factors of skill comparable to those employed in the *i*th industry. Mathematically,

$$\dot{W}_i^* = \dot{W}_i^A + \epsilon_i. \tag{4}$$

In the special case in which all industries in the sample use comparable skill mixes during the entire period, the sample variance of $\dot{W}^*$ is simply equal to the sample variance of ϵ, which may be large or small. However, if the sample includes industries which are not all comparable in this respect, the general expression is again given by

$$S_{\dot{W}^*}^2 = S_{\dot{W}^A}^2 + S_\epsilon^2. \tag{5}$$

In this more general case, the sample variance of $S_{\dot{W}^*}^2$ is equal to the sample variance of the random-error term (S_ϵ^2) plus the sample variance of the alternative wage changes ($S_{\dot{W}^A}$). Even under the special assumption of the competitive hypothesis, the sample variance of wage changes may be large or small.

III. *The Spillover Hypothesis and Wage Change Variance*

One version of the spillover hypothesis emphasizes the influence of wage outcomes, in a theoretically specified sector of the economy, on the bargaining outcome in a particular industry, but not to the complete exclusion of industry specific supply and demand factors; institutional relationships among industries supplement rather than supplant a market theory of wage determination.[7] The hypothesis may be expressed in the form of two equations. Let the unobserved market-determined percentage wage change in the *i*th industry be given by

$$\dot{W}_i^* = B_0 + B_1\dot{X}_i + B_2\dot{W}_i^A + u_i. \tag{1}$$

An equation describing the impact of spillovers is added to obtain a full description of the wage determination process. The spillover may be introduced as follows:

$$\dot{W}_i = \dot{W}_i^* + \delta(\dot{W}_i^s - \dot{W}_i^*) + v_i, \tag{6}$$

[6] M. Reder [6, pp. 299-311] gives an excellent summary of the competitive theory in which this interpretation and its empirical implications are developed extensively.

[7] A model with these features has been used recently by O. Eckstein and T. Wilson [2].

where $\dot{W}_i$ is the observed percentage change in wages; $\dot{W}_i^s$ is the percentage wage change in some other sector of the economy which influences the bargaining demands of unions in i as well as the offers of employers in i; v_i is a random-error term summarizing omitted variables which influence the bargining outcome in the ith industry;[8] and δ is a spillover coefficient.

Equation (6) may be rewritten as

$$\dot{W}_i = (1 - \delta)\dot{W}_i^* + \delta \dot{W}_i^s + v_i. \tag{7}$$

This relationship summarizes the influence of both market and institutional forces on the determination of wages in the ith industry. If $\delta = 1$, then industry specific market forces play no role in wage determination and wages are determined only by wage outcomes in the spillover sector and by random factors as summarized by v_i.

Now consider a sample of wage changes for a group of industries, all of which are members of the same spillover domain, which implies that $\dot{W}_i^s$ is the same for all i. If the above model is correct, the sample interindustry variance of wage changes will be given by

$$S_{\dot{W}}^2 = (1 - \delta)^2 S_{\dot{W}^*}^2 + S_v^2.\text{[9]} \tag{8}$$

In an absolute sense, there is no a priori reason for assuming that the variance in (8) will be small, for even if it is maintained that δ is near unity, the error term v may generate considerable interindustry wage change variance. As stated, the theory specifies (1) the relevant spillover domain, which is institutionally defined, and (2) the signs of the coefficients. The additional assumption that σ_v^2 is small or zero is arbitrary, and has no basis in economic or statistical theory.

Furthermore, the theory does not imply that the variance of wage changes will be smaller in the presence of spillovers (8) than in their absence [i.e., in (3)]. From (8) we see that the spillover and market models would imply the same variance in the observed percentage change in wages if

$$(1 - \delta)^2 S_{\dot{W}^*}^2 + S_v^2 = S_{\dot{W}^*}^2,$$

that is, if

$$S_v^2 = [1 - (1 - \delta)^2] S_{\dot{W}^*}^2.$$

[8] The error term v has the same underlying rationale as the error term in (1). Equation (6) is a behavioral relationship which in the unionized case summarizes the collective bargaining process. Underlying this relationship is a set of behavioral relationships describing the employer's and union's bargaining policies. Obviously, the variables included in (6) do not exhaust the many forces which influence both parties. The union's bargaining policy, for example, may be influenced by leader-member relationships, the age of workers (which influences their taste for money wages versus fringes), and a host of other forces.

[9] Because it is assumed that the sample consists of industries in the same spillover domain, the variance term $S_{\dot{W}^s}^2$ is zero. For expository simplicity, we assume that $\dot{W}^*$ and v are uncorrelated.

The market model would imply greater variance of wage changes than the spillover model if

$$S_v^2 < [1 - (1 - \delta)^2]S_{W^*}^2,$$

while the spillover model would yield the greater variance if the inequality were reversed.

Apparently a combined spillover-market mechanism of wage determination does not necessarily imply a lower variance of wage changes than a pure market mechanism, and the comparison of wage change variances for two sectors, one with spillovers and one without them, is an ambiguous test procedure. This proposition holds even if $S_{W^*}^2$ is statistically controlled. However, given identical or randomized values of $S_{W^*}^2$ and S_v^2, wage change dispersion, in the context of a spillover model, varies systematically with δ. Provided that industry groups can be ranked according to the size of δ, meaningful variance comparisons are empirically feasible, at least in principle, although none of the existing literature has demonstrated that the necessary assumptions are satisfied.[10]

The difficulties of the variance comparison test are not easily avoided by other test procedures. The principal weakness of the spillover theory is its inability to specify unambiguously the spillover domain. The counterpart of this difficulty in the market model is the specification of the alternative wage domain. It is not unlikely that these two domains will overlap, creating a serious identification problem. For example, a positive time-series relationship between wage changes in one sector and those in a spillover sector will be ambiguous evidence of a spillover mechanism unless one can specify a spillover variable not easily interpreted as a labor supply variable. This problem is particularly acute if one works with broadly defined sectors.

IV. *Statistical Considerations*

Comparisons of variances of wage changes have been shown to be of questionable value as a means of discriminating between a market model and a spillover model of wage changes, since there is no basis for forming a priori expectations of the values of the residual variances of these models. It will now be shown that even if the absolute or relative values of these variances were known for a given level of aggregation, it would still be impossible to make inferences from a comparison of wage change variances since (1) within each set of industries the variance of wage changes is a decreasing function of the level of aggregation, and (2) it is generally not clear when data for two or more industries represent the same level of aggregation.

The absolute value of interindustry wage change variance will be sensitive to the observation unit chosen. The variance may be computed on an

[10] The expectation that the wage change variance declines as δ rises requires the maintained hypothesis that $\delta \leq 1$; otherwise, from (8) we see that S_W^2 is not a monotonically decreasing function of δ.

establishment basis or on the basis of a collection of establishments corresponding to a particular aggregation procedure. In general, a smaller interindustry variance will be observed if the observation unit is defined in terms of, say, the 2-digit industrial classification than if it were defined by, say, the 3-digit classification. Measures of dispersion vary systematically with the aggregation level because aggregation averages out, so to speak, random errors.[11] In terms of the interindustry variance of wage changes, this relationship between aggregation and dispersion is verified by the available data, which indicate that the aggregation effect is quantitatively important.

Percentage changes in average hourly earnings over the period 1958–63 were computed for three industry groups—durable manufacturing, nondurable manufacturing, and nonmanufacturing, which includes trade, transportation, services, etc. The interindustry variance is computed for each industry group at the 2-, 3-, and 4-digit level whenever sufficient data are available.[12] The standard deviation for each of the three sectors at each of the three levels of aggregation is shown in Table 1. Also shown in the table are the arithmetic mean of wage changes, the coefficient of variation (the standard deviation divided by the mean times 100), and the range.

The statistics in Table 1 highlight the main argument of this section. For each of the three sectors—durables, nondurables, and nonmanufacturing—both the standard deviation and the coefficient of variation, a measure of relative dispersion, decline in magnitude as the level of aggregation increases. In the durable-goods sector, for example, the standard deviation is about 50 per cent larger when computed on the basis of a 4-digit industry classification than when computed at the 2-digit level. The absolute size of the variance is a statistical artifact and the notion of an absolutely small dispersion is either meaningless or not very helpful.

The data in Table 1 also reveal an inherent difficulty in testing the spillover hypothesis by comparing interindustry variance between two sectors, one in which spillovers are alleged to operate and one in which they do not. It is suggested frequently that the extensively unionized durable-goods sector is a spillover domain. Assuming that spillovers reduce variance, which coefficients of variation (or other measures of dispersion) are to be compared? It is not at all obvious that wage dispersion at the 2-digit level in the durable sector should be compared to its counterpart in the nondurable or nonmanufacturing sector. It may be no less appropriate to compare 2-digit durables with, say, 3- or 4-digit nondurables than with 2-digit nondurables. The Standard Industrial Classification at the 2-digit level, or at

[11] For example, with some modest manipulation we can obtain from the well-known formula for partitioning a sum of squared deviations the relationship, $S^2_{3,g} = S^2_{3.2} + S^2_{2,g}$, where $S^2_{3,g}$ is the variance of 3-digit wage changes computed around the global mean of wage changes; $S^2_{3.2}$ is the variance of 3-digit wage changes computed around the respective 2-digit means; and $S^2_{2,g}$ is the 2-digit variance computed around the global mean. In general, $S^2_{3.2} \neq 0$ and, consequently, $S^2_{3,g} > S^2_{2,g}$.

[12] Because A. M. Ross [7] and others assert that the durable sector is a single spillover domain, this sector has been isolated in computing the variances. The other two groupings were arbitrarily chosen.

TABLE 1—MEASURES OF ABSOLUTE AND RELATIVE DISPERSION FOR PERCENTAGE CHANGES OF AVERAGE HOURLY EARNINGS FOR 2-, 3-, AND 4-DIGIT SIC CLASSIFICATIONS, 1958–63*

Industry Group	Level of Aggregation		
	4-Digit	3-Digit	2-Digit
Manufacturing			
Durables:			
S (Standard deviation)	3.06	2.62	2.04
$\bar{X}$ (Arithmetic mean)	16.4	16.0	15.7
$(S/\bar{X})\times 100$ (Coefficient of variation)	18.7%	16.5%	13.0%
Range	9.1–23.0	11.8–22.5	12.4–19.9
N (Number of observations)	79	66	11
Nondurables:			
S	4.45	3.84	2.99
$\bar{X}$	16.7	16.2	16.1
$(S/\bar{X})\times 100$	26.7%	23.7%	18.5%
Range	5.3–25.3	6.4–22.1	12.3–20.8
N	41	53	10
Nonmanufacturing			
S	N.C.	4.44	3.06
$\bar{X}$	N.C.	18.6	18.4
$(S/\bar{X})\times 100$	N.C.	23.9%	16.6%
Range	N.C.	7.0–28.4	13.1–22.4
N	N.C.	28	11

* Based on 1957 Standard Industrial Classification.

N.C. = Not Computed, too few observations.

Source: Computed from U.S. Bureau of Labor Statistics, *Employment and Earnings Statistics for the United States, 1909–64*, Bulletin 1312-2, Washington 1964. In computing statistics by industry and level of aggregation, all possible observations from this source were utilized. See page v for list of industries from this source.

any level, does not necessarily imply the same level of aggregation for all industries.[13] Unless comparability is specified, any conclusion may be reached by making the appropriate arbitrary comparison.[14]

V. *Conclusions*

Three major conclusions follow from the analysis in this paper: (1) the absolute dispersion of wage changes is a statistical artifact; (2) in comparing two different sets of industries, the level of aggregation affects the amount of measured dispersion, yet comparability is difficult to obtain or even

[13] For example, does the 4-digit industry "Farm Machinery and Equipment" (SIC-3522) represent the same level of aggregation as the 4-digit industry "Motor Vehicles and Parts" (SIC-3717), or the 3-digit industry "Motor Vehicles and Motor Vehicle Equipment" (SIC-371), or the 2-digit industry "Transportation Equipment" (SIC-37)?

[14] This problem arises even when the dispersion for a group of 2-digit industries is examined at different points in time to determine whether the advent of unionism has reduced inter-industry wage change dispersion. This technique has been employed by J. E. Maher [5] who finds that coefficients of variation in manufacturing have risen rather than declined since the growth of unionism in the 1930's. While this would appear to contradict the spillover hypothesis, the aggregation issue makes it difficult to interpret this evidence.

specify; (3) for the same set of industries, spillovers need not imply a smaller dispersion than market forces.

TIMOTHY W. MCGUIRE AND LEONARD A. RAPPING*

* The authors are, respectively, research associate and assistant professor of economics at Carnegie Institute of Technology. Leonard Rapping's research time was made available by a Ford Faculty Research Grant. Full responsibility for the contents of this paper rests with the authors.

REFERENCES

1. G. F. BLOOM AND H. R. NORTHRUP, *Economics of Labor Relations*, 4th ed. Homewood 1961.
2. O. ECKSTEIN AND T. WILSON, "The Determination of Money Wages in American Industry," *Quart. Jour. Econ.*, Aug. 1962, *76*, 379–414.
3. P. HOEL, *Introduction to Mathematical Statistics*. New York 1962.
4. J. E. MAHER, "The Wage Pattern in the United States," *Indus. Lab. Rel. Rev.*, Oct. 1961, *15*, 3–20.
5. ———, untitled communication, *Indus. Lab. Rel. Rev.*, Oct. 1961, *16*, 125–33.
6. M. REDER, "The Theory and Measurement of Wages," in *Aspects of Labor Economics—A Conference of the Universities-National Bureau Committee for Economic Research*, Princeton 1962.
7. A. M. ROSS, "The External Wage Structure," in G. W. Taylor and F. C. Pierson, eds., *New Concepts in Wage Determination*, New York 1957.
8. B. SOFFER, "On Union Rivalries and Minimum Differentiation of Wage Patterns," *Rev. Econ. Stat.*, Feb. 1959, *41*, 53–60.

Returns to Scale and the Spacing of Isoquants

In the usual statement of production theory, average returns to scale are specified in terms of scale elasticity: $E=(dQ/Q)/(dk/k)$, where $Q=q(k\overline{X}, k\overline{Y})$, $k>0$, and $\overline{X}$ and $\overline{Y}$ are initial values of X and Y which fix factor proportions when k, the scale coefficient, equals 1.[1] Further, in the usual statement of production theory, the scale elasticity is related to the spacing of isoquants as follows: When $E>1$, the successively numbered isoquants are getting ever closer (which means that the production surface, viewed from the origin, is getting steeper znd steeper; i.e., $q_k''>0$). When $E<1$, the isoquants are getting ever farther apart ($q_k''<0$); and when $E=1$, it is asserted that the isoquant spacing does not change ($q_k''=0$). It is the pur-

[1] The only exception that we have been able to find in a check of the literature appears in Schneider [1, p. 149]. Professor Schneider defines constant, increasing, and decreasing returns to scale as follows:

$dx/d\lambda$ is constant
$dx/d\lambda$ increases as λ increases
$dx/d\lambda$ diminishes as λ increases

where x represents output and λ represents the scale coefficient (our k).

pose of this note to show (1) that isoquant spacing depends on marginal, not average, returns to scale; and (2) that the above usually-stated relationships between average returns to scale and spacing of isoquants are accurate only for homogeneous production functions.

Scale elasticity is the ratio of marginal returns to scale to average returns to scale: $E=q_k'/(Q/k)$. In the case of a linear homogeneous production function, q_k' and Q/k are both constant and equal and $E=1$. And since q_k' is a constant, the isoquants are equally spaced. For homogeneous functions, where there are constantly increasing average returns to scale, marginal returns to scale must always be greater than average returns to scale and also constantly increasing, E is a constant greater than 1, and the isoquants are spaced closer and closer together. For homogeneous functions where average returns to scale are constantly decreasing, $E<1$ and the isoquants are spaced farther and farther apart.

Figures 1a and 1b show that these relationships do not necessarily hold for nonhomogeneous functions. These figures show increasing average returns to scale for values of k up to k_2 and decreasing average returns beyond k_2. (This is the usual assumption of economic theory.) Marginal returns are increasing up to the inflection point, k_1, and decreasing thereafter. Thus, while average returns to scale are increasing up to k_2, marginal returns to scale first increase, reach a maximum, and then decrease, but they are always greater than average returns. In terms of isoquants, this would mean that up to k_1 the isoquants are getting ever closer and beyond k_1 ever farther apart. But since the marginal returns are greater than the average returns, $E>1$ up to k_2. These diagrams have thus shown that the usually posited relationships between average returns to scale (and E) and spacing of isoquants are violated in the case of a nonhomogeneous production function.

We now show the *general* relationship between scale elasticity and the spacing of isoquants along a ray from the origin. We begin with the definition of scale elasticity:

$$E = \frac{q_k'}{\frac{Q}{k}}. \tag{1}$$

Differentiating with respect to k:

$$\frac{dE}{dk} = \frac{d}{dk}\left[q_k' \frac{k}{Q}\right] = q_k'' \frac{k}{Q} + \frac{1}{Q} q_k' - \frac{k}{Q^2} [q_k']^2.$$

Substituting (1) in the above, we have:

$$\frac{dE}{dk} = q_k'' \frac{k}{Q} + \frac{1}{Q} q_k' (1 - E). \tag{2}$$

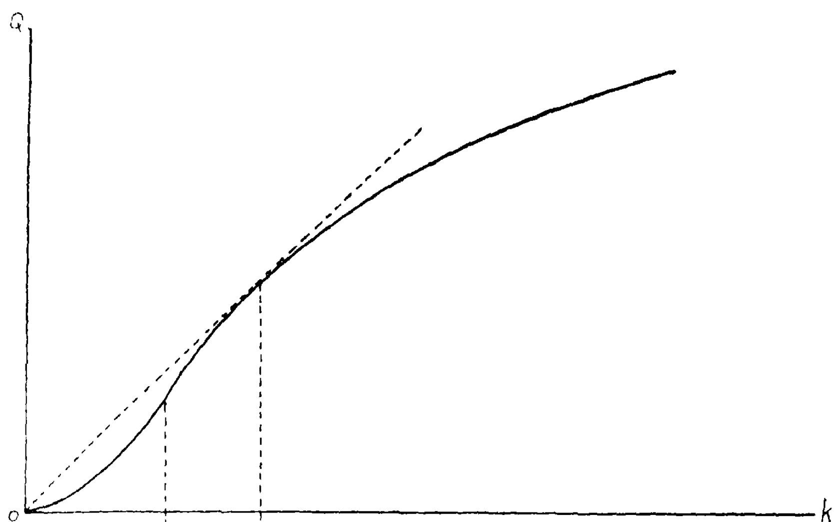


Figure 1a

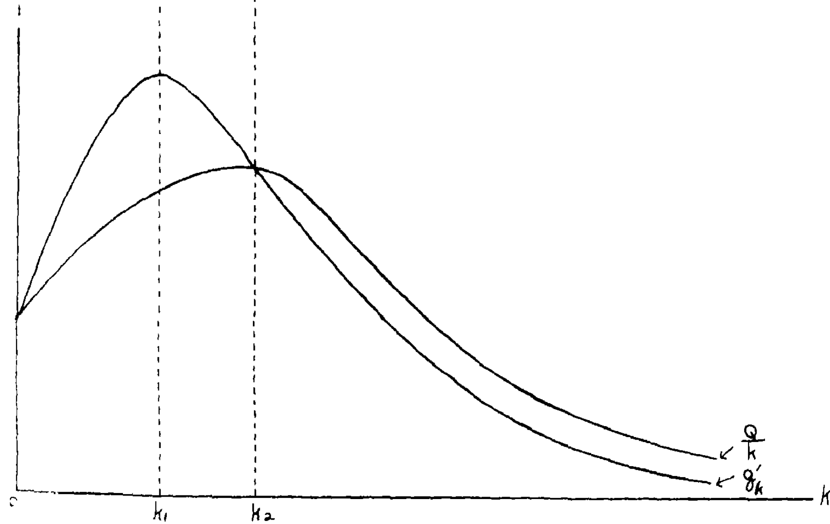


Figure 1b

For homogeneous functions, E is a constant, and therefore, $dE/dk=0$.[2] In addition we assume $k>0$[3] and $q_k'>0$. It follows from (2) that:

$$q_k'' \frac{k}{Q} = -\frac{1}{Q} q_k' (1-E)$$

and

$$\frac{q_k''}{q_k'} = \frac{E-1}{k}. \tag{3}$$

Thus, when $E>1$, $q_k''>0$ (isoquants are ever closer)
when $E=1$, $q_k''=0$ (isoquants are equally spaced)
when $E<1$, $q_k''<0$ (isoquants are ever farther apart).

The spacing of isoquants is not discussed at length in the literature; where it is discussed, it is implicitly or explicitly in terms of homogeneous production functions. For nonhomogeneous functions, we investigate first the case where scale elasticity is positive and increasing, i.e., $dE/dk>0$. Again we assume $k>0$ and $q_k'>0$. From (2):

$$\frac{dE}{dk} = q_k'' \frac{k}{Q} + \frac{1}{Q} q_k' (1-E) > 0$$

and

$$\frac{q_k''}{q_k'} > \frac{E-1}{k}. \tag{4}$$

If $E>1$, $q_k''>0$
$E=1$, $q_k''>0$
$E<1$, $q_k''\gtreqless 0$.

[2] $q(k\overline{X}, k\overline{Y}) = k^i q(\overline{X}, \overline{Y})$

$$q_k' = ik^{i-1}q(\overline{X}, \overline{Y})$$

$$\frac{Q}{k} = \frac{k^i q(\overline{X}, \overline{Y})}{k} = k^{i-1}q(\overline{X}, \overline{Y})$$

$$E = \frac{q_k'}{\frac{Q}{k}} = \frac{ik^{i-1}q(\overline{X}, \overline{Y})}{k^{i-1}q(\overline{X}, \overline{Y})} = i \text{ (a constant)}$$

$$\frac{dE}{dk} = \frac{d}{dk}(i) = 0.$$

[3] In Figures 1a and 1b we show $k=0$ at the origin. This is the case where no output is being produced. For algebraic exposition, it is convenient to neglect the trivial case where no output is being produced; i.e., $k=0$.

Of more importance for economic analysis is $dE/dk<0$, i.e., a stage of increasing returns to scale followed by constant and/or decreasing returns to scale. Our assumptions are now $dE/dk<0$, $k>0$, and $q_k'>0$ Then (2) gives:

$$\frac{dE}{dk} = q_k'' \frac{k}{Q} + \frac{1}{Q} q_k'(1 - E) < 0$$

$$\frac{q_k''}{q_k'} < \frac{E-1}{k}. \tag{5}$$

If $E>1$, $q_k''\gtrless 0$
$E=1$, $q_k''<0$
$E<1$, $q_k''<0$.

Referring to Figure 1b, we see that, with the usual assumption of an initial stage of increasing returns to scale followed by a stage of constant average returns to scale and then a stage of decreasing average returns to scale, $E>1$ in the first range, $E=1$ in the second range, and $E<1$ in the third range. We have shown, however, that marginal returns to scale may be increasing, constant, and decreasing when average returns are increasing; but marginal returns must be decreasing at the point of diminishing average returns to scale and thereafter. Thus, the most general production function diagram would show isoquants along a ray from the origin initially ever closer, then equally spaced, and finally ever farther apart within the range of increasing average returns (up to k_2 in Figure 1b) and continuingly ever farther apart in the stage of decreasing average returns.

ALBERT M. LEVENSON
BABETTE SOLON*

* The authors are assistant professors of economics at Queens College of the City University of New York. They are indebted to Mr. Joel Stone, a student at Queens College, who noted ambiguities in the usual discussions of the spacing of isoquants.

REFERENCE

1. E. SCHNEIDER, *Pricing and Equilibrium*. New York 1962.

Lasting Effects of Economics Courses at Different Types of Institutions

We recently reported the results of a nationwide study of the economic understanding of a large sample of all high school social studies teachers in the United States [1]. These results indicated, somewhat alarmingly, that teachers who had taken one or two semesters of elementary economics in college did not do significantly better on a widely used "Test of Economic Understand-

ing" than did those who had taken no college economics, and that not until the teachers had taken five or more college economics courses did they perform significantly better on the test that those who had had no economics.

Different hypotheses have been advanced to explain this result. One is that high school social studies teachers retain little from their college economics courses because they have taken relatively weak courses at relatively weak colleges. This note presents evidence on how much lasting learning in economics occurred at institutions of five different types, evidence which suggests that the lasting results were greatest at small liberal arts colleges and least at large universities, with intermediate results at other types of institutions. The results also provide separate data on students in "education" majors and in other undergraduate fields.

The results reported here are based on the same nationwide survey reported in the earlier article, which included information on where each teacher took his college economics; they are thus subject to the same reservations as those reported in the earlier article. We present them because we believe they may be of interest to the profession, not because we belive they represent a complete measure of the achievements of economics courses at different institutions, for they are obviously only a very partial measure.

I

To investigate the possibility that the discouragingly low retention of economics results primarily from students having taken their courses at relatively weak institutions or in relatively weak courses for particular groups of students, we first classified all colleges and universities represented in the sample into five groups: (1) 20 "high prestige" schools, including both universities and liberal arts colleges;[1] (2) 50 large universities, excluding those included in group (1); (3) 50 well-known liberal arts colleges, excluding those in group (1) above; (4) "teachers colleges," whether or not they still use that title in their official names; and (5) all other schools. Since the basic survey focused on teachers, it was reasonable to suppose that a large number of them would have been "education" majors, and other studies have indicated that such majors are relatively weaker students than other majors. We therefore classified separately education majors and noneducation majors in each group of schools.

Table 1 summarizes the raw test performances of students from the five different types of schools, without holding constant differences in student ability, number of courses taken, and the like; a regression analysis aimed at isolating these variables within the different groups of schools is reported in Table 2. The first three columns of Table 1 report the basic scores for students with

[1] The group includes six leading private universities, two state universities, and two institutes of technology from the larger schools; and three coeducational, three men's, and four women's smaller liberal arts colleges. The selection was based on general academic standing, and on average CEEB scores for entering students.

A separate analysis was also made dividing the ten liberal arts colleges from the ten universities within this group, but it seemed desirable to investigate separately the results at the entire group of such "prestige" schools with especially high prestige faculty members and students of especially high academic ability.

no economics and those taking some college economics in each class of school, and the difference between the no-economics and with-economics scores for each class. In each type of school there was some improvement from taking economics courses, but the degree of improvement ranged from virtually none for the large state universities to nearly six points on a 50-point scale for the 50 leading liberal arts colleges, as shown in column 3. If we assume (not implausibly) that *within* each of the five groups of schools students who do and do not take economics are otherwise similar (except for the education-noneducation breakdown shown), then the improvements shown in column 3 should provide some indication of the lasting effect produced on students at the various schools. It will be recalled from the earlier article that on the

TABLE 1—TEST SCORES OF STUDENTS WITH AND WITHOUT COLLEGE ECONOMICS[a]

Type of School	All Students			Noneducation Majors			Education Majors		
	No Econ.	With Econ.	Difference	No Econ.	With Econ.	Difference	No Econ.	With Econ.	Difference
20 "Prestige" Schools	37.7	39.3	+1.6	37.5	40.3	+2.8*	38.3	36.3	−2.0
50 Large Universities	35.0	35.8	+0.8	35.8	37.7	+1.9	34.2	34.3	+0.1
50 Liberal Arts Colleges	31.6	37.5	+5.9*	33.1	38.2	+5.1*	29.2	35.6	+6.4*
"Teachers Colleges"	30.8	33.8	+3.0*	—	—	—	30.8	33.8	+3.0*
Other Schools	30.7	34.5	+3.8*	31.9	35.7	+3.8*	29.9	33.2	+3.3*
All Schools	31.6	34.7	+3.1*	33.2	36.6	+3.4*	30.8	33.7	+2.9*

[a] For information on test used and on schools included in various categories, see text. Number of students in each category shown in footnote to Table 2.

* Statistically significant at the .05 level. Negative figure for education majors at "prestige" schools rests on only six students in the no-economics category; most of the "high prestige" schools do not offer, or emphasize, undergraduate majors in "education."

average the teachers in the survey had been out of college about eight years.

The next six columns of Table 1 show the same data separately for noneducation and education majors. While noneducation majors show a consistently higher *level* of achievemment, the general pattern of *improvement* appears to be similar for education and noneducation majors.[2]

The basic data in Table 1 may, however, conceal a variety of factors which help to explain the results. For example, on the average, students in some types of institutions may have taken more courses in college economics than those at others; particularly high-ability students may have chosen economics in some colleges, and relatively low-ability students in others; and the like. We therefore ran a multiple regression equation separately for each of the five classes of schools, similar to the basic regression reported in the earlier article. We know from the evidence that student abilities vary substantially among the five classes of schools, but by comparing economics versus no-economics students *within* each class of schools we should gain some indication of how

[2] Except for the insignificant group noted in the footnote to Table 1.

much the economics courses contribute to after-college test performances holding constant the other factors noted in the table.[3]

The results are shown in Table 2. Under the heading, "marginal subgroup coefficients," X_1 shows the separate marginal effect of one or two economics courses; X_2 shows the marginal effect of adding one or two additional courses; X_3 shows the marginal effect of adding one or more additional courses to the four already taken. These three figures must be summed to get the full effect of the college economics courses taken in explaining the score on the "Test of Economic Understanding."[4]

Table 2 suggests that, when we hold constant the effects of college class standing (a proxy for ability plus motivation), recency of economics courses, sex, college major, and the like, there remain substantial differences in the contribution made to ex-students' test scores by the five classes of institutions. These vary from a net improvement from having taken economics of 1.95 in 50 large universities to 8.71 in 50 well-known liberal arts colleges (the summation of X_1, X_2, and X_3 in each case). It is interesting to note that the net contribution of one or two semesters of economics to later understanding appears to have been small except at the liberal arts colleges, where a substantial marginal improvement came at that level. At the "high prestige" institutions, the biggest improvement came at the three-four course level; while at teachers colleges and other schools the biggest advance occurred only when students took five or more courses. As has been indicated by earlier studies, on the average, student ability tends to be lower among "education" majors than among most other groups of students; this shows up clearly in both Table 1 and in the variable X_{10} coefficients of Table 2.[5]

II

Different hypotheses may be suggested to explain the differences reported in Tables 1 and 2. It is possible, as was suggested in the earlier article, that the test is a poor measure of the amount of economic understanding retained some years after completion of college, or that other unmeasured variables so dominate the amount of understanding after college as to make these results misleading. The counterevidence was presented in the earlier article. We also sug-

[3] Although the comparison in Table 2 is only between students within each group of institutions, we did run a separate regression for all students at all schools to measure the relative importance of being a student at each different class of school. This, of course, reflects in some considerable part the differences in student abilities at the different institutions, but reflects other factors as well. Using the data for all schools with a separate variable added for class of school, the coefficient for "prestige schools" is 2.78; for 50 large universities, 2.24; for liberal arts colleges, 1.51; for "teachers colleges," —1.41; and for other schools, —1.36. All results are significantly different from zero at the .01 level.

[4] For a more complete explanation, see [1, p. 347].

[5] Comments on, and possible explanations of, some of the coefficients are included in the earlier article. See also [1, p. 347] for comments on the relatively low R^2's for all the regressions.

Our tests show no significant multicollinearity among the explanatory variables used in the regressions.

TABLE 2—MULTIPLE REGRESSION ANALYSIS OF RELATIVE INFLUENCE OF SELECTED VARIABLES ON TEST SCORES, BY TYPE OF SCHOOL[a]

Explanatory Variable	20 Prestige Schools[b]		50 Large Universities[c]		50 Liberal Arts[d]		Teachers Colleges[e]		Other Schools[f]	
	Marginal Subgroup Coefficients	Coefficients	Marginal Subgroup Coefficients	Coefficients	Marginal Subgroup Coefficients	Coefficients	Marginal Subgroup Coefficients	Coefficients	Marginal Subgroup Coefficients	Coefficients
College economics courses[g]										
X_1=one or more courses	1.14		−.52		4.01*		.12		.57	
X_2=three or more courses	5.56*		.89		1.55		2.78*		.67	
X_3=five or more courses	−.34	6.36	1.58	1.95	3.15	8.71	2.80*	5.70	1.90*	3.14
Took more economics after bachelor's degree										
X_4=took more economics		−2.12		−.01		−.73		1.22*		1.53*
Have taken economics course in last few years										
X_5=yes		2.16		−1.70*		.13		1.00*		.63
College class standing										
X_6=in top 10 per cent of class		.45		4.38*		−.47		4.16*		4.05*
Watched "The American Economy"										
X_7=three or more times a week		2.51		7.66*				5.07*		1.82
Teach high school economics or P.O.D.										
X_8=teach such a course		2.04		.85		2.73*		4.94*		.65
Sex										
X_9=male		2.64*		2.87*		−3.24*		2.26*		4.49*
College major										
X_{10}="education" major		−4.67*		−2.46*		−3.20*				−2.52*
R_2		.250		.159		.169		.193		.142

[a] The procedures used in the study and in running the regression were described in [1, pp. 347–48]. Some variables which were insignificant in the earlier study are omitted from this table.
[b] n=103, of whom 34 were majors in education.
[c] n=503, of whom 318 were majors in education.
[d] n=138, of whom 40 were majors in education. One student omitted because only one took TAE and his inclusion, with weights, would produce misleading results on X_7 and X_8.
[e] n=1251, all majors in education.
[f] n=1945, of whom 997 were majors in education.
[g] For an explanation of subgroups, see accompanying text.
* = Significant at .05 level.

gested there (based on the evidence and the psychological literature on learning) that student motivation to learn and to continue using the economics learned in after-college years is essential to obtaining lasting effects from college economics courses.

One further hypothesis, with which the data reported here are consistent, is that the lasting impact of undergraduate college economics courses depends heavily on the emphasis that faculty members put on such teaching, and that this factor is more important than the "quality" of the faculty as measured by the usual criteria of research papers and the like. The smallest lasting improvement from taking undergraduate economics courses appears to come at well-known large universities, where a considerable part of elementary teaching is done by graduate assistants and where many leading professors concentrate on graduate teaching and research. The biggest improvement comes at the 50 well-known liberal arts colleges, where undergraduate teaching is presumably the primary focus of faculty attention, even though the faculty members may not be as "distinguished" professionally as those at some of the 20 "prestige" schools or at some of the other well-known large universities. Interestingly, the next largest contribution appears to come about equally at the 20 "prestige" schools (of which half are small liberal arts colleges) and at the "teachers colleges," where again undergraduate teaching is the primary focus of faculty members and where relatively little teaching is done by graduate students, but where presumably economics faculties are on the average less prestigious in terms of professional standing than those at the first three groups of schools. A breakdown between the liberal arts colleges and the large universities in the 20 "prestige" institutions is consistent with this hypothesis. Although the numbers in the sample so divided are too small to justify placing much weight on the comparison, the level of achievement is slightly higher for the large universities, but the superiority of economics over no-economics students is somewhat larger for the liberal arts college students.

While the results in the study as a whole are mixed, we interpret them as inconsistent with the alternative hypothesis that the students who started from the lowest level of understanding would advance the most through taking economics courses because it is easier to raise such students' understanding from low beginning levels. The results are also clearly inconsistent with the counter hypothesis that the brightest students will be improved the most, even though they start from the highest average level of understanding.

Obviously, the results presented here and in our earlier article are far from definitive on the issues raised. But tentative as they must be, we hope they will be sufficiently provocative to focus attention on the need to improve our undergraduate economics teaching, and equally to stimulate further attempts at careful measurement of teaching results to supplement the impressionistic (and all-too-often wishful) evaluations on which we now rest most of our educational process.

G. L. Bach and Phillip Saunders*

* The authors are, respectively, professor and assistant professor of economics at Carnegie Institute of Technology.

REFERENCES

1. G. L. BACH AND PHILLIP SAUNDERS, "Economic Education: Aspirations and Achievements," *Am. Econ. Rev.*, June 1965, *55*, 329-56.

Optimization and Suboptimization in Fishery Regulation: Comment

In the March 1964 issue of this journal, Ralph Turvey states that "if the *optimum optimorum* is to be reached, . . . then (fishery) regulation must extend not only to the scale but also to the mode of operation" [1, p. 64]. He recommends both a toll or charge per pound of fish caught (scale regulation) and the imposition of a legal minimum mesh size (mode regulation) in order to achieve a Pareto-optimum.

If certain implicit assumptions in his argument are made explicit, however, it becomes clear that the necessity for either or both of these regulations is hardly an immutable law of nature. The chief fault in Turvey's otherwise excellent paper is that he does not clearly state the relation between these critical assumptions and his conclusions for public policy. It should be useful to examine in more detail exactly which conditions must be present for one or the other of these regulations to in fact be necessary for a Pareto-optimum.

It is well known that if institutions are such that technological externalities are present, competitive equilibrium will not be Pareto-optimal. Turvey states that two kinds of external diseconomies are present in the kind of competitively organized, single-trawl fishery which he considers. In the first place, a fish caught by fisherman A reduces the stock of fish available to other fishermen, increasing the amount of resources which these others must use to land a given catch. Secondly, a fish caught today by fisherman A will not be around tomorrow as a larger, more valuable fish, again imposing additional costs upon the industry as a whole.

Let the set of all fish in the banks be divided into two subsets: fish that have reached catchable size (called the "fish stock") and fish which have not yet reached that size. The size of the smallest fish in the fish stock is determined by the size of the mesh in nets used by the fishermen. Turvey states [1, p. 65] that operational models used by biologists to assess fisheries commonly make the following assumptions: (a) growth of fish and natural mortality are functions of age, (b) fishing mortality rate (catch) is proportional to "effort," and (c) recruitment into the fish stock, i.e., the number of fish reaching catchable size, is exogenous.

If assumption (c) is taken literally, then "weight recruitment," or *pounds* of fish reaching catchable size per time period, would depend only on mesh size, since larger mesh would lead to recruitment at a heavier weight. Specifically, the number of fish in the fish stock or the size of the catch would not affect recruitment. It seems reasonable to assume that the number of fish eggs laid per year is proportional to the number of fish in the banks, and that the number of fish hatching is proportional to the

number of eggs laid. If recruitment is to be exogenous, then, mortality in the set of fish which have not reached catchable size must be inversely proportional to the number of fish of egg-laying age, which contradicts assumption (a). It would seem somewhat less fishy to assume that mortality is an increasing function of the number and size of fish competing for food.

The simplest recruitment assumption consistent with (a) and (b) seems to be that natural mortality is a function only of the age of the fish, and that the number of eggs laid per year is determined exogenously. (We will examine the consequences of relaxing this assumption below.) Let the growth

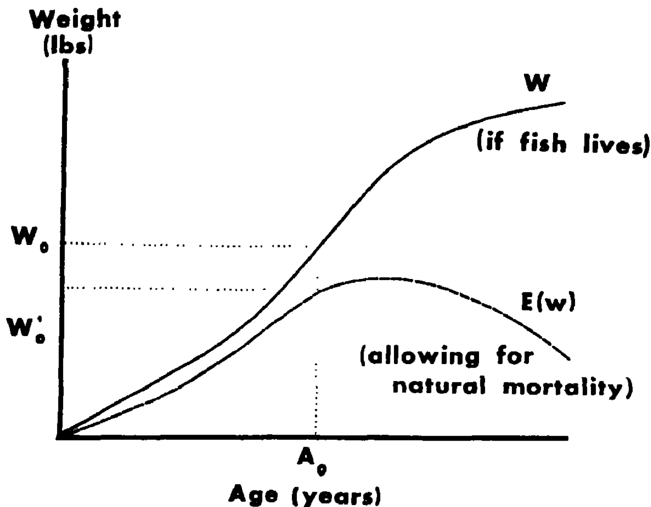


FIGURE 1. GROWTH OF A TYPICAL FISH

of a typical fish be depcited in Figure 1. The solid upper curve shows the growth of the fish as a function of its age, provided the fish lives. The probability that a given fish, once hatched, will have died can be expected to increase as the time elapsed since his hatching grows longer. The bottom dashed curve gives his expected weight (in the statistical sense) when natural mortality has been allowed for.

The size in pounds of the fish as it reaches catchable size is proportional to the cube root of mesh size: thus, the larger the mesh, the older the youngest fish in the fish stock. Given a mesh size of m_0 inches, a fish will be recruited into the fish stock at the corresponding weight w_0, at age a_0. The total "weight recruitment" into the fish stock is given by:

$$r = n \cdot w_0' \tag{1}$$

where r=recruitment into the fish stock (lbs./yr.), n=number of eggs hatching per year, and w_0' =expected weight of typical fish at age of re-

cruitment (lbs.). Given our assumptions, w_0' is a function only of mesh size, so that equation (1) becomes:

$$(1') \qquad r = r(m) \qquad \text{(recruitment function)}$$
$$m = \text{mesh size} \qquad \text{(inches)}.$$

For the individual fisherman, catch (and hence revenue) is a function of mesh size, size of fish stock, and of the resources he expends. Turvey defines an aggregated input called "fishing effort," defined in such a fashion that the fisherman's catch, measured in pounds, is proportional to effort. Effort, then, appears as the argument in the fisherman's cost function. His production function exhibits constant returns to scale if doubling of all inputs results in a doubling of effort as defined. If, in addition, all inputs are available to the industry in perfectly elastic supplies, average cost, *ceteris paribus* (fish stock and mesh size constant), per pound of fish will be constant. If, on the other hand, factor supply curves are not all perfectly elastic, then average cost (again, *ceteris paribus*) will be a rising function of industry output. For the industry as a whole, then, we have:

$$(2) \qquad f = g(F, E, m) \qquad \text{(industry production function)}$$

$$(3) \qquad C = C(E) \qquad \text{(total industry cost)}$$

where f = total fish catch (lbs. yr.), F = size of fish stock (lbs.), C = total cost of catching fish ($), and E = fishing effort (units).

Biological steady-state equilibrium of the fish stock, given the biological envrionment, depends both on the quantity of fish entering the fish stock (recruitment) and on the quantity of fish removed from the fish stock (catch) each year:

$$(4) \qquad F = B(f, r) \qquad \text{(biological equilibrium)}.$$

Substituting (4) into (2), we have:

$$f = g(B(f, r(m)), E, m)$$
$$(5) \qquad f = h(E, m) \qquad \text{(sustained-yield production)}.$$

Equation (5) represents sustained-yield production as a function of mesh size and fishing effort. In other words, it represents a steady-state equilibrium level of catch compatible both with fishing technology and with the biological productivity of the fishing banks.

Turvey assumes that the price of fish depends only on the weight of the industry catch, subject to the condition that all fish be at least of a given "minimum marketable size" [1, p. 67]. Thus, rounding out our model, we have:

$$(6) \qquad P = P(f) \qquad \text{(industry demand)}$$
$$P = \text{price of fish (\$/lb.)}.$$

Variations in mesh size can be assumed to be costless. Hence, for any given level of fishing effort, Pareto-optimality requires that mesh size be such as to maximize sustained-yield production. Given our assumptions, however, a fisherman, left to his own devices, will choose a mesh size such that the smallest fish caught is also the minimum-marketable-size fish [1, p. 68]. In Figure 2a, this minimum marketable size is w_{min}, and the corresponding recruitment is as indicated. Legislating a larger mesh size in this case would increase, *ceteris paribus*, costs for the individual fisherman. Since recruitment would be reduced (shown by the downward-sloping $E(w)$ curve), size of the fish stock would actually be reduced, further increasing costs.

In Figure 2b, increasing mesh size would increase recruitment, and thus the fisherman's parameter F. Only if the decreased costs associated with

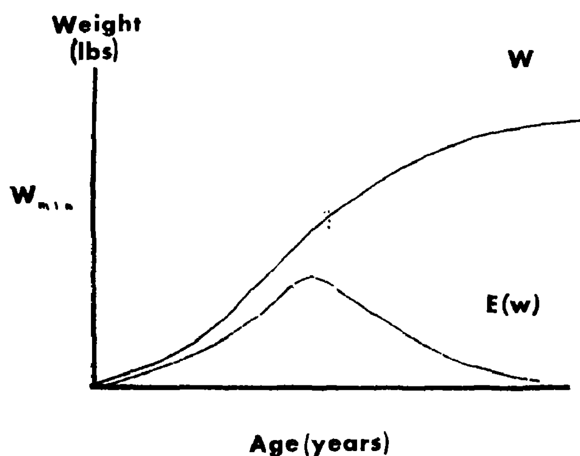


FIGURE 2a. GROWTH OF TYPICAL FISH: NO MESH-SIZE REGULATION NEEDED

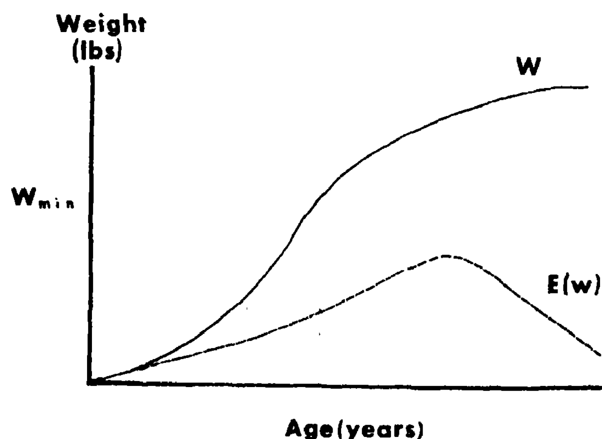


FIGURE 2b. GROWTH OF TYPICAL FISH: MESH-SIZE REGULATION POSSIBLY NEEDED

the increased steady-state fish stock at least offset the increased production costs due to the larger mesh size, however, will a larger mesh size be Pareto-superior. If, and only if, this is indeed the case will external diseconomies of the second type mentioned above exist, and then, and only then, will some sort of regulation of mesh size be necessary for a Pareto-optimum.

Let us now examine the consequences of altering the assumptions that natural mortality is a function only of the age of the fish, and that the number of eggs laid per year is determined exogenously. If both are made functions of the size of the fish stock (F), no new variables are introduced, and the model is not significantly altered. Let the natural mortality (at a given age) and the number of eggs laid per year both vary directly with F. Then equation (1) becomes:

$$r = n(F)\cdot w_0'(F). \tag{1a}$$

$$r = r(m, F) = r(m, B(f, r)) = r'(m, f). \tag{1a'}$$

Stated in words, steady-state recruitment now depends both on mesh size and on the size of annual catch. Equation (5) becomes simply:

$$f = g[B(f, r'(m, f)), E, m]$$

$$f = h'(E, m). \tag{5a}$$

As before, sustained-yield catch depends on industry effort and mesh size, although in general the functional form will be different and perhaps more complex. Moreover, the same conclusions hold. If, and only if, increasing mesh size will lower total (steady-state) industry costs will mesh size regulation be necessary for a Pareto-optimum.

More formally, the assumption that variations in mesh size are costless implies:

$$\left.\frac{\partial h}{\partial M}\right|_{\Delta E=0} \leq 0. \tag{7}$$

The strict inequality holds for the case where m is such as to catch fish of minimum marketable size. When equation (7) holds, suboptimization with respect to mesh size has been achieved, and then industry catch can be written as a function solely of industry effort:

(8) $f = S(E)$ (industry sustained-yield catch, given suboptimum mesh size).

Equation (8) describes the "eumetric yield curve" [1, p. 70; p. 66, Fig. 1]. Alternatively, effort expended can be written as a function of fish caught:

$$E = t(f). \tag{9}$$

Since at sufficiently high levels of effort, sustained-yield catch may actually decline (negative industry returns to effort), t may be double-valued.

Benefits derived from the fish stock may be written:

$$G = \int_0^f P(f)df - C(E)$$
$$= \int_0^f P(f)df - C[t(f)].$$

Maximizing G, we have:

$$\frac{\partial G}{\partial f} = P(f) - \frac{\partial C}{\partial E}\frac{\partial t}{\partial f} = 0$$

$$P = \frac{\partial C}{\partial E}\frac{\partial t}{\partial f} = \frac{\partial C}{\partial f}. \tag{10}$$

In words, additional fish ought to be caught up to the point where the increment in costs associated with an additional pound of fish just equals the value the market places on that additional pound of fish. In the absence of a toll or charge for the use of the fish stock, long-run competitive equilibrium will be achieved when $P = C/f$, the average cost to the fishing industry.

Now, average cost equals marginal cost only when the former is a constant. If this is the case, additional industry output will be proportional to the extra fishing effort expended. This is equivalent to stating that increases in *industry* output have a negligible effect on the fish stock, and, hence, that the marginal product of the fishing banks is zero. If, however, increases in industry output decrease the fish stock, the marginal product of the fishing banks will be positive. Average cost will be less than marginal cost as defined, and external diseconomies of the first type will be present. If a toll, T, per pound of fish caught is levied, then $P = T + C/f$ is the condition for long-run competitive equilibrium. Hence, from equation (10), optimum utilization of the fishing banks requires that:

$$T = \frac{\partial C}{\partial f} - C/f. \tag{11}$$

In words, a toll should be levied on each pound of fish caught equal to the difference between the marginal social cost $(= \partial c/\partial f)$ and the marginal private cost ($= C/f$, the average industry cost in long-run competititve equilibrium). This toll is completely analogous to a rent on any fixed facility, and is exactly equal to the toll that would be collected by a profit-maximizing, perfectly competitive fishing-banks operator, if, indeed, fishing banks were owned by individuals.

It is interesting to note that if the marginal product of the fishing banks is zero, then "type 2" externalities are not present either. For if increasing the industry output has no effect on the fish stock, then fishing mortality is a negligible fraction of total mortality. Increasing mesh size from the minimum would also have a negligible effect on the size of the fish stock. The converse is not true; it is conceivable that "type 1" diseconomies could

exist without "type 2" diseconomies. Thus, (a) no regulation, (b) toll only, and (c) toll plus mesh-size regulation, are three possible optimizing prescriptions, depending on conditions actually encountered.

In the special case where Pareto-optimality does require both scale and mode regulation (case (c)), there is still no necessity to institute regulation by fiat to control mesh size. A penalty toll on all fish in a catch smaller than the size corresponding to the optimum mesh size would accomplish the same end. This fact hints that Turvey's dual recommendation, when valid, is a product of the peculiar technology involved in fisheries. Suppose that fishermen could choose whatever size distribution of catch they wanted, instead of being limited to choosing only the minimum size of fish in the catch. Then, regulation only of the minimum-size fish and of fishing effort would probably not be sufficient for a Pareto-optimum.

In terms of Figure 1, fishermen ought to be penalized for catching fish younger than the maximum expected weight age, and encouraged to devote relatively more effort to older age classes. A variable toll, either continuous or by size class, would be necessary. The actual form of the toll would also depend on the relative costs of landing different-size fish and upon the effects of the fishing activity on the shape of the $E(W)$ curve and on the number of eggs hatching. But, in general, it would not be infinity for fish smaller than a certain size and contant for larger fish.

J. HAYDEN BOYD*

* The author, instructor in economics at the University of Minnesota, is grateful to Herbert Mohring for several helpful comments.

REFERENCE

1. RALPH TURVEY, "Optimization and Suboptimization in Fishery Regulation," *Am. Econ. Rev.*, March 1964, *54*, 64–76.

Shifts in Demand and the Inflation Problem: Comment

Professor W. G. Bowen and Mr. S. H. Masters, in their paper "Shifts in Demand and the Inflation Problem" [1], discuss the relationship between the rate of inflation and the unevenness of changes in demand for the products of different industries. Their inquiry was inspired by Charles L. Schultze's well-known study of the inflation of 1955-57 [2], but their model differs from his in its emphasis on disparate changes in demand rather than differences in the prevailing levels of demand. Bowen and Masters divided the period from January 1947 to March 1963 into six subperiods, chosen to minimize the effects of cyclical factors, but varying in length from 18 to 68 months. For each subperiod, changes in the value of sales of 21 two-digit industries were tested for dispersion by two alternative but closely related measures. One of these measures (I_{st}) required the differences between the average proportional increase in sales and the growth rates of the separate industries' sales to be expressed as absolute values, weighted by the respective contributions of the industries to income originating in manufacturing

and summed. The other measure (I^*_s) is the weighted standard deviation of the different growth rates. The principal conclusions are, first, that there were indeed uncommonly pronounced disparities in demand changes during the period 1955-57 and, secondly, that the period from July 1957 to March 1963 was one in which the disparities were extraordinarily slight. From the latter result, a by-product of the inquiry, Bowen and Masters infer that recent price stability may have been due partly to a fortuitous circumstance.

It is argued in this comment that Bowen and Masters' evidence relating to changes in sales is an unsatisfactory basis for these inferences. The central problem is whether the changes in the money value of sales on which they rely can be accepted as a measure of changes in demand. Although Bowen and Masters draw attention to some defects of their technique, its limitations require greater emphasis. Why does the value of sales in one industry increase at a different rate from sales in another? If both industries are initially in market equilibrium, the following possibilities are perhaps the most important:

1. The two industries, although they have demand and supply functions with similar elasticities, are confronted with unequal movements in their demand curves. In this instance the unequal changes in sales can be attributed unambiguously to unequal changes in demand, and Bowen and Masters' reasoning is justified.

2. Similar movements in the demand curves cause different changes in the equilibrium quantities and prices because demand and supply elasticities differ.

3. Supply functions may change for a variety of reasons, including changes in physical production functions and spontaneous alterations in the costs of factors of production. Wage increases due to union aggressiveness are one example of the latter. Again, unequal movements in sales may ensue either because the separate industry supply functions are unequally affected or because similar changes interact with different demand and supply elasticities.

4. Although the new equilibrium conditions may involve equal proportional changes in sales, the speeds at which industries and their markets respond to changed circumstances may differ. Thus within any period less than that required to reach equilibrium, sales may change at unequal rates.

Relaxing the assumption of initial equilibrium, we encounter two further important possibilities. One is that factors of production previously in limited supply become available, with unequal effects on the output of different industries; the other that, for reasons implicit in factor (4) above, industries inherit from earlier periods unequal pressures to alter their production and prices.

No attempt can be made here to separate the effects of these factors, but the implicit assumption of Bowen and Masters that (1) predominates seems gratuitous. Two hypotheses related to this general reasoning are advanced, however, which give more concrete ground for rejecting their technique. The first is that the measures of dispersion and the rate of increase of prices are strongly correlated. This hypothesis is suggested by the supposition that firms and industries do not respond instantaneously or simultaneously to changes in market conditions; hence they are more likely to behave in a similar fashion

when there is little pressure to change. Moreover, the process of raising prices has a sequential character, as increases in some industries lead to increases elsewhere. In *any* short period of an inflationary nature, therefore, considerable dispersion in the rate of change of sales is likely to be apparent. It can, of course, be objected that the suggested correlation merely confirms the importance of unequal changes in demand as a cause of inflation. This is a logical possibility. The postwar inflation *may* have been due largely to unequal changes in demand, and economists may have been mistaken in emphasizing the role of such pressures as high liquidity, generalized excess demand, and union aggressiveness. A more reasonable interpretation, however, is that the association of varying degrees of dispersion with changing rates of inflation is less a causal relationship than a consequence of the tendencies outlined above. In any event, it becomes difficult to maintain that there was anything special about the periods 1955-57 and 1957-63. Bowen and Masters would need to show that sales changes were even more unequal in the former period and even less so in the latter than the normal relationship between inflation and dispersion would lead one to predict.

The second hypothesis is more straightforward: the degree of dispersion is likely to diminish as the interval of time considered is extended, for the general movement of sales, under conditions of inflation and increasing real output, comes increasingly to submerge short-term changes in prices and output in different industries. If this expectation is confirmed, the Bowen-Masters measures cannot validly be used to compare periods of different length.

Considerable manipulation of the published data is necessary before these hypotheses can be tested, and limitation of time has obliged me to rely on Australian data which, for other purposes, were converted into suitable form. Professor Bowen and Mr. Masters, after reading a draft of this comment, tested the second hypothesis against their data for the United States and have generously allowed me to use their results; they could not, unfortunately, undertake a similar test of the first hypothesis. The disadvantage of using the Australian statistics is perhaps reduced by Australia's diverse experience of inflationary conditions, because of which properties of the Bowen-Masters measures which emerge less conspicuously in the American data may be easier to detect.

The Australian manufacturing industries distinguished in the official statistics have been consolidated in 27 groups,[1] and the two dispersion measures are calculated on this basis for each year from 1948-49 to 1961-62. Statistics of sales are not available and value of output estimates, differing from sales to the extent of inventory changes, are substituted. Otherwise the data appear to be strictly comparable with those used by Bowen and Masters. Table 1 shows both the values of the dispersion statistics calculated for successive annual changes in output and estimates of annual price increases. The latter estimates are obtained by calculating the implicit price index for gross national product

[1] A list of these groups, to be useful for verification or further research, would need to be supplemented by a detailed statement of the various industries consolidated into them. This information is available on request.

TABLE 1—INFLATION AND INEQUALITY OF CHANGES IN VALUE OF OUTPUT IN AUSTRALIA, 1948–49 TO 1961–62
(All numbers relate to changes since previous year)

Year	Percentage Price Increases	Bowen-Masters Measures	
		I_{st}	I^*_s
1949–50	10.1	6.07	8.27
1950–51	13.8	9.17	12.00
1951–52	17.1	9.44	12.57
1952–53	17.0	8.55	11.10
1953–54	4.6	5.98	8.53
1954–55	2.4	6.07	7.55
1955–56	5.2	5.36	6.47
1956–57	5.6	6.45	8.31
1957–58	1.7	5.11	6.76
1958–59	1.7	2.56	3.81
1959–60	4.3	4.65	5.23
1960–61	4.9	3.55	4.48
1961–62	1.0	2.80	3.60

less exports.[2] This index minimizes distortions due to external trade influences which affect the more conventional indices and prevent these indices from indicating accurately the behavior of internal prices. However, the general relationship which emerges if the Consumer Price Index is used is similar to that apparent in Table 1, although slightly less pronounced.

The Spearman coefficients of rank correlation between price increases and the two dispersion measures are 0.84 (for I_{st}) and 0.82 (for I^*_s). Although there is sufficient unexplained variance to preclude any suggestion that the

TABLE 2—VALUES OF BOWEN-MASTERS MEASURES APPLIED TO CHANGES IN VALUE OF OUTPUT: AUSTRALIA, SELECTED PERIODS BETWEEN 1948–49 AND 1961–62

	I_{st}	I^*_s
1948–49 to 1953–54		
Minimum annual values during period	5.98	8.27
Mean of annual values during period	7.84	10.49
Values calculated for entire period (annual equivalents)	2.92	4.17
1953–54 to 1959–60		
Minimum annual values during period	2.56	3.81
Mean of annual values during period	5.03	6.36
Values calculated for entire period	3.31	3.77
1948–49 to 1961–62		
Minimum annual values during period	2.80	3.60
Mean of annual values during period	5.47	7.19
Values calculated for entire period	2.41	2.90

[2] Implicit price index numbers calculated from *Australian National Accounts: National Income and Expenditure 1948–49 to 1963–64* (Commonwealth Bureau of Census and Statistics).

TABLE 3—THE RELATIONSHIP BETWEEN BOWEN-MASTERS MEASURES AND LENGTH OF TIME INTERVAL: UNITED STATES, 1947–63

	I_{st}	I^*_s
January 1947 to July 1948	4.96	7.19
July 1948 to March 1951	3.94	5.00
January 1947 to March 1951	2.86	4.11
March 1951 to July 1953	4.76	5.96
July 1953 to September 1955	2.46	3.17
March 1951 to September 1955	1.89	2.68
September 1955 to July 1957	5.34	6.34
July 1957 to May 1960	1.48	1.99
September 1955 to May 1960	2.57	2.79
May 1960 to March 1963	1.68	2.04
July 1957 to March 1963	1.57	1.94
September 1955 to March 1963	1.60	1.92
January 1947 to March 1963	1.67	1.91

Bowen-Masters measures are merely proxy price indexes, the correlation is sufficient to require considerable caution in deriving inferences, from the dispersion of sales increases, about the importance of structural variations in demand as a cause of inflation.

Tables 2 and 3 show the effect on the Bowen-Masters statistics of extending the time interval examined. In Table 2, relating to Australia, the results of the annual analysis given in Table 1 are compared with dispersion measures calculated for three longer periods. The tendency for dispersion to diminish with the extension of the period is apparent in all of these comparisons. Table 2 combines periods originally studied by Bowen and Masters to allow similar comparisons for the United States, italics being used to designate composite periods. The same tendency emerges, but more conspicuously with respect to earlier periods, lending support to the suggestion that the tendency is due to the effect of inflation in generating short-term dispersion.

The lesson is that better tests of the demand-shift hypothesis are necessary. With the data available, these tests are difficult to devise and it was presumably for this reason that Bowen and Masters experimented with the change-in-sales method. The apparent inadequacy of this technique means that the substantive issues which they attempted to resolve must for the present be regarded as open.

KEITH HANCOCK*

* The author is professor of economics at Flinders University of South Australia and in 1964-65 held a visiting appointment at Princeton University. He is indebted to Professor Bowen and Mr. Masters for magnanimous comments and assistance. The note includes statistical results obtained by the use of computer facilities at Princeton supported in part by National Science Foundation Grant NSF-GP 579.

REFERENCES

1. W. G. BOWEN AND S. H. MASTERS, "Shifts in Demand and the Inflation Problem," *Am. Econ. Rev.*, Dec. 1964, *54*, 975-84.
2. C. L. SCHULTZE, Study Paper No. 1 in Joint Economic Committee, *Recent Inflation in the United States*. Washington 1959.

A Theory of the Bargaining Process: Comment

J. G. Cross's theory of the bargaining process is composed of three elements: a theory of decision-making, a theory of expectations, and a theory of learning. The theory of decision-making consists of the maximization of the present value of an expected payoff, in utility terms, and this aspect of Cross's work will not be questioned in the present paper. The theory of expectations, however, will be examined from the point of view of both empirical plausibility and theoretical fruitfulness, and as a result of this analysis a reinterpretation will be suggested. This will be shown to have repercussions not only within the learning part of the model, but also for the predictions of the theory as a whole. It will be argued that the revised model has some relevance to a bargaining situation of considerable theoretical interest and practical importance, which could not otherwise have been treated within the framework of the Cross theory. This is a bargaining situation involving "brinkmanship."

I. *Bargainer's Expectations*

The particular aspect of the theory of expectations which I would like to examine is that concerning each bargainer's expectations of the other's concession rate. Obviously, relatively simple assumptions must be made about the behavior of such rates if an analytically manageable model is to be built. However, by making the particular assumption that he does, Cross automatically excludes from his theory a particularly interesting case—that of bargaining involving brinkmanship. What I mean here by a brinkmanship situation is one in which a bargainer expects his opponent to make a concession at some point in the future and, in the light of this expectation, is prepared to wait, making no concession himself. Casual observation seems to suggest that this brinkmanship type of situation is one that typically occurs in bargaining processes.

Let us confine our attention, as Cross does, to the pure intransigence case of bargaining, i.e., the case where each bargainer always expects to obtain agreement at his own current demand. Using the Cross notation, we have utility functions $f(q_1)$, $g(q_2)$ defined over the quantity variables q_1, q_2 where q_1 is I's share of Q, q_2 is II's share, and the total amount of Q is M. Disagreement occurs when the demands $q_1+q_2>M$, and the amount of disagreement (i.e., the total concessions which must be made before agreement is possible) is (q_1+q_2-M). In his paper Cross supposes that each bargainer expects the other to concede at a certain rate, i.e., he expects the other to make a continuous series of demands with no discrete jumps. Thus, if I

expects II to concede at the rate r_2, he is expecting that $-(dq_2/dt)=r_2$. However, each expects the other to start conceding, but neither intends making any concession himself. According to Cross, each bargainer immediately begins to modify his expectations of the other's concession rate, since in general

$$-\left(\frac{dq_1}{dt}\right)_{t=0} \neq r_1$$

$$-\left(\frac{dq_2}{dt}\right)_{t=0} \neq r_2;$$

and hence the expectations r_1, r_2 must change. These new expectations due to learning feed back into the equation by which each made his original optimal choice of q_j, and a new optimal q_j can be found in each case. And what looks like a new optimum to the bargainer who made it, looks like a concession (or retraction) to the other. Hence the r_j are further modified, and so it goes on.

Let us focus our attention on the form of expectations the bargainers are assumed to have. According to Cross's assumptions each bargainer expects the other to concede at a constant rate over future time (although this rate which is constant over future time may in fact change as present time elapses). It is my contention that bargainers are typically faced with a different form of expectation from this one, the difference being one which materially affects the workings of Cross's model.

In practice, each bargainer typically expects any concession to occur in a sudden discrete lump.[1] Thus, in the pure intransigence case where each bargainer expects the other to agree to his own demand, I may expect II to make the whole concession suddenly from q_2 to $M-q_1$ at or before some point in the future, t_2, and II may expect I to make the whole concession suddenly from q_1 to $M-q_2$ at or before some point in the future, t_1. Thus if neither bargainer has made any concession up to some point in time which is less than both t_1 and t_2, the situation is quite consistent with the expectations of each bargainer. Hence, neither bargainer will modify his expectations as a result of the other's failure to concede during this period, as they would under Cross's assumptions. In fact no learning can occur in this case until the time $t=\min(t_1, t_2)$, at which point one of the bargainers must modify his original expectations.

It is here that an interesting asymmetry, which was absent in Cross's theory, arises. Whereas with Cross's theory the bargainers could learn that their estimates of the concession rate were correct or too large or too small, learning in the present theory is confined to discovering that the estimate of the time before the other bargainer concedes completely is too small.

[1] What I am arguing against here is not the assumption of continuity by the use of differential equations rather than difference equations, which is a legitimate analytical approximation, but rather the assumption that the expected rates of concession r_j are independent of future time.

Whereas Cross's estimated concession rates r_j can increase or decrease as a result of learning, the times t_1, t_2 in the brinkmanship theory can only increase as a result of learning.

Bargainers whose expectations run along brinkmanship rather than constant-concession-rate lines are not in a position to engage in the kind of learning process which is basic to the Cross theory. We will perhaps understand this better if we consider both kinds of expectation as special cases of the general case: the expected concession rate of bargainer II is a function of future time, $R_2(t)$. The expected time elapsing before agreement occurs, w, is then given implicitly by

$$\int_0^w R_2(t)dt = -(q_1 + q_2 - M). \tag{1}$$

Following R. H. Strotz [2] and Cross [1], the optimal choice of q_1 is given by the condition that the present value of I's expected payoff, $f(q_1)e^{-aw}$, is a maximum with respect to the choice of q_1 (neglecting any fixed costs of bargaining per period, since these add nothing new in principle to the discussion).

$$\frac{d}{dq_1} f(q_1)e^{-aw} = 0$$

i.e.,

$$f'(q_1) - af(q_1)\frac{dw}{dq_1} = 0$$

$$f'(q_1) + a\frac{f(q_1)}{R_2(w)} = 0 \tag{2}$$

since

$$\frac{dw}{dq_1} = \frac{-1}{R_2(w)}.$$

Now $R_2(t)$ is bargainer I's guess at the value of $-(dq_2/dt)$ at time t after the start of the bargaining. After time t_0 has elapsed, I knows the value of $-(dq_2/dt)$ for all $t \leq t_0$, and hence I can learn about the function $R_2(t)$ in the interval $0 \leq t \leq t_0$. However, in order to solve equation (1) for w, for any given q_1, I will in general have to estimate $R_2(t)$ for $t > t_0$. It may be possible for a bargainer to use the learning about $R_2(t)$ for $t \leq t_0$ to modify his expectation of $R_2(t)$ for $t > t_0$, but it is here that the two special cases arise. These, together with the general case, are shown in Figure 1. This figure displays various assumptions which could be made about the expectations which each bargainer has of the other's concession behavior. The most general assumption would be that bargainer I, say, expects II's demand to be some (well-behaved) function of future time which, in the above notation, will be given by

$$q_2(t) = \overset{0}{q_2} + \int_0^t R_2(z)dz$$

(where the ith demand of the jth bargainer is q_j^i). Such an assumption is not very fruitful, since it is devoid of any implication regarding the workings of the model. A more restrictive assumption is shown as the Cross case. This involves expecting that the other bargainer's demand will decrease linearly with future time or, in other words, that he will concede at a constant rate. The brinkmanship assumption, on the other hand, restricts the form of expectation in another way. What is expected here is that a time

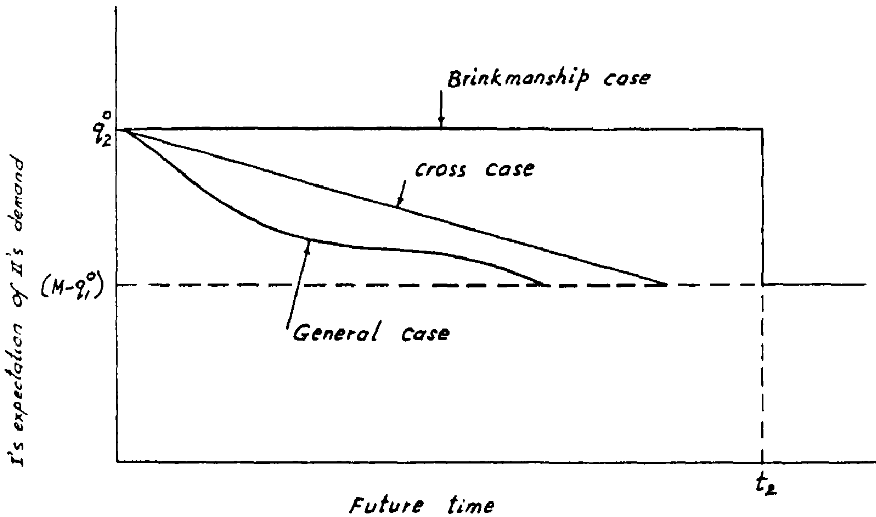


FIGURE 1. EXPECTATIONS OF CONCESSION BEHAVIOR

will come (t_2) at which the other bargainer will give in to the demand which is being made of him.

Let us examine the effect of learning in the circumstances characterized by the three different forms of expectations represented in Figure 1.

1. In the case of the Cross assumption, any learning about II's concession rate, $-(dq_2/dt)$, for $t>0$ will, at that particular instant, modify the slope of the line for all future time in an unambiguous way (i.e., the slope is correct or too big or too small at that particular instant).

2. In the general case, any learning about $-(dq_2/dt)$ for $t \leq t_0$ modifies $R_2(t)$ for $t \leq t_0$ and could conceivably be used to make modifications of $R_2(t)$ for $t>t_0$, but not in an unambiguous way. However, if one could develop a reasonable learning model for this general case, then, with any well-behaved R-function, this modification of the Cross theory would presumably yield a determinate solution through the process of utility maximization by each bargainer. (We note that the R-function in the pure brinkmanship case is not well behaved in that it is not differentiable at $t=t_2$.

3. In the brinkmanship case any learning about q_2 for $t \leq t_0$ does n modify the curve at all if $t_0 < t_2$ and $t = t_2$ is the only point at which it ca be modified. A learning model to account for this case would differ fro Cross's in an essential way.

We see that as we examine different forms of expectations regarding co cessions from the other bargainer, Cross's learning-concession process worl in the special case of expected concession rates constant over future tim runs into difficulties in the general case of expected concession rates a fun tion of future time, and breaks down in the brinksmanship case. (That to say, it breaks down in the sense that it would not yield a solution in tl absence of a considerably modified learning model.) Thus, bargaining pr cesses in which expectations of brinkmanship are an essential componei would require a development of Cross's theory to describe them adequatel We here outline a possible development in this direction.

II. *A Brinkmanship Model*

In the brinkmanship case, I expects II to give in completely to his ow demand, q_1, after holding out for a time t_2. Thus, if we apply the princip of utility maximization to predict I's choice of the value of q_1, we find r limit imposed. With the theory as it stands, utility maximization woul require that I demands the largest q_1 which is possible, i.e., $q_1 = M$. Similarl this principle would require II to demand $q_2 = M$. This rather barren pos tion can be avoided by reasoning as follows: I knows that, if q_2^i is II demand, he can have $q_1 = (M - q_2^i)$ immediately, but demands for $q_1 > (M - q$ will take longer to obtain (and similarly for II). We suppose, then, th each bargainer expects the time which the other will hold out before givin in completely to be a function of his own demand. I expects $t_2 = F(q_1)$,] expects $t_1 = G(q_2)$. Following Cross in making a linear assumption, we have

$$t_2 = U(q_1 + \overset{0}{q}_2 - M) \tag{3}$$

[3] It could be argued that the linear expectations used here and in Cross's theory are n economically plausible since each bargainer knows that no bargain will ever be conclud outside the contract zone, i.e., there should be no finite t_2 corresponding to $q_1 > M$ and r finite t_1 corresponding to $q_2 > M$. (In practice, things are a bit fuzzier than this because neith bargainer knows the size of M exactly.) The linear functions, however, yield a finite t_j f any q_k, however large.

Plausible nonlinear functions expressing the expectations within the contract zone wou be, for example:

$$t_2 = \frac{S_1(q_1 + \overset{0}{q}_2 - M)}{(M - q_1)}, \qquad q_1 \leq M$$

$$t_1 = \frac{S_2(\overset{0}{q}_1 + q_2 - M)}{(M - q_2)}, \qquad q_2 \leq M.$$

Here the times become infinite as the ends of the contract zone are reached. Such refinemen are not considered here since they raise many issues which are not directly relevant to tl argument of this paper.

$$t_1 = V(\overset{0}{q_1} + q_2 - M) \tag{4}$$

where U, V can, for the moment, be thought of as parameters.

If we assume discounting functions e^{-at} for I and e^{-bt} for II, then the present value to I of the demand q_1 is $f(q_1)e^{-at_2}$ where t_2 is given by (3). The condition that q_1 maximizes this expression is

$$f'(q_1) - aUf(q_1) = 0. \tag{5}$$

And similarly for II,

$$g'(q_2) - bVg(q_2) = 0. \tag{6}$$

These are the conditions that each bargainer makes, in the light of his own expectations, an optimal demand.

Suppose the original q_1, q_2 at $t=0$ are such that

$$t_2 = \overset{0}{t_2} < t_1 = \overset{0}{t_1}.^{3}$$

I cannot do any learning until $t=t_2^0$ and II cannot do any until $t=t_1^0$. Thus, in our case of $t_2^0<t_1^0$, I is the first to do any learning. At time $t=t_2^0$ I observes that II does not give in and realizes that his expectation of t_2 was wrong. Thus he wishes to revise his estimate, and we may suppose that he comes to the conclusion that he has been associating too low a value of t_2 with each value of q_1. I's learning will therefore lead to an upward shift in his t_2-function, although we still must have $t_2=0$ when $q_1=(M-q_2^0)$. The t_2-function pivots upwards about $q_1=(M-q_2^0)$ as a result of learning. This result follows from supposing that

$$U = U_0 \qquad \text{when } t \leq \overset{0}{t_2}$$

$$U = U_1 > U_0 \quad \text{when } \overset{0}{t_2} < t \leq \overset{1}{t_2}$$

or, in general,

$$U = U_{i+1} > U_i \quad \text{when} \quad \overset{i}{t_2} < t \leq \overset{i+1}{t_2}. \tag{7}$$

Similarly, for II we would have

$$V = V_{i+1} > V_i \quad \text{when} \quad \overset{i}{t_1} < t \leq \overset{i+1}{t_1}. \tag{8}$$

This then is the basis of a modified learning theory applicable to the brinkmanship case.

[3] If I makes the first demand he will have to use an estimate of q_2^0, $q_2=q_2^*$, say, this being the demand he expects II to make. This yields an expected delay time $t_2=t_2^*$ However, as soon as II's actual demand q_2^0 becomes known (still at $t=0$) q_2^* is replaced by q_2^0 and t_2^* is modified to $t_2^0=U(q_1^0+q_2^0-M)$.

Let us examine the workings of this model. After a time $t < t_2$ has elapsed, the present value of I's expected payoff is $f(q_1)e^{-a(t_2-t)}$, and the q_1 which maximizes this is given by

$$f'(q_1) - a\frac{dt_2}{dq_1}f(q_1) = 0,$$

which is identical with (5) above since $dt_2/dq_1 = U$. Hence, although the present value of I's expected payoff is increasing as time passes, the value of q_1 which maximizes it is invariant with respect to time, so long, of course, as the parameters remain unchanged.

The q_1 which satisfies (5) can be thought of as the solution of the pair of simultaneous equations

$$y = f'(q_1) \tag{9}$$

$$y = aUf(q_1). \tag{10}$$

Consider now what happens when I learns at time t_2^0 and increases the value of U. As shown in Figure 2, the function OS will shift upwards to OT, giving a new value for the optimal q_1, OB. This will be less than the previous value, OA. Thus I makes a concession AB. Here we are faced with a similar

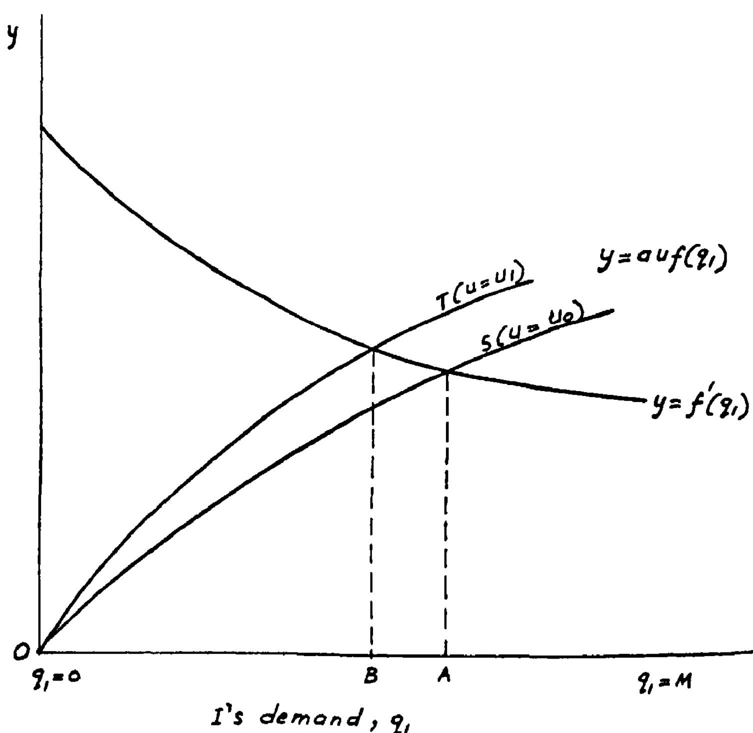


FIGURE 2. THE OPTIMAL-DEMAND DECISION

paradox to that which Cross observed in connection with the intransigence aspect of the theory. Each bargainer expects the other to make complete concessions $q_1 \to (M-q_2)$ and $q_2 \to (M-q_1)$, but each himself only makes what are in general partial concessions (like AB). Any change in q_1 could be a complete concession if U increases enough to bring the intersection above or to the left of $q_1=(M-q_2)$.

When II observes I make a partial concession at a time t_2^0, he does not regard this as inconsistent with his expectation that I will give in completely at or before $t=t_1^0(>t_2^0)$. Hence II does not do any learning until $t=t^0{}_1$, in which case V increases, giving a new value for $t_1(t_1^1>t_1^0)$,[4] and a new optimal q_2 less than the previous one.

Thus, since each change of demand by one or the other bargainer represents a partial concession and not a retraction, the learning-concession process should lead eventually to an agreement. A learning theory consistent with (7) and (8) but giving the actual changes in U, V, and hence in t_1, t_2, would enable a solution to be derived.

Whereas in Cross's original version changes in demands could be concessions or retractions, in this brinkmanship version of the theory, all the changes in demands are concessions. The reason for this is that when a bargainer gets to the time at which he expects a complete concession, he can learn that his estimate of the time was correct (if the other gives in) or that it was too small (if the other does not give in). Thus, learning in the brinkmanship model always consists in increasing the expectation of the time t_1 (or t_2), i.e., in increasing U or V. (In Cross's theory the rates r_1, r_2 could increase or decrease as a result of learning.) Thus, the function $y=aUf(q_1)$ can shift upwards but not downwards. Since $y=f'(q_1)$ is a strictly decreasing function of q_1, by the assumption of diminishing marginal utility, we can see that if $y=aUf(q_1)$ (which is a strictly increasing function of q_1) shifts upwards, it must always intersect $y=f'(q_1)$ at a smaller value of q_1, i.e., the change of q_1, being a decrease, represents a concession and not a retraction.

This analysis may cast some light on one of Cross's comments. He says [1, p. 76]:

> Often, retraction of an offer during negotiation is thought to be either unethical or at least undesirable because of the unfavorable utility shifts which such behavior may induce in the other player. . . . Nevertheless, such retractions sometimes occur in practice. We will, therefore, retain the possibility of their occurrence, recognizing that forces probably exist which discourage increases in demands during the course of negotiations.

It seems that the model here developed displays the forces which discourage increases in demands during the course of the negotiations by taking into account the brinkmanship aspects of the bargaining process.

[4] In order for $t_1^i > t_1^{i-1}$ we require that $dt_1/dq_2<0$, since q_2 cannot increase over time. It can be shown that a sufficient condition for this is $t_2>1/a$. (In fact this is a rather strong condition and, in general, t_2 does not have to be as large as this for the condition to hold.) Similarly, we require $t_1>1/b$. To simplify the analysis we assume that these conditions are satisfied.

In summary, it seems that one aspect of Cross's model which is over-restrictive, in the sense that it appears openly inconsistent with casual observation, is the assumption of the expectation of a rate of concession independent of future time. However, it would appear that, with the type of modifications suggested above, the essence of Cross's argument would remain. It would still be possible, as has been indicated, to construct a determinate, descriptive theory of the bargaining process based on the utility-maximizing behavior of individuals.

ALAN CODDINGTON*

* The author is the Ellis Hunter Memorial Fellow in Economics at the University of York.
He would like to thank John H. Williamson for his valuable advice and comments on earlier versions of this paper.

REFERENCES

1. J. G. CROSS, "A Theory of the Bargaining Process," *Am. Econ. Rev.*, March 1965, *55*, 67–94.
2. R. H. STROTZ, "Myopia and Inconsistency in Dynamic Utility Maximization," *Rev. Econ. Stud.*, 1956, *23*(3), 165–80.

A Theory of the Bargaining Process: Reply

There is little doubt that concessions during the bargaining process are composed of discrete lumps. The presence of indivisible components of the payoff is by itself sufficient for this conclusion and furthermore there is substantial evidence that bluffing behavior tends to bring about discontinuous changes in explicit demands as well. Alan Coddington's primary objection to my model is not that assuming a constant concession rate makes for a poor approximation to discrete reality, however, but rather that even "casual observation"(!) suggests that negotiators expect no concessions at all until a time of utter capitulation is reached. Since expectations themselves are generally not observable under any but the most carefully controlled experimental circumstances, I conclude that Coddington is inferring his hypothesis from observations of bargaining *behavior*. This, then, is in part an empirical question. Of course one can find many examples of negotiations in which initial demands are preserved over considerable periods before any concessions are made. These circumstances, however, need not be associated with discontinuous expectations, for they are easily attributable to the presence of bluffing behavior and to the absence of any substantial disagreement costs as well (e.g., the strike date may still be six weeks away and, although initial positions have been outlined, serious negotiations have not yet even begun). Furthermore, both logical and empirical considerations lead me to be skeptical about Coddington's hypothesis:

1. Coddington's own model predicts that the negotiators will concede continuously after some point in time. Logically, it is hard to see why experienced negotiators would not come eventually to *expect* such behavior.

We are asked to accept a model in which an individual's expectations lead to outcomes which are fundamentally different from those anticipated, and yet in which subsequent negotiations are approached with expectations which are unchanged from their original form! Coddington attempts to avoid this dilemma by assuming that each player simply disregards any of his opponent's concessions which fall short of utter capitulation, but again one must be nagged by the realization that each player knows that his own concessions *do* reflect his own preferences and expectations, and hence that they should not be disregarded.

2. Experimental evidence does not support Coddington's suggestion that a substantial period will pass without serious concessions. For example, Figure 1 contains a plot of a typical case from Siegel and Fouraker's [2] bargaining experiments.[1] Concessions here take place immediately and, in fact, they are virtually continuous.

3. Empirical studies place so much significance upon the *sequence* of concessions that it is hard to believe that they would not be taken seriously by the negotiators themselves. For example, in a recent study, R. W. Walton and R. B. McKersie specifically state that "the alternation of concessions is not a mechanical process. *Each side is alert to the meaning of the other's concession*" [3, p. 88, italics added]. That is, not only are concessions *expected,* but they are deliberately analyzed for new information.

A related point deals with Coddington's suggestion that even very large concessions coming long before the expected capitulation date will not influence the behavior of a negotiator. This position also appears to be implausible on empirical grounds. For example, Walton and McKersie devote some time to the dangers of "signalling weakness" to an opponent through one's own concession behavior [3, pp. 89–90]. Furthermore, as Coddington realizes, his hypothesis would preclude the retraction of bids during negotiations—a phenomenon which is far from uncommon in experience. I am afraid that Coddington has misinterpreted those statements which I made regarding the retraction of bids, and which he has quoted in his *Comment.* I was by no means searching for economic forces which could prevent "undesirable" demand retractions. In fact, it should be emphasized that my statements were meant to reflect my decision to reject just such a model as Coddington has proposed. The point is that any mechanistic model of behavior must either introduce psychological variables relevant to the environment in which the operation takes place or it must not be construed to cover situations in which that environment is likely to be altered drastically. Retractions can generate resentment, and the resulting change in psychological atmosphere can alter the whole course of the bargaining process. (Indeed, in many historical cases, it has.) Thus I was not seeking a device to *prevent* retractions, but rather was expressing doubt that their extent and frequency could be described adequately without a more psychologically-oriented model.

[1] Figure 1 is reproduced from Figure 5.2, page 78, S. Siegel and L. E. Fouraker, *Bargaining and Group Decision Making* (New York, McGraw-Hill Book Company, 1960) with permission from the publisher.

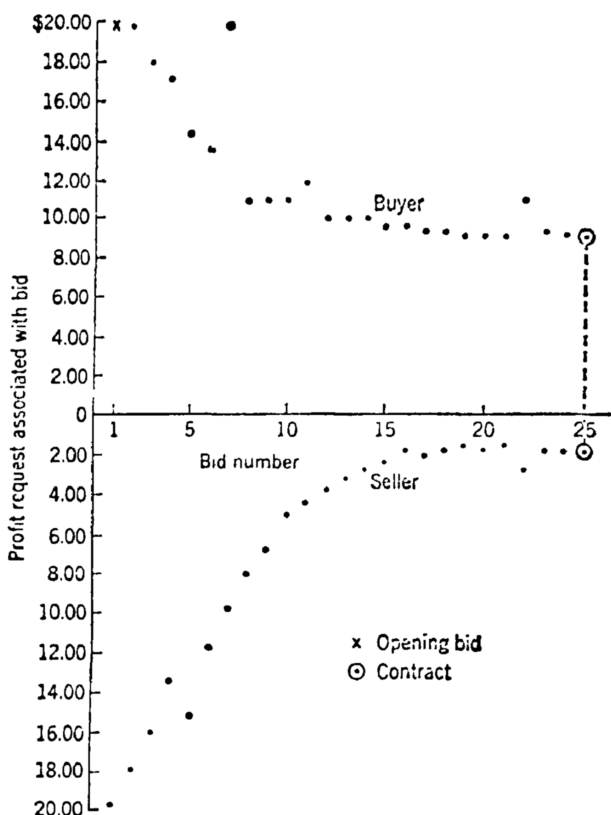


FIGURE 1

Source: From *Bargaining and Group Decision-Making* by S. Siegel and L. Fouraker. Copyright 1960 by McGraw-Hill Book Company. Used by permission.

Coddington states that his modifications do not affect the determinacy of the model. This is not strictly true. Although his model does have a determinate solution, his refusal to permit intermediate concessions to influence the learning process has eliminated a crucial interdependence which was present in my original formulation. Suppose that we consider one bargainer in Coddington's model. This individual makes a bid, say q_1^0, which he maintains over a period t_0; after t_0, if his opponent has not already capitulated, he begins to concede regardless of what his opponent may be doing. If the other never concedes at all, the first player will eventually give in all the way to $q_1 = M - q_2^0$.The important point is that, under Coddington's formulation, ***until agreement is reached, each player's time path of concessions is completely independent of the other's behavior***, and, in fact, each player's concessions are determined solely by his own initial expectations and by the nature of his own learning process. The outcome, of course, is dependent upon the same variables. A major aspect of my own model was the demonstration of the existence of an equilibrium process

during negotiation, such that within the limits imposed by the finite time dimension of bargaining, the outcome is ***independent of initial expectations*** (and hence of the initial demands). Of course, neat dynamic properties of economic models cannot be taken as indicators of their empirical validity, and for this reason, it would be unfair to cite these properties as a criticism of Coddington's comment. Nevertheless, I think this distinction between the workings of the two models is extremely important; if the equilibrium process does not hold, we must conclude that random variations in initial expectations are directly reflected in bargaining outcomes, and our hopes of constructing a model, capable of reasonably accurate prediction, must be greatly dimmed.

Finally, the generalized expected-concessions function, $R(t)$, is a useful addition to the theory. As Coddington points out, it is not difficult in principle to deal with this function. The learning model can be simpler than he supposes, however. For the simple model, we needed only the learning condition that the expected concession rate was increased whenever actual concessions were found to exceed expectations, and vice versa. In the general case, when the expected rates are found to underestimate reality, we would have the value of

$$\int_0^{q_1+q_2-m} \frac{1}{R(t)}\, dq,$$

decreasing for all values of (q_1+q_2-M). (Although Coddington does not mention it, it is likely that the function $R(t)$ is dependent upon M as well as upon time and the learning process.) The model can then be carried through just as before, with the forms of the $R(t)$ functions becoming important additional parameters in the determination of negotiation outcomes. For example, it can be shown (if it is not already obvious) that, even if the two parties expect the bargaining process to take the same total amount of time, the outcome, *ceteris paribus*, will be biased against the party who expects his opponent to concede at the higher initial rate.

John G. Cross*

* The author is assistant professor of economics at the University of Michigan.

References

1. J. Cross, "A Theory of the Bargaining Process," *Am. Econ. Rev.*, March 1965, *55*, 67–94.
2. S. Siegel and L. E. Fouraker, *Bargaining and Group Decision Making*. New York 1960.
3. R. E. Walton and R. B. McKersie, *A Behavioral Theory of Labor Negotiations*. New York 1965.

Windfall Income and Consumption: Comment

The purpose of this comment is to suggest a reconciliation of the contradictory findings of Ronald Bodkin [1, pp. 602–14] and M. E. Kreinin [5, pp. 388–90] published earlier in this *Review*. Bodkin's findings, which were based on the 1950 Survey of Consumer Expenditures of the Bureau of Labor Statistics, revealed that the marginal propensity to consume out of windfall income (MPC^w) is of the same order of magnitude as the marginal propensity to consume out of current income (MPC^y). These findings are at variance with Friedman's permanent income hypothesis [3] [4] and were challenged by Kreinin who found that MPC^w, as computed from the 1957/58 Israeli Saving Survey, was about 0.17 or only about a quarter of the MPC^y estimate (0.80).

Bodkin attempted [2, pp. 445–47] to reconcile this contradiction by advancing some arguments which he thought explained the low MPC^w in Israel. First, during the period of the Savings Survey, various exchange rate restrictions were in force in Israel, but recipients of German restitution payments enjoyed a number of privileges as long as they held the money in special accounts. According to Bodkin this provided an incentive to save the restitution receipts. Second, expectations of devaluation induced the restitution recipients to hold their money in liquid assets linked to the foreign exchange rate. These are Bodkin's main arguments, but he noted two secondary factors.

First, he doubted whether it was correct to classify restitution receipts as windfall income, since there were prolonged negotiations before the money was received. The second factor was that the "restitution recipients had as a group experienced many hardships (and worse) during an earlier period of their lives. It seems quite likely that such experiences would make individuals cautious, alert to adverse possibilities, and hence less likely to spend a one-time receipt than would be likely for a group of young American veterans."

We shall attempt to refute these arguments in the light of some additional tests we have made. However, apart from the empirical evidence, we suggest that Bodkin did not interpret correctly the possible effect of the Israeli exchange rate restrictions on consumption out of restitution receipts as compared with the marginal propensity to consume out of current income.

We believe that the reason for the discrepancy between the results of Bodkin and Kreinin lies in the relatively small sums which served as windfall income in Bodkin's data—about 7 per cent of current income on the average—while in Kreinin's data the restitution receipts were of the same order of magnitude as current income of the recipients. This is an important difference, as our findings suggest that the marginal propensity to consume out of windfall income (MPC^w) decreases strongly as windfall income rises.

I. *Some Reflections on Bodkin's Explanation of Kreinin's Findings*

We shall start with Bodkin's last argument that the MPC^w of restitution recipients is especially low because these were people who had experienced an extremely hard life. In order to test this hypothesis we took out of the 1963/64 Family Expenditure Survey[1] all the families who had received any lump-sum income in 1963–64 and divided this group into two: restitution recipients and others. There were 82 families in the first group, and 170 in the second. In each group we ran a regression of the form:

$$C = \alpha_0 + \alpha_1 Y + \alpha_2 N + \alpha_3 P + u, \tag{1}$$

where C is family consumption[2] expenditure; Y is family current income; N is family size; and P is restitution receipts or other[3] windfall income.

If Bodkin's argument holds, the estimate of α_3 obtained from the first group should be significantly lower than that obtained from the second group. But the results revealed that the estimates did not differ significantly. MPC^w for the first group was 0.2603 ($\pm$0.0744) and for the second 0.2544 ($\pm$0.050). We think that these results disprove Bodkin's hypothesis concerning different consumption behavior of restitutions recipients.

We shall turn now to Bodkin's argument[4] that it was not legitimate to classify the restitution receipts as windfall income because the typical restitution payment did not surprise the recipient, since it had been subject to prior negotiation. We think that his reservation on this point is reasonable on the grounds that if people look forward to a windfall for some time they may well increase their consumption on account of it (perhaps by taking loans); when they actually receive the windfall the net increase in their wealth is therefore less than the total windfall, and the estimate of MPC^w is downward-biased. If this is so, restitution payments received in period $t+1$ should affect consumption expenditure in period t.

In order to test this we took out from the Israeli 1958/59 reinterview Savings Survey[5] all families[6] who received windfall income in 1958–59 and

[1] See Appendix.

[2] Throughout this paper durables are included in consumption expenditure otherwise cited. The exclusion of durables did not affect the conclusions, the results in this case being $\alpha_3 = 0.2050$ for restitution recipients and 0.1695 for recipients of other kinds of windfall income.

[3] Other windfall incomes consisted of severance pay by employers, lottery prizes, local legacies in cash, cash gifts from abroad, domestic cash gifts, and lump-sum National Insurance gratuities.

[4] Bodkin has another objection to the classification of restitution receipts as windfall income: "the restitution payments might be considered analogous to insurance receipts, as they were intended to be partial compensation for losses sustained by the individual in the past. Thus, some individuals might have regarded their receipts as similar to a return of capital and hence might have been less prone to consume out of such a receipt." As the restitution receipts were intended to be a partial compensation for loss of families and wealth during the Second World War we do not think Bodkin's argument sound in this case.

[5] See Appendix.

[6] To be exact those were investigation units the number of which differed by about 5 per cent. We had no information on which of them received restitution payments, but as these account for about two-thirds of total windfall income, the possible error probably can be neglected.

TABLE 1—INCOME AND CONSUMPTION OF FAMILIES 1957–58
(Yearly averages in I£)

	(1) Number of Families	(2) Current Income	(3) Windfall Income in 1957–58	(4) Consumption Expenditure	(5)* Adjusted Consumption Expenditure
Received windfall income in 1958–59	161	3,764	798	3,602	3,602
Did not receive windfall income in 1958–59	799	3,233	388	3,040	3,564

* The estimates for adjusted consumption expenditure excluding durables were 3,336 and 3,290, respectively.

computed their consumption expenditure in 1957–58. We made the same calculation for families who did not receive windfall income in 1958–59.

But as these two groups received different amounts of current and windfall income in 1957–58 we made a computation which allowed[7] for these differences. As column (5) in Table 1 shows, future windfall income did not affect current consumption.[8] In order to test this result in a different way we ran a regression of the form:

$$C_t = \beta_0 + \beta_1 Y_t + \beta_2 W_t + \beta_3 W_{t+1} + v_t, \quad (2)$$

where C_t is consumption expenditure in 1957–58; Y_t is current income in 1957–58; W_t is windfall income in 1957–58; and W_{t+1} is windfall income in 1958–59.

The results presented in Table 2 reveal that the value of the MPC out of future windfall income is extremely low, not different significantly from zero. These findings confirm our conclusion that consumers behaved as if they did not expect[9] any lump sum; these receipts in our data can therefore

TABLE 2—MARGINAL PROPENSITIES TO CONSUME OUT OF CURRENT AND WINDFALL INCOME

	MPC out of Current Income	MPC out of Windfall Income	MPC out of Future Windfall Income
Consumption-expenditure	0.7609 (±0.0173)	0.1881 (±0.0245)	−0.0038 (±0.0224)

[7] We multiplied the differences between these two groups in current and windfall income by their respective MPC and added this to 3040 thus receiving 3564.

[8] The same results were obtained for real estate purchases.

[9] We can interpret these results in another way: one could argue that although receipts were expected, consumers were not able to realize them. This interpretation, however, does not contradict our conclusion. What is important for us is the fact that these receipts did not affect consumption until the recipients got them.

be classified as windfall income not causing any bias in the estimate of MPC^w.

We now come to Bodkin's two principal suggestions about the low MPC^w in Kreinin's paper: "the restricted portion (80 per cent) of the restitution receipts might have been saved, by at least some recipients, in anticipation of a devaluation of the Israeli pound, as it was well known that the official exchange rates overvalued the currency." Concerning the second part of the receipts, Bodkin says that "there were economic pressures to defer consumption out of the unrestricted (1/5) portion of the restitution payments. With foreign exchange restrictions, an individual receipient would not have been able to obtain in the future unlimited foreign exchange for personal use. Consequently, he might have deferred consumption out of at least a fraction of this portion also."

We believe that Bodkin's second argument might be valid, but as it is relevant to only 20 per cent of the receipts it cannot explain the very large discrepancy between his and Kreinin's findings. But as regards to devaluation expectations, which affected the predominent share of the receipts, Bodkin ignored the fact that any Israeli could acquire various liquid assets linked to the foreign exchange rate, and thus hedge against devaluation. Consequently, we see no reason for such expectations to affect the ratio MPC^w/MPC^y.

We shall now attempt to refute Bodkin's conclusion about the magnitude of MPC^w, with the help of data from the 1963/64 Family Expenditure Survey and the 1958/59 reinterview Savings Survey.

Since the devaluation of February 1962, there has hardly been any advantage in holding restitution money in the form of liquid assets, so that there is no longer any reason for restitution recipients to defer final allocation of their lump-sum receipts[10] as far as concerns speculation about possible devaluation. If we are right in rejecting Bodkin's interpretation, we should thus obtain results similar to those in Kreinin's paper. In order to test this, we used the data of the 1963/64 Family Expenditure Survey and computed a regression of the form:

$$C = \gamma_0 + \gamma_1 Y + \gamma_2 N + \gamma_3 W + e, \tag{3}$$

where C is current consumption expenditure; Y is current income; N is family size; and W is restitution receipts.

The results were $\gamma_1 = 0.8397$ (± 0.0712) and $\gamma_3 = 0.2603$ (± 0.0744), confirming Kreinin's findings.

Table 3 presents additional evidence. It shows that in 1958–59 there was a time lag in the final allocation of restitution receipts, but the final allocation is not as indicated by Bodkin. In fact the time lag hardly affects the allocation of the restitution payments between consumption and savings. The time lag is important in final allocation of restitution receipts as regards the structure, but not the amount of savings. Of the 45 per cent of restitu-

[10] After the devaluation the price level continued to rise moderately by 5–6 per cent each year, but as far as we know there is no evidence that this has anything to do with the marginal propensity to consume out of any income.

TABLE 3—LUMP-SUM PERSONAL RESTITUTION PAYMENTS FROM GERMANY AND THEIR USE 1957-58 TO 1958-59
(Per cent of amounts received)*

Use of Funds	Restitution Payments Received in 1957-58		Restitution Payments Received in 1958-59
	Expenditure in 1957-58	Expenditure in 1958-59	Expenditure in 1958-59
Current consumption	17	4	18
Durable consumer goods	9	4	18
Real estate	20	25	40
Financial assets	45	−33†	22

* Percentage figures do not add up to 100 as not all uses were included in this table.
† Excluding accumulated interest.
Source: Quoted from Bank of Israel, *Annual Report 1959*, Jerusalem 1960, page 326, Table XVIII-4.

tion receipts used in the first year to acquire financial assets, 25 per cent was spent a year later for real estate, and 4 per cent each on durables and current consumption.

We believe that these results are such as to refute Bodkin's attempted reconciliation of the contradictory findings, and we shall now endeavor to show that the differences between Kreinin's and Bodkin's results stem from the disparity in the amounts of average windfall incomes in their data.

II. *Marginal Propensity to Consume out of Windfall Income* (MPC^w)

From the 1958/59 Saving Survey, which was a reinterview survey, we chose all families who received lump sums in at least one of the years. We then combined the data for both years and adjusted them for the rise in prices. In this way we got about 300 families.

These 300 recipients of lump sums were classified into five groups of w/y, in ascending[11] order.

As may be seen from Table 4, by using this classification we got approximately five equal current income (y) groups, which differed as regards the average windfall income (w). For each group we computed a regression of the form:

$$C = \delta_0 + \delta_1 Y + \delta_2 N + \delta_3 W + w. \tag{4}$$

The notation is as before. The result was a strongly decreasing coefficient of w, which means a decreasing marginal propensity to consume out of windfall income. This, we believe, accounts for the differences between Bodkin's and Kreinin's findings.

An unexpected result in this connection is the high value of MPC^w in Group I. We can offer no convincing explanation for this as yet and are

[11] Each group contains also the families of the lower groups, so that the groups are cumulative.

TABLE 4—MPC OUT OF CURRENT (y) AND WINDFALL (w) INCOME BY w/y GROUP

Group	Number of Families Cumulated	Annual Current Income per Family (I£)	Windfall Income per Family (I£)	$\frac{w}{y}$ %	MPC out of: Current Income	Family Size	Windfall Income
I	103	4,312	300	7	0.710 (±0.061)	127.4 (±124.0)	1.972 (±0.772)
II	151	4,449	617	14	0.746 (±0.050)	194.9 (±109.1)	0.583 (±0.276)
III	184	4,429	894	20	0.720 (±0.043)	193.2 (±104.6)	0.450 (±0.177)
IV	247	4,383	1,696	39	0.745 (±0.034)	174.1 (± 85.9)	0.244 (0.070)
V	297	4,243	2,830	66	0.751 (±0.034)	197.4 (± 95.9)	0.233 (±0.035)

trying to analyze the behavior of this group by using additional variables. It is possible that those who receive very small sums as windfall income spend them with little thought, thus behaving in a manner different from that which economic theory generally considers to be rational consumer behavior. However this is only conjecture. In any event the extremely high MPC^w (1.972)[12] in Group I does not invalidate our conclusions about the reconciliation of Bodkin's and Kreinin's results.

As current income per family and family size are almost the same in all five of the groups of Table 4, we can regard these groups as similar with respect to their permanent income, except for the lump-sum effect. If this is the case, the decreasing MPC^w implies a decreasing marginal propensity to consume out of permanent income, where permanent income Y_p is defined as: $Y_p = rT$ (r is interest rate, and T is total wealth). In our case, the windfall can be regarded as an increase in $T(w=\Delta T)$, and therefore a decreasing MPC out of W implies a decreasing MPC out of Y_p.

MICHAEL LANDSBERGER*

[12] This high value of MPC^w is not significantly different from unity.

* The author is a senior economist in the research department of the Bank of Israel. He is indebted to Dr. Nissan Liviatan of The Hebrew University, who read an earlier draft of this comment and offered many valuable comments, as well as to Dr. Ronald Bodkin for his helpful critical remarks. Responsibility for any possible errors and deficiencies lies of course with the author.

APPENDIX

The 1958/59 Saving Survey was a reinterview survey and covered about 1000 Jewish urban families. The original Saving Survey covered the period 1957–58 and embraces 3000 Jewish urban families.

The methods and definitions used in the Survey[13] are very similar to those in the Oxford Saving Surveys and in the U.S. Surveys of Consumer Finances.

The 1959/60 and 1963/64 Family Expenditure Surveys were carried out by the Central Bureau of Statistics[14] and covered each about 1600 urban wage and salary earning families.

The 1963/64 survey included also information about saving items.

[13] The surveys were carried out jointly by the Bank of Israel, Central Bureau of Statistics, Department of Economics of the Hebrew University, Falk Project for Economic Research in Israel, and Israel Institute of Applied and Social Research. The most important findings and definitions are presented in Bank of Israel *Bulletin No. 10* and Bank of Israel, *Annual Report 1959*, pp. 319–27.

[14] See "Family Expenditure Surveys (1950/51–1956/57–1959/60), Special Series No. 148 Central Bureau of Statistics, Jerusalem 1963. Results of the 1963/64 Survey will be published by the Central Bureau of Statistics.

REFERENCES

1. R. BODKIN, "Windfall Income and Consumption," *Am. Econ. Rev.*, Sept. 1959, *49*, 602–14.
2. ———, "Windfall Income and Consumption: Comment," *Am. Econ. Rev.*, June 1963, *53*, 445–47.
3. M. FRIEDMAN, *A Theory of the Consumption Function.* Princeton 1957.
4. ———, "The Concept of Horizon in the Permanent Income Hypothesis," in C. F. Christ, M. Friedman, *et al.*, ed., *Measurement in Economics—Studies in Mathematical Economics and Econometrics in Memory of Yehuda Grunfeld*, Standford 1963.
5. M. E. KREININ, "Windfall Income and Consumption—Additional Evidence," *Am. Econ. Rev.*, June 1961, *51*, 388–90.

Windfall Income and Consumption: Reply

Michael Landsberger is to be congratulated on a stimulating, insightful comment. Although I am not completely convinced that differences in the magnitude of the average one-time receipt (relative to average "regular" income) is the full explanation of the divergent results obtained by Kreinin [6] and me [2], nevertheless I believe that Landsberger's discussion throws some new light on these issues.

Landsberger's explanation of the different results found in the studies of Israeli restitution payments and American National Service Life Insurance (herein abbreviated NSLI) dividend receipts is based on the hypothesis that the marginal propensity to consume windfalls is not constant, but instead decreases as the size of the windfall increases. Hence one could argue that the same consumption relationship characterizes both groups of recipients of windfall income, the differences in the observed MPC's merely reflecting the position (in the two separate cases) of the typical receipient of windfall income on the common curvilinear relationship. Landsberger attempts to test this hypothesis, using Kreinin's and some addi-

tional data on Israeli recipients of restitution payments made by the West German government. It should also be possible to make a similar test of this hypothetical relationship on the basis of the selected sample of the NSLI dividend recipients analyzed in [2].

In this sample of 1414 veterans, the size of the dividend payment varied substantially—from as little as $60 to well over $1000. Accordingly, it was possible to break the sample into two subgroups which depend upon the size of the dividend received. The sample was divided into two subsamples in the following manner: all NSLI dividend recipients (in the original selected sample) who received dividend payments of $300 or less were put into Group A; the remainder (those whose dividend payments exceeded $300) were put into Group B. (Hence, unlike the groups studied in Landsberger's Table 4, these groups are mutually exclusive.) As was the case in the original analysis of these data, consumption behavior is analyzed for the two separate cases in which consumption is defined to include expenditures on durable goods (these results appear in Table 1) and the case where consumption is defined net of expenditure on durables (Table 2). Let C denote household consumption (either concept), y "regular" household disposable income (not including the receipts of NSLI dividend payment), and d the amount of the NSLI dividend payments received by the household in question. For both Group A and Group B, one can calculate regressions of the form:

$$C = \alpha_0 + \alpha_1 y + \alpha_2 d + u, \tag{1}$$

where the α's are parameters, and u represents a stochastic disturbance.[1] This was done, and the results appear in Tables 1 and 2. Although the columns of these tables are largely self-explanatory, it may be pointed out that the numbers in parentheses, which appear immediately beneath the reported regression coefficients, are the associated standard errors of these coefficients. Also, as is conventional, R^2 denotes the coefficient of multiple determination (uncorrected for degrees of freedom) of the respective regression.

It should be observed that there are substantial differences in the mean NSLI dividend payment d received by the two groups: Group B received, on the average, almost three times as large a payment as did Group A, and even relative to its somewhat larger mean "regular" income, Group B received more than $2\frac{1}{2}$ times as much of this one-time payment. The results of Tables 1 and 2 lend no credence to the hypothesis of a declining marginal propensity to consume windfall income. Taken at face value, the point estimates suggest that the marginal propensity to consume windfall income increases with the size of the windfall. Fortunately, the standard errors are large enough to swamp this theoretically embarrassing possibility,

[1] At present I think that a more sophisticated analysis is required in order to make a tighter test (than was done in [2]) of the proposition that the marginal propensity to consume transitory income (which is not the same thing as a windfall or windfall income) is zero. Roger Bird and I have attempted to carry out such a test in [1]. But for present purposes (examining the issue of a declining marginal propensity to consume windfall income), the less sophisticated nalysis is probably satisfactory.

TABLE 1—RESULTS OF THE SUBGROUP CONSUMPTION REGRESSIONS WHEN CONSUMPTION INCLUDES EXPENDITURES ON DURABLE GOODS

Group	Size of Group	Mean y	Mean d	Ratio of d to y (for the Means)	α_0	α_1	α_2	R^2
A	990	$3,699	$164	4.4%	$ 873	0.779 (0.022)	0.782 (0.445)	0.559
B	424	4,213	468	11.1	1,141	0.713 (0.027)	0.907 (0.303)	0.621

TABLE 2—RESULTS OF THE SUBGROUP CONSUMPTION REGRESSIONS WHEN CONSUMPTION EXCLUDES EXPENDITURES ON DURABLE GOODS

Group	Size of Group	Mean y	Mean d	Ratio of d to y (for the Means)	α_0	α_1	α_2	R^2
A	990	$3,699	$164	4.4%	$ 952	0.583 (0.016)	0.157 (0.324)	0.571
B	424	4,213	468	11.1	1,183	0.532 (0.021)	0.563 (0.236)	0.601

and the only justifiable conclusion from this particular test appears to be one of an insignificant difference in the coefficients of d for the two groups.

The conclusion that the marginal propensity to consume the windfall NSLI dividend payment does not decline with an increasing size of this receipt might merely reflect, for the previous test, the choice of the level of the windfall at which to break the selected veterans sample into subgroups. Accordingly, an alternative test of the possible nonconstancy of the MPC out of d can be tried. One can compute regressions of the form:

$$C = \beta_0 + \beta_1 y + \beta_2 d + \beta_3 d^2 + v, \tag{2}$$

where the β's are parameters and v denotes a stochastic disturbance. In this case, the marginal propensity to consume the windfall receipt d is given by:

$$\frac{\partial C}{\partial d} = \beta_2 + 2\beta_3 d. \tag{2}$$

TABLE 3—RESULTS OF REGRESSIONS OF THE FORM (2), 1,414 MEMBERS OF THE SELECTED VETERANS SAMPLE, ALTERNATIVE DEFINITIONS OF CONSUMPTION

Concept of Consumption Employed	β_0	β_1	β_2	β_3	R^2
Includes Expenditures on Durable Goods	$964	0.747 (0.017)	0.962 (0.443)	0.00001 (0.00051)	0.601
Excludes Expenditures on Durable Goods	969	0.560 (0.013)	0.645 (0.332)	0.0001 (0.0004)	0.602

A systematic tendency for the marginal propensity to consume the NSLI dividend to decrease with the size of this payment would be indicated by a (statistically significant) negative estimate of the parameter β_3. The results of two regressions (with alternative definitions of consumption), for the entire 1414 observations of the selected veterans sample, are presented in Table 3. As in Tables 1 and 2, the numbers in parentheses under the estimated regression coefficients are their associated standard errors.

As with Tables 1 and 2, there is no evidence to support the proposition of a declining marginal propensity to consume the windfall receipt. Again, the perverse effect is swamped by large standard errors. Parenthetically, it may be observed that the value of β_3 estimated for the case in which consumption is defined to include expenditure on durable goods seems as close to the hypothetical zero of the null hypothesis as one ever attains in an uncertain world. Nevertheless, these results should not be interpreted to imply that the apparent constancy of the MPC to consume the windfall would hold from zero to infinity. It seems reasonable to believe that this MPC would eventually decrease. However, such a decline appears to be rather slow (that is, it does not appear to take hold within the observed range of variation in the dividend receipts for the selected veterans sam-

ple). Accordingly, it seems questionable whether the differing results ol tained by Kreinin and Landsberger for Israeli restitution payments an myself for these data are wholly explicable in terms of the differing relativ magnitudes of the average one-time payment.

Leaving these questions aside, one can return to the larger issue of th comparability of the two sets of data. Landsberger attempts to answer m argument that restitution-payment recipients might display a great de gree of intrinsic thriftiness, especially with regard to one-time payment: by "controlling" on a group of Israeli recipients of "other windfall" (nor restitution payments) income. Such a "control" regression is, in my view largely beside the point. Israel is a country of refugees, and few indeed ar the nonnative-born citizens of this country who have not suffered fror some form of religious discrimination. One might well expect the same sens of thriftiness, caution, and orientation toward security to characterize mos of the citizens of this country, not only restitution-payment recipient: There are the larger questions as well of institutional differences among th two economies.[2] Thus, Landsberger notes that "after devaluation th [Israeli] price level continued to rise *moderately* by 5–6 per cent each year. (Emphasis added.) Such a rate of inflation would hardly be moderate i the United States. Another institutional difference, one mentioned b Kreinin in [6], is a differing availability of consumer goods on domesti markets. Such institutional differences suggest that one might expect t find some behavioral differences in the two economies, and that difference in the response to two differing kinds of one-time payments are hardly sur prising.[3]

Finally, it seems worth pointing out that Landsberger's results do no constitute a particularly strong confirmation of the permanent incom hypothesis, at least in the form in which Milton Friedman has cast i [4] [5]. As Landsberger notes, the declining marginal propensity to con sume windfall payments does imply a declining marginal propensity t consume permanent income, because in Friedman's recent statement of hi "horizon" model [5], the former payments are a constant fraction of th latter. Moreover, under the permanent income hypothesis, the failure c future restitution payments to influence current consumption is puzzling to say the least. For under this theory of consumer behavior, the consume might be expected to write up his wealth and to consume more immediately

[2] At the time [3] was written, I was not aware of the existence of domestic Israeli bond linked to the consumer price level or the foreign exchange rate. However, one can still argu that, even with the existence of these hedges, the easiest way to take advantage of an impend ing devaluation was simply to wait to convert the restricted portion of one's restitution pay ment. As for the postdevaluation experience, it can always be argued that the experience c one devaluation might reinforce the expectation of another one sometime in the future, pa ticularly if domestic prices were still rising more rapidly than those of one's trading partner:

[3] Moreover, one can argue that the two types of receipts are quite different in characte Thus, as was argued in [3], it is questionable whether the restitution payments were considere (by the recipients) as a genuine windfall, or merely as a partial compensation for past losse: It can further be argued that such a receipt is much less likely to be devoted to consumptio expenditures than a genuine windfall which does not merely offset (in part) past losses.

upon learning news of an unanticipated payment which will reach him in the near future.[4] In other words, the permanent income hypothesis would predict that the coefficients of W_t and W_{t+1} in Landsberger's equation (2), β_2 and β_3, respectively, would be almost equal,[5] provided that the recipient had been able to anticipate the future receipt, as we have argued. This should be true irrespective of the actual value of the marginal propensity to consume permanent income. Since these coefficients appear to differ significantly from each other, this result does not seem consistent with a permanent income framework.

In sum, I do not wish to deny that Landsberger's explanation has some relevance or to imply that the disposition of windfalls is unlikely to vary, depending upon the (relative) magnitude of these windfalls. I still suspect, however, that institutional conditions may have played a larger role in producing the divergent sets of results than Landsberger believes. This suspicion is strengthened by the fact that, for the selected sample of veterans who received NSLI dividend payments in 1950, there is no evidence of a declining marginal propensity to consume this windfall. In any event, Landsberger's results do not constitute a particularly strong confirmation of Friedman's permanent income hypothesis; in this respect, at least, his work appears to supplement, rather than contradict, the findings of [1] and [2].

RONALD G. BODKIN*

[4] There would seem to be only two ways in which a permanent income theorist could logically argue that this effect (increased immediate consumption) would not occur. One of these would be the case in which news of the payment and its actual receipt coincided, which was certainly not the case with the restitution payments, as these were generally the subject of lengthy negotiations. Secondly, the recipient could have taken an "I'll believe it when I see it" attitude. However for the 1957–58 survey year examined by Landsberger, this attitude could hardly have been widespread, as individuals scheduled to receive restitution payments in the 1958–59 survey year could observe that other individuals in the community were already receiving restitution payments from the German government.

[5] Actually, the predicted values of these two coefficients would be in the ratio of $1:1+r$, where r is the subjective rate of discount of the "typical" household of the society. Since r has been estimated to lie in the range between one-third (U. S. data) and one-fifth (Israeli data), the ratio between the predicted values of the coefficients would be between 3:4 and 5:6. The discrepancy between Landsberger's estimated coefficients still appears to be statistically significant, according to this interpretation, which is based upon the permanent income hypothesis.

* The author, who is associate professor of economics at the University of Western Ontario, wishes to thank Roger C. Bird for his very helpful comments without committing him to any errors of fact or analysis which may be present in this reply.

REFERENCES

1. R. C. BIRD AND R. G. BODKIN, "The National Service Life-Insurance Dividend of 1950 and Consumption: A Further Test of the 'Strict' Permanent Income Hypothesis," *Jour. Pol. Econ.*, Oct. 1965, *73*, 499–515.
2. R. G. BODKIN, "Windfall Income and Consumption," *Am. Econ. Rev.*, Sept. 1959, *49*, 602–14.

3. ———, "Windfall Income and Consumption: Comment," *Am. Econ Rev.*, June 1963, *53*, 445–47.
4. MILTON FRIEDMAN, *A Theory of the Consumption Function*. Princeton 1957.
5. ———, "Windfalls, the 'Horizon,' and Related Concepts in the Permanent Income Hypothesis," in C. F. Christ, M. Friedman, *et al.*, ed. *Measurement in Economics—Studies in Mathematical Economics and Econometrics in Memory of Yehuda Grunfeld*, Stanford 1963, pp. 3–28.
6. M. E. KREININ, "Windfall Income and Consumption—Additional Evidence," *Am. Econ. Rev.*, June 1961, *51*, 388–90.

International Factor Costs and Factor Use: Comment

In an excellent review article [2] Leontief questions the usefulness of the Minhas factor reversal study [4] because Minhas' estimates of the elasticity of substitution are very close to one, for it is well known that factor reversals cannot occur in a Cobb-Douglas world. Leontief alleges that likely errors in measuring the wage rate lead to a downward bias in the least-squares estimates of the elasticity of substitution. He therefore concludes that the true values are unity, which implies that we are in a Cobb-Douglas world and the recent work on the generalized constant elasticity of substitution production function is of questionable usefulness. Using a common errors in variables framework, this note will show that the bias is towards, and not away from, unity as Leontief suggests.

Leontief says:

> Interested in demonstrating the practical importance of crossovers, he [Minhas] naturally rejected the Cobb-Douglas function . . . and reached out for a formula capable of showing their existence. . . . This suggestion would appear to be even more plausible if in applying the least-squares method to estimate the slopes, b_i [which are the estimates of the elasticity of substitution] in the log linear equation · · · [$\log(V/L)_{ij}=\log a_i+b_i \log w_{ij}$],[1] Minhas had not proceeded on the assumption that only the variable $(V/L)_i$ is subject to random errors, while the variable w_i is not. Had he instead, in fitting the slopes of these regression lines, allowed also for errors affecting the observed magnitudes of w_i, all estimated elasticities would necessarily turn out to be larger, since in 23 out of the 24 industries examined by him, the magnitudes of the b_i's . . . turn out to be less—although in most instances only slightly less—than 1. This means that their values would be still closer to 1—the constant elasticity of the Cobb-Douglas function [2, p. 343].

In order to understand Leontief's error, first consider the method used for measuring the wage rate.[2] It is measured by dividing a given industry's

[1] $(V/L)_{ij}$ is the value added-labor ratio for industry i in country j, and w_{ij} is the wage rate of industry i in country j.

[2] This analysis is a modification of Lucas [3, pp. 26–30].

total wage bill by the number of man-hours employed by that industry, i.e., $w_{ij} = W_{ij}/L_{ij}$; where W_{ij} is industry i's total wage bill. Since W_{ij} comes from accounting records, it can reasonably be assumed to be measured without error. Therefore, the error must be in the measurement of L_{ij}. The recorded number of man-hours is correct, but the quality of labor is probably heterogeneous over the sample. In other words, an hour of labor services in one part of the world is not the same commodity as an hour of labor services in another part of the world.

To find the bias arising from this type of error, suppose that L_{ij}, the measured labor input, is only a portion of the true (quality-corrected) labor input. Let $L_{ij} = L^*_{ij}/u_{ij}$, where L^*_{ij} is the true value and u_{ij} the error.

If the true wage rate is w^*_{ij}, it will equal

$$\frac{W_{ij}}{L^*_{ij}} = \frac{W_{ij}}{L_{ij}} \Big/ u_{ij},$$

which implies that

$$\log \overset{*}{w}_{ij} = \log \frac{W_{ij}}{L_{ij}} - q_{ij} = x_{ij} - q_{ij} = \overset{*}{x}_{ij},$$

where

$$q_{ij} = \log u_{ij},\ x_{ij} = \log W_{ij}/L_{ij} \quad \text{and} \quad \overset{*}{x}_{ij} = \log \overset{*}{w}_{ij}.$$

By the same reasoning, the true output-labor ratio can be shown to be

$$\frac{V_{ij}}{L_{ij}} = \left(\frac{V_{ij}}{L_{ij}}\right) \frac{1}{u_{ij}},$$

which implies that

$$\log \frac{V_{ij}}{L^*_{ij}} = \log \frac{V_{ij}}{L_{ij}} - q_{ij} = y_{ij} - q_{ij} = \overset{*}{y}_{ij},$$

$$\text{where} \quad y_{ij} = \log \frac{V_{ij}}{L_{ij}} \quad \text{and} \quad \overset{*}{y}_{ij} = \log \frac{V_{ij}}{L^*_{ij}}\,.$$

Given these considerations, the "true" specification for estimating σ_i would be

$$y_{ij} = a + \sigma_i x_{ij} + (1 - \sigma_i) q_{ij} + \eta_{ij}. \tag{1}$$

This, of course, means that the previous specification

$$y_{ij} = \hat{a} + \hat{\sigma}_i x_{ij} + \epsilon_{ij} \tag{2}$$

is wrong (η_{ij} and ϵ_{ij} are the errors).

If σ_t were estimated by least-squares with the commonly used (2) specification, the estimating equation would be[3]

$$(3)\quad S = \frac{\Sigma(x-\bar{x})(y-\bar{y})}{\Sigma(x-\bar{x})^2} = \frac{\Sigma(x-\bar{x})[\sigma x + (1-\sigma)q - \sigma\bar{x} - (1-\sigma)\bar{q}]}{\Sigma(x-\bar{x})^2}$$

$$= \sigma + (1-\sigma)\frac{\Sigma(x-\bar{x})(q-\bar{q})}{\Sigma(x-\bar{x})^2}.$$

Since $x = x^* + q$ and $\bar{x} = \bar{x}^* + \bar{q}$,

$$(4)\qquad \text{plim}\,(S) = \sigma + (1-\sigma)\,\text{plim}\,\frac{\Sigma(x^* + q - \bar{x}^* - \bar{q})(q-\bar{q})}{\Sigma(x-\bar{x})^2}$$

$$= \sigma + (1-\sigma)\frac{\text{Cov}\,(x^*q) + V(q)}{V(x)}.$$

If x^* and q are assumed to be independent (a reasonable assumption since x^* is nonstochastic), the bias will be

$$(5)\qquad (1-\sigma)\left[\frac{V(q)}{V(x)}\right],$$

which is positive for $\sigma < 1$ and zero for $\sigma = 1$. If, however, x^* and q are not independent, the bias will be

$$(5')\qquad (1-\sigma)\left[\frac{V(q)}{V(x)} + \frac{\text{Cov}\,(x^*q)}{V(x)}\right].$$

For a linear homogeneous production function x^*, the efficiency-corrected wage rate depends on the efficiency-corrected capital-labor ratio. If technical progress across countries is Hicks neutral (as Arrow, Chenery, Minhas, and Solow [1] suggest it to be), the efficiency-corrected capital-labor ratio is the same as the uncorrected ratio. Because the most efficient labor force is usually found in the countries with largest capital-labor ratios, we can expect a positive correlation between x^* and q (the quality index). Therefore, the covariance between x^* and q is positive, which only implies a stronger upward bias.[4]

Therefore, Leontief is incorrect about the direction of the bias due to errors in the variable w. Rather than expecting the true values of σ to be smaller than the estimated values, we would expect them to be larger. The existence of the bias does not detract from the usefulness of Minhas' work; it gives it, instead, greater significance.

PERRY SHAPIRO*

[3] Subscripts are deleted from all variables.

[4] The point about nonzero covariance was suggested by the referee.

* The author is a graduate student in economics at the University of California, Berkeley.

REFERENCES

1. K. J. ARROW, H. B. CHENERY, B. S. MINHAS, AND R. M. SOLOW, "Capital-Labor Substitution and Economic Efficiency," *Rev. Econ. Stat.*, Aug. 1961, *43*, 225–50.
2. W. LEONTIEF, "International Factor Costs and Factor Use," *Am. Econ. Rev.*, June 1964, *54*, 335–45.
3. R. E. LUCUS, *Substitution Between Labor and Capital in U. S. Manufacturing; 1929–1958*. Unpublished doctoral dissertation, Univ. Chicago, 1963.
4. B. S. MINHAS, *An International Comparison of Factor Costs and Factor Uses*. Amsterdam 1963.

BOOK REVIEWS

General Economics; Methodology

Economics: Analysis and Policy. By NORMAN F. KEISER. New York: John Wiley and Sons, Inc., 1965. Pp. xviii, 720. $8.50.

The chief virtues of this recent entry in the textbook sweepstakes are its wide array of useful information, its effective use of historical and institutional material, its extensive documentation, and its ideological commitment. Its major faults are its inability to present economics as an engine of analysis, together with its graceless and pedestrian prose.

To detail accomplishments first, Professor Keiser's book presents a varied menu of recent and pertinent information on growth rates (e.g., Denison's study), corporate size and concentration, income distribution, union organization, and other staples of the elementary text. His discussion of federal, state, and local expenditures (Chap. 16) seems especially well done and his examination of "the methods of monopoly" (Chap. 25) offers a particularly helpful summary of the available information even though we need not accept his pronouncements (pp. 136, 586) about the prophetic significance of Berle and Means. As exemplified by the chapter on "cultural variations in the solution of basic economic problems" the author covers, though he does not always command, the territory of history and anthropology; among the many readings interspersed throughout the text is a hair-curling account of child labor in the mines and mills (on pp. 39-43 of that chapter) that shows the author's concern with the evils of industrial civilization.

As another entry on the positive side of the ledger, the footnote citations are far more extensive than the average elementary text and, at the cost of making the format more formidable, enhance the reference value of the book for both the ambitious student and the indolent instructor. Example: half a dozen items at page 508 dealing with changes in labor's share of the national dividend. A rough count of the index reveals from one to 14 citations apiece for about 360 authors—and 15 for Norman F. Keiser (plus 4 more that escaped the indexer).

On the debit side, the author plows through 150 pages of price theory—Chapters 3, 4, 17 through 21, and parts of 25—where his heart was clearly not in his work. The analysis is perfunctory, the exposition slipshod, the examples lifeless, and the significance for the rest of the book dubious. The relation between the firm's cost curves and the industry supply schedule is never properly explored. This unfortunate omission occurs because the author makes a horseback survey of the industry in Chapters 3 and 4, develops the marginal analysis of the competitive firm in 18 and 19, but neglects to return to the industry for the pick and shovel work that then becomes appropriate. Almost completely ignored is the determination of equilibrium for the economy as a whole. The instructor who uses this treatise for its institutional material and its

macroeconomic analysis ought to piece it out with a good paperback primer on price theory.

The style, while honest and unpretentious, appears graceless and garrulous. Consider this typical example where the second sentence with its pair of "one in which's" and its unbalanced ending indicates both careless writing and absent-minded editing. "The principle of diminishing returns is directly related to cost. Typically, the pattern for a firm is one in which (starting from zero production) the addition of more workers to a fixed amount of capital is one in which increasing marginal returns initially results, then diminishing and finally negative marginal returns set in. The addition of workers simply means that labor is now operating machines that were previously lying idle" (p. 426).

Since form and substance are inseparable, however, the author's expository problems arise from his conceptual difficulties. His failure to see and state that indivisibility of the fixed factor represents a necessary condition for increasing returns in the initial phase of the production function invites the tiresome stylistic maneuvers of the passage quoted above. Creating an elementary textbook is an exacting art form which ought not to be attempted by academicians who refuse to take their writing seriously.

The book has a clear and distinct point of view which emerges in the populist rhetoric that spices up its bread and butter style when the author turns his attention to the "scandalous practices" of corporations (p. 130), the "neurotic drive to please their superiors" on the part of corporate executives (p. 136), "the mansions of the wealthy" that escape their fair share of property taxes (p. 395), our "basically probusiness and antilabor" ideology (p. 522), and "the infamous Nate Shefferman, union buster par excellence" (p. 547). Since Samuelson has become a man for all seasons, this new text which speaks for the winter of our discontent offers a special commodity for a particular sector of the market.

PROCTER THOMSON

Claremont Men's College and
Claremont Graduate School

Basic Economic Principles. By ROBERT A. LYNN. New York: McGraw-Hill Book Co., 1965. Pp. viii, 428. $6.75.

According to the book jacket, this text is designed to be used for a "one-semester introductory course at the beginning undergraduate level." The premise behind *Basic Economic Principles* seems to be that existing texts are too difficult but not too comprehensive for this purpose. It is possible to simplify by greatly restricting the number of concepts or ideas the student is asked to comprehend and then working over this restricted list thoroughly. Robert Lynn, however, has attempted to retain the broad coverage of the typical elementary text while reducing the complexity of the included analytical material. To quote the book jacket once more, "Freshmen and sophomores using this text can be introduced to macro- and micro-economics without wading through a sea of language and symbol." Unfortunately, in avoiding the sea they will end up in a bog.

To retain comprehensive coverage while keeping the analysis "simple," Lynn is forced to give only the most cursory treatment to many important topics. To avoid taxing the student's ability to follow a logical chain of reasoning or to grasp abstractions, familiar concepts are reworded or stated in a nonrigorous fashion, e.g. "Because of ability to pay, diminishing utility, and substitution, we have seen that a price cut can produce a stretch in the quantity demanded. The amount of this stretch is called the *elasticity* of demand" (p. 225). Even the style of writing suffers from the attempt to "water-down" economic analysis to the level presumed digestable by beginning college students.

The text is divided into eight parts which are further subdivided into chapters. After the usual introductory material, Lynn takes up national income analysis and the monetary system, international economics, supply and demand, competitive and monopolistic markets, and factor pricing in that order. The book concludes with a discussion of current economic problems and a glossary of key concepts.

The several parts of the text vary greatly in merit. The early chapters on business and labor organizations are well done, but the sections following, on national income determination and the monetary system, are very weak. Lynn presents the usual conclusions from the simple income model without presenting anything resembling the model, and the student pursuing "why" questions is bound to become confused. Except for a good exposition of the deposit-creation process, his treatment of the monetary system is so brief as to be misleading. Both these sections and the following discussion of international economics suffer from the fact the student has not yet been introduced to the concepts of supply and demand. However, once these concepts have been introduced, things improve materially. The treatment of competitive markets and the various degrees of monopoly is fully satisfactory at the intended level, providing a balanced evaluation of the issues and many useful insights into practical problems. But for a confused analysis of interest rate determination, the discussion of factor pricing would be of the same quality. Certainly the strength of this text lies in the sections concerned with microeconomics. This strength results from the use of the usual graphs and analytical tools of price theory. The presentation is at once both more precise and more easily understood. However, it does not differentiate Lynn's product.

The teaching devices (summary, learning key, questions for review and discussion) at the end of the chapters emphasize definitions and memory work rather than applications of the analysis. They might be appropriate for secondary school students but could be seriously questioned for use at the college level. For example, the learning key consists of a "fill-in-the-blanks" question set for which the first two letters of the correct word are provided. Similarly, one might question the complete lack of suggested follow-up or supplementary reading and the almost complete absence of supporting footnotes for the many factual statements in the text. There are numerous graphs but often they are not discussed in the text. Interested students deserve better!

A conscientious reviewer would have to note many other differences with the statements made, the definitions used, and the space allotted to various topics in *Basic Economic Principles*. In particular, the definitions of terms or

concepts in the text and glossary may surprise many economists (e.g., "Assets and Liabilities are always equal and thus balance," p. 387). However, the difficulties Lynn has all seem to stem from the attempt to bring economic theory down to some assumed "understandable" level— a difficult task under the best of circumstances.

Only those readers who share Lynn's appraisal of the need for a "watered-down" version of the standard elementary text are likely to find his book useful. They will have to take care, especially in the macroeconomic section, in building on the blocks provided, but all the topics are there for a starter. Others, sharing the reviewer's opinion that a more restricted, more intensive approach will offer greater classroom dividends, will find selecting assignments from existing texts designed for the usual two-semester sequence better suited to their purposes than the book under review.

BURNHAM O. CAMPBELL

University of California, Los Angeles

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Modern Capital Theory. By DONALD DEWEY. New York: Columbia University Press, 1965. Pp. x, 238. $6.50.

The object of this book is to present an intelligible but rigorous restatement of the theory of capital. Professor Dewey serves clear warning that he is concerned only with the real theory of capital; he is not interested in "the monetary superstructure of capital theory that began in the 1920's, reached staggering proportions in the 1930's and 1940's, and still warps the teaching of economics in many places" (p.v.). His is the theory of Irving Fisher, in which capital productivity and the "taste" for investment jointly determine the marginal productivity of investment, the rates of interest on loan contracts, the rate of capital accumulation, the rate of income growth, the value of capital assests, and the division of income between investment and consumption.

Dewey concentrates almost exclusively upon the fundamentals of capital theory and clings "tenaciously to the truth that every proposition about income is also a proposition about capital and, conversely, that every proposition about capital is a proposition about income." This is truly the world of Fisher and Knight, and not the least of Dewey's contributions is to untangle some of the latter's cumbersome prose. There is but one factor of production, namely capital, which is defined as *anything* that yields a flow of services over time. Thus income is the surplus of services obtained over those necessary to maintain. If this reminds the reader of Knight's remarkable piece of vegetation, the Crusonia plant, it should. Indeed, the Crusonia model is the heart of the book.

Dewey achieves a great deal with the Crusonia model; to me, more than Knight did. He discusses Knight's case, constant returns to investment. But he introduces the case of diminishing returns as well by distinguishing between "live" Crusonia, which reproduces, and "dead" Crusonia, which can only be consumed. In the latter case, the real capital stock and the value of

the capital stock differ because a unit of live Crusonia must exchange for more than one unit of dead Crusonia. The system must have a *numéraire*; and one must distinguish between the rate of interest (marginal productivity of investment) and the natural rate of growth of the real capital stock.

Given the model and the framework of discussion, the object of the book is achieved with clarity and simplicity. The framework is established in Chapters 1-3 and the basic elements of the model are presented in Chapters 4-5. Chapter 6, a bare fourteen pages, contains an analysis of equilibrium in capital theory; and it is a classic piece of exposition. By means of two simple graphs, Dewey explains all the fundamentals of capital theory. Chapter 7 contains a more detailed discussion of returns to investment, while Chapters 10-11 are devoted to the roles of risk and money in the theory of interest. Finally, in Chapter 12 Dewey clearly and logically disposes of some capital curiosa that have persisted for many years.

Ignoring a few trivial or nuisance errors, this book is subject to criticism along two lines. The first involves an error of commission. More specifically, the professional reader should skip, and the student be absolutely forbidden to read, Chapters 8-9. Chapter 8 contains a discussion of the theories of production and distribution in a two-factor model; but one factor (land) is absolutely fixed. In Chapter 9 a two-variable input model, with a Cobb-Douglas function in which both coefficients are one-half, is presented. Nothing is gained in either chapter; and unless one keeps Dewey's *basic* model in mind, a good deal may be lost through misunderstanding.

The second criticism concerns a matter of omission; hence it may not be a valid criticism at all. Nonetheless, the book is entitled *modern* capital theory; and the author specifically states that he has fashioned a complete theory of capital "from the materials willed to us by our predecessors from Thomas Aquinas to Robert Solow. . . ." To an extent this is true; one sees the names of Samuelson and Solow, Ramsey and Robinson, and even others. But more fundamentally, the Crusonia model cannot capture the essence of the recent advances in capital theory. Perhaps this is just to say that Dewey has concentrated upon the fundamentals of capital theory. Yet whichever side of the Atlantic one prefers, one is likely to belive that there have been fundamental developments in *modern* capital theory since the time of Fisher.

C. E. FERGUSON

Duke University

Economic Growth and Structure: Selected Essays. By SIMON KUZNETS. New York: W. W. Norton & Co., 1965. Pp. viii, 378. $7.50.

This book consists of twelve reprinted essays on economic growth from Kuznets' cautious and independent pen. The essays carefully underline the precariousness of the basic data from which they grow, and they are careful in stating what is not known; they are often speculative and suggestive. The central interest lies in the generalizations, rather than in the data—in contrast with the nine statistical papers Kuznets has published in *Economic Development and Cultural Change*.

Five of the essays are on growth theory, population changes, and their recent patterns; three on industrialization, the takeoff doctrine, and agriculture; two on inequality and growth; and two on U.S. growth and its long cycles. They were published originally from 1954 to 1964 in various places, ten in English, two previously only in French.

Many of Kuznets' contributions have become standard items in the literature of growth. Over a span of ten years, it is likely one would sometimes change his mind. Kuznets recants on only two matters. One of them is that he is not so sure now as in 1954 that retardation of per capita income growth rates is "observable and immanent" (pp. 20-23).

He is doubtful about the usefulness of current explicit theories of economic growth. He even stated (in 1955) that no "significant theoretical work" on the theory of economic growth broadly defined has been done in the hundred years since the mid-nineteenth century (pp. 4, 5), except for attempts to revive Marxist theory. I would argue in partial contrast that major progress has been achieved in the past 15 years which need not give place to anything done in the nineteenth century.

Economics as such is downgraded in the Kuznets vision of economic growth. He accepts what has come to be orthodox: "The transformation of an underdeveloped into a developed country is not merely the mechanical addition of a stock of physical capital; it is a thoroughgoing revolution in the patterns of life and a cardinal change in the relative power and position of various groups in the population" (p. 30).

Accumulation of "empirical and tested knowledge" is seen as the true basis of production and population growth during the past two centuries (p. 60), and so there is a central need for a theory of the production of knowledge. Most theories of growth underemphasize political and social institutions; only oversimplication yields determinant answers (pp. 69, 79). Free formulations and reformulations of testable theories accumulate perceptions and undermine dogmatic theories that claim universal validity (p. 81).

The application within nations of the growing international stock of useful knowledge is most unequal, due largely to "differences in historical background and antecedents" (p. 114). The use of force in international relations in the long run accelerates this spread of modern economic growth (pp. 116-18)—but the apparently inconsistent theorem is also presented that major wars depress growth in participating countries (pp. 54-57). Population growth has both positive and negative effects on income growth, the latter mainly relevant to low-income countries (pp. 137 ff.). The physical misery of the world's poor is diminishing, as their per capita incomes increase a bit; but their "political misery," the tension created by consciously falling farther behind the rising income levels of the high-income countries and by consciously greater technical lag, has increased (p. 174). Per capita incomes in present low-income countries are "much lower" than those of the present developed countries (save Japan) just before they began to industrialize (above $200 in 1952-54 prices, as compared with below $100 now in much of Africa and Asia) (pp. 117-18, 257-87). Underdeveloped countries have had

"entirely different beginnings" from those of present high-income countries (with qualification for Japan), and there are disadvantages, as well as advantages, accruing from a late start (pp. 176-93).

In a closed economy increasing productivity in agriculture is a requisite for industrialization; and desirable, despite the example of England in the nineteenth century, in an open economy. Growing efficiency in transport, communication, and warehousing is also crucial (pp. 197-99).

Rostow's takeoff concept is put through the Kuznets mill: it comes out discredited on statistical and logical grounds (pp. 213-25). In a number of countries, the ratio of net domestic capital formation to income was substantially higher than Rostow's "say 5 per cent or less" at the beginning of takeoff periods; and in no case among 12 countries does the ratio approach double its initial size during dated "takeoffs." "I can only conclude that the available evidence lends no support to his suggestions" (p. 227). There are other objections (p. 232), both to the sequence of stages and to the notion of a distinct takeoff stage.

On size distribution of income, Kuznets finds from scanty evidence somewhat greater inequality in less developed countries than in high-income ones. As growth occurs, dynamic changes check and in fact reverse the trends toward greater concentration resulting from cumulating past savings.

U.S. history combines a retardation in population and work-force growth, with acceleration in growth per capita and per worker. Three long swings in population and related variables are conspicuous in U.S. data since 1870.

The kind of inquiry here presented—induction from statistical and historical materials over a long period and many areas, leading by both cautious and imaginative reasoning to economic-social-political generalizing—seems a thoroughly fruitful approach toward understanding economic growth. These essays are most significant, not for the questions they answer, but for their purpose "to pose questions and advance suggestions." The data, when properly examined, says Kuznets, "always reveal findings that call for explanations that are not at hand."

THEODORE MORGAN

University of Wisconsin

Theory of the Firm: Resource Allocation in a Market Economy. By KALMAN J. COHEN AND RICHARD M. CYERT. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. xx, 406. $7.95.

This book is an important addition to the rapidly growing stock of intermediate price-theory texts. It is a careful and comprehensive exposition, showing clear evidence of careful thought and hard work on the part of the authors.

Despite its title, the book covers all the major topics in price theory. In addition to extensive treatment of the firm, there are chapters on consumer demand, market equilibrium, general equilibrium analysis, and welfare economics. The authors' primary interest, however, is clearly the firm, and they give more weight to that subject than is common in price-theory texts. Indeed, chapters on other subjects give the impression of being more routine and me-

chanical than those on the firm. The chapter on general equilibrium, for example, hardly gets beyond matters of notation and the counting of equations and unknowns.

Part II, which is the core of the book, contains chapters on all the standard topics in the theory of the firm. Six chapters on competitive markets are followed by chapters on monopoly, monopolistic competition, duopoly and oligopoly, and imperfections in factor markets. The authors' negative appraisal (in my opinion justified) of the value of Chamberlin's model of monopolistic competition makes one wonder why they include a whole chapter on the subject in an already large book. The chapter on duopoly and oligopoly is especially well done and sets the stage for Part III of the text, "New Approaches to the Theory of the Firm."

The first chapter in Part III is an excellent one on decision-making in uncertain situations. There follow two chapters on "behavioral" theories of the firm. These chapters borrow heavily from well-known research on the subject by the authors and others at Carnegie Institute of Technology. Here, the going will be pretty heavy, at least for students not raised in the Carnegie tradition.

This is a long book. I find it hard to believe that many sophomores could fight their way through it in one semester. It is also a difficult book. Calculus is used freely, including partial derivatives, integration, and constrained maxima. Clearly, no one should adopt this text for students who have not had a good year-course in calculus. Even then, many students will need special help in places.

Two final comments: First, empirical material is used to illustrate almost every major theoretical topic. This is an important improvement over almost all existing texts. Nevertheless, the empirical sections give the impression of having been tacked on for the most part. We have yet to see the first price-theory text that will show how good theory and careful quantitative analysis can be used to illuminate the real world. Second, a word on the choice of topics. It is useful to have the material on behavioral approaches available in textbook form. But in a text written at Carnegie, it would also be nice to have some material on managerial economics. Chapters on programming and inventory theory would be natural candidates. My own preference would have been for a chapter on at least one of these topics instead of the second chapter on behavioral models.

EDWIN S. MILLS

The Johns Hopkins University

An Introduction to Microeconomic Theory. By WILLIAM I. DAVISSON AND JOHN G. RANLETT. New York: Harcourt, Brace & World, Inc., 1965. Pp. vii, 226. Paper, $2.45.

The authors of *An Introduction to Microeconomic Theory* had as their purpose in writing this text the development of "an approach to microeconomic theory that would be uniquely adapted to the introductory course in economics."

The text is divided into three parts. Part I, entitled "The Purpose and

Method of Microeconomics," defines microeconomics, outlines "the theoretical approach," and discusses briefly the basic concepts of price theory. In Part II, "The Tools of Analysis"—encompassing production theory, costs of production, and the theory of demand—are explained at some length. Finally, in Part III, "Market Structures," the authors detail the market characteristics of perfect competition, monopoly, and oligopoly; describe specific industry examples of each of these three market structures; and examine the theory of these three structures, as well as that of monopolistic competition. The last chapter in Part III is devoted to a consideration of the allocation of resources and the distribution of income. The book also contains a glossary of the economic terms used by the authors.

The authors have done a workmanlike job of presenting the essence of product pricing theory. The question arises, though, as to whether theory coverage which is little more than essence alone is sufficient for an introductory text. At the principles level, it may be argued that the maximum amount of elaboration and illustration consistent with the time span available for the coverage of the principles course is desirable. Further, only one chapter is devoted to factor pricing. The theoretical portion of this particular chapter is limited to a consideration of the functional distribution of income as explained by the marginal productivity theory.

A step forward in the authoring of introductory texts is accomplished by the omission of such topics as the history of American labor unions and the structure of business organization. Valuable though information about subject areas such as these is, the classroom coverage of them often means that the theory itself is covered in less depth. The graphical presentation used by William Davisson and John Ranlett is in black and white only, but it should be singled out for its clarity and usefulness as a complement to the verbal portion of the theoretical analysis.

In the Preface, the authors state "that there is a need for an elementary text that can provide an analysis of actual market situations as well as an explanation of the premises on which the logic of microeconomic theory is based. Too often the beginning student is left with the feeling that although economic theory may be interesting it has relatively little to do with practical aspects of the economic society in which we live. With this in mind we have designed this text specifically to demonstrate the meaningful relationships between microeconomic theory and the practical realities of the American business world." The approach used to interrelate the theoretical and the real is the presentation of separate "market structure" chapters for perfect competition, monopoly, and oligopoly in which the market characteristics of each of their market structures are outlined; then, a specific industry, such as trucking for perfect competition, is described for the purpose of illustrating each of the market structures under consideration. Each of the market-structure chapters is followed by a "market theory" chapter in which perfect competition, monopoly, and imperfect competition (encompassing both oligopoly and monopolistic competition) are examined in theoretical terms.

Swayed by his own biases and preconceptions, one may question the value of such a "design" for the principles course. In any case, it seems that the au-

thors have devoted far too much space, at least relatively speaking, to the description of the specific industries which are used to illustrate the market structures. Approximately one-fifth of *An Introduction to Microeconomic Theory* is directed to a consideration of such things as the historical development and recent production statistics of these industries. Generally speaking, this descriptive material does not go as far in providing the desired link between theory and reality as would have been provided by an examination of such practical matters as price and output determination in the industries under consideration. Furthermore, it seem as if it would have been more logical to present the theory chapters before, rather than after, the structure chapters.

A final point, which in no way is intended as a criticism of the contents of *An Introduction to Microeconomic Theory,* relates to the practical consideration of the present or future availability of a macroeconomic theory counterpart to the book under review. Those selecting texts for two-term principles courses no doubt would make the availability of such a counterpart one of the several criteria to be used in the selection process if they were considering the adoption of *An Introduction to Microeconomic Theory.*

RICHARD E. NEEL

University of South Florida

Theory of Economic Statics. By MICHAEL J. BRENNAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. 535. $8.95.

As the fundamental purpose of this book, Michael Brennan sets out to stress the use of theory as a tool of analysis and to avoid having the students come away with a knowledge of the "buzz words and the fragments" but no knowledge of "how they fit together." I am entirely in sympathy with this purpose and would certainly strongly recommend any intermediate theory book which accomplishes this feat. Unfortunately, the set of all such intermediate theory books remains empty. This is not a condemnation of Brennan, since he has written an intermediate theory book that is of the first rank in its coverage of the material and is among the best in readability.

The book itself is a bit unusual in that it includes 20 chapters on microeconomics and then has a nine-chapter section on macroeconomics. In addition, he has included a three-chapter section on the how, what, and why of economics: the first two chapters essentially contain a discussion paralleling Knight's, *The Economic Organization,* and the last contains a brief introduction into methodology. His treatment of consumer demand theory appears to be the standard treatment of a text at this level, except that no consideration is given to the classical utility approach (cardinal utility). On this point I have a feeling that many economists seem to believe that because cardinality of utility is not necessary to derive the theorems of demand theory it cannot exist. Or put differently, because a given set of demand functions for an individual consumer is not integrable into a unique utility function does not imply that the consumer cannot possess a unique utility function. In spite of this omission the discussion is very complete and assumes little or no prior knowledge on the part of the student. This is a distinct advantage, since

many students either have understood little about economics from their principles courses or have forgotten what little they did understand.

The production side is again handled in the standard way and again a very thorough and well-written job is done. The treatment of supply functions is adequate but certainly no better than, say, Leftwich. The discussion of long-run equilibrium for monopoly (as in all other texts at this level that I am familiar with) handles the scale problem in a peculiar fashion. The point is made that a monopolist may not operate an optimum-scale plant (by this is meant that he may not operate at the minimum point on his long-run average cost curve) and that a competitive firm would, and thus the implication is given that monopoly may be inefficient for this reason. However, since the monopolist is the industry, any comparison of his position of long-run equilibrium must be made with respect to the long-run equilibrium of a competitive industry and not a competitive firm. Since there is no presumption that a competitive industry will operate at the minimum point on its long-run average cost curve, this should not be required of the monopolist.

Brennan's book offers more than the usual intermediate text on the problem of the pricing of resources. In addition, he has a fine chapter on applications of the theory of resource pricing that should stimulate thinking and discussion on the part of the students. As pointed out above, the final part of the book contains a discussion of macroeconomic theory where the distinguishing characteristic is that it is a clear, concise statement of the level of macroeconomics usually contained in intermediate textbooks. I'm not sure why this macro section is included, since the book contains much too much material for a one-semester course covering both microeconomics and macroeconomics, and for a strictly macro course this section is too brief to be used as a text.

In conclusion, this is a very well-written book with quite extensive coverage of the microeconomic material. Some people might prefer a more mathematical treatment (Brennan relegates almost all mathematics to a brief mathematical appendix at the close of the book). However, for those who feel that understanding of economic theory is more likely to occur from a combination of verbal exposition and graphical manipulation, this book will be very adequate.

THOMAS R. SAVING

Michigan State University

Price Theory in a Changing Economy. By HENRY GRAYSON. New York: Macmillan Co.; London: Collier-Macmillan, Ltd., 1965. Pp. xxi, 247. $5.95.

It was said of the Bourbons at their restoration to the French throne after the Revolution that they had remembered everything and learned nothing. This micro-theory textbook, aimed, its preface claims, at both the graduate and undergraduate market, mentions everything and teaches nothing. This is its major, although by no means only, flaw, and we shall deal with it first.

There is frequently no apparent connection between the formulas and graphs on the one hand, and the text on the other. In the very first technical discussion in the book, a formula is given for arc elasticity which is totally unexplained (p. 7). Not even the notation in the formula is defined. Someone who has been through it all many times and who has some confidence in his

ability in manipulating formulas may guess what the symbols mean and work back to find the connection between the formula and the words, but students will probably be encouraged by the author's tactics to take the formula on faith and memorize it. This is not an isolated case. Some of the graphs are explained in so cursory a fashion that it is impossible to follow the discussion, where there is one.

The book abounds in absurdities and infelicities such as, "When elasticity of supply is under discussion, we always assume that the supply curve slopes up and to the right. Otherwise, measurement of elasticity would be meaningless" (pp. 7-8).

Pseudo-empirical observations of doubtful validity, which supposedly illustrate the theory, present a divertissement from the innumerable diagrams. A short discussion of the cobweb theorem notes that when the supply is elastic relative to the demand, the equilibrium is unstable and the oscillations grow in magnitude. Since the supply of agricultural products is inelastic and that of industrial products elastic, our author goes on to say, "Consequently, in an industrial economy we may expect to find a considerable measure of instability and a tendency toward change, whereas in an agrarian economy static conditions will be more likely to predominate" (p. 12). The usual thought on these matters is that the cobweb analysis applies almost exclusively to agriculture, where lags are long, rather than to manufacturing. The author seems to be confusing the alternative of damped versus explosive price and quantity fluctuations, which is what the cobweb theorem is about, with the alternative of growth versus stagnation where a more complex analysis seems to be called for.

One of the real curiosities in the book is the section entitled "Cybernetics and Automation" which appears in the chapter on the theory of wages. The author claims we have a structural unemployment problem, with a shortage of jobs for the less skilled and a shortage of workers for the more skilled jobs. The situation is ominous; since "By far the larger proportion of the labor force is composed of individuals who through hereditary or environmental limitations, or both, are not amenable to the training or retraining necessary to equip them with the kinds of skills needed. Rather we are faced with the prospect of a comparatively small elite group . . . for whom the demand in the labor market is likely to exceed the supply, and a majority group who will not meet most of the job requirements for whom the demand in the labor market will fall far short of the supply" (p. 185). The author asserts that the "remarkable" changes that are bringing this situation about "do not lend themselves to ordinary price-theory analysis," but he does not attempt to develop an extraordinary analysis of his own to deal with them. He is probably right to refrain.

Although it is difficult to cite any portion of this almost uniformly bad book as the worst, a strong candidate is the Part on the subject of distribution. In its 33 pages are found four chapters including the chapter on wages containing the discussion of unemployment referred to above. But surely the gem of this Part is the 6-page chapter entitled "Capital and Investment Theory." None of the 11 sections into which this 6-page chapter is divided contains

anything about the productivity of roundaboutness, time preference, trees being cut down, or kegs of wine aging. Instead we have three paragraphs on liquidity preference, which apparently disposes of the matter of the rate of interest.

The author acknowledges the help of 14 colleagues. They should sue.

BARBARA R. BERGMANN

University of Maryland

Francis Hutcheson and David Hume as Predecessors of Adam Smith. By W. L. TAYLOR. Durham, N.C.: Duke University Press, 1965. Pp. viii, 180. $6.00.

This volume is a study of relationships among the economic ideas of three of the chief figures in eighteen-century moral philosophy. Part I is an expansion of Taylor's 1956 article which appeared in the *South African Journal of Economics.* It is concerned for the most part with showing the nature of the personal and intellectual relationships existing among Hutcheson, Hume, and Smith. While it does not present any facts not previously known to the student of the period, the focus of this study and the manner of presentation provide sufficient novelty to hold the interest of the reader, whether he be thoroughly familiar with the original sources or not.

Part II, which is entitled "Economic Commentaries," attempts a task which is beset with pitfalls, i.e., to establish the influence of specific ideas or doctrines of Hutcheson and Hume upon Adam Smith. In some, a minority, of these cases, the reader may well feel that the author has failed to make his case. The first chapter of Part II, for example, attempts to show that Smith's view of the division of labor was strongly influenced by the teachings of Francis Hutcheson. There is, however, nothing in the text or the many quotations from Hutcheson which indicate that Hutcheson's ideas on the division of labor were substantially more advanced than those of Aristotle or Xenophon. This being the case, perhaps the most that we can really say is that Hutcheson may have been responsible for introducing Smith to the ideas of the Greek philosophers on this subject.

The second chapter of Part II, "Notions on Value," on the other hand, is full of interest to the historian of economic thought. Hutcheson is shown to have had as good a conception of the joint roles of utility and cost in the determination of value as was possible prior to the development of the marginal concept. Further, it is shown that Smith followed along Hutchesonian lines in his *Lectures* of 1763 but for some inexplicable reasons, perhaps the influence of David Hume, put the greatest emphasis on cost of production in the *Wealth of Nations.*

Of the remaining chapters, those on money and interest, luxury, trade, and taxation hold for the reader much the same interest and stimulation afforded by Chapter 2. The only chapter which falls substantially below this level is "The Theory of Property." Here it is not so much Taylor's treatment as the lack of anything to be treated that makes the chapter sag. Smith's remarks on the subject are so few and so generally in keeping with those of his age that any attempt to ascribe a definite ancestry to them must necessarily be rather unconvincing.

One of the more interesting facts brought to light by Taylor's study is that

Smith did occasionally exercise rather poor judgment in his borrowing of ideas. His choice of Hume's emphasis on labor as the source of value rather than Hutcheson's balanced emphasis on utility and scarcity and his decision to follow Hutcheson's rather pedestrian approach to questions of money and interest rather than Hume's brilliant treatment being cases in point.

Taking both the strong and the weak points of this study into consideration, one is bound to conclude that the strong far outbalance the weak and that it is an essential piece of reading for anyone who aspires to an understanding of the development of economic thought in the eighteenth century.

JAMES A. GHERITY

Northern Illinois University

Economic History; Economic Development; National Economies

The Vital Few: American Economic Progress and Its Protagonists. By JONATHAN HUGHES. Boston: Houghton Mifflin Co., 1966. Pp. vi, 504. $6.95.

Fowler, in the latest edition of his *Modern English Usage,* comes close to conceding defeat on "protagonist." Jonathan Hughes, with an assertive major title chosen, leaves his subtitle ambiguous. Are the eight entrepreneurs vividly portrayed in this book to be thought of as advocates of U.S. economic progress, or as "leading actors" in the drama, or perhaps as representing in their personalities and behavior some of the leading themes in the process? I suspect the last interpretation is correct, for Hughes explicitly says that he is using them as "surrogates," or substitutes, for the many other individuals who "did the same sorts of things in life" but whose biographies are inaccessible or less interesting. These eight men—William Penn, Brigham Young, Eli Whitney, Thomas Edison, Andrew Carnegie, Henry Ford, E. H. Harriman, and J. P. Morgan—are chosen because their acts exemplify kinds of behavior that Hughes considers important in determining the way the economy of the United States has developed.

Henry Ford, I feel sure, would not relish the idea of being cast as a stand-in for anyone else, and perhaps the same might be said of each of these inner-directed individualists. What does Hughes mean by calling them surrogates? What do they represent? Of what are they typical? Of what universe is this an efficient and informative sample?

Hughes's answer lies in a conceptual scheme which he describes alternatively as a set of "stages of history," "conceptual categories," or "basic economic functions." These are (i) idealism; (ii) invention; (iii) innovation; (iv) organization; and (v) stagnation and decline. I must confess that I find this confusing. The five categories do not seem to stand all on the same footing; they are certainly not mutually exclusive; and the list seems uncomfortably open-ended (does not pragmatism have as good a claim to inclusion as idealism?). To call them "stages" is certainly to invite misunderstanding, for Hughes explicitly admits that they are not chronological and that "the nation lives simultaneously in all these stages." As "basic economic functions" the first four carry more meaning, but "stagnation and decline" sticks out like a

sore thumb. Hughes, significantly, has no entrepreneurs to represent this final "stage."

The expository strategy which underlies this list of concepts is to create a framework "in which the role of the individual in the economic history of the United States may be illuminated by the study of the contributions of a few men." As a justification for Hughes's sampling procedure, this leaves the reader dissatisfied. What Hughes is really after is a classification of entrepreneurs in terms of personality types—specifically, in terms of particular skills or orientations that can be shown to have been economically significant. The fifth stage—stagnation and decline—does not fit this interpretation. Hughes's interest here is in bureaucracy, which is not necessarily the same thing. It would not have been too difficult to identify two entrepreneurs whose chief claim to fame has been their ability to use bureaucratic structures effectively, and I am surprised that Hughes gives us instead an interpretive essay on "the credentials society"—albeit a very stimulating one.

Hughes's attempt at system-building is, in my judgment, a very qualified success. His biographical chapters, on the other hand, are excellent. They show wide reading, careful research, a lively and curious mind—and above all they are not dull. This is economic and entrepreneurial biography at its best; not antiquarian, but infused by a keen awareness of the economic process at all levels. If they have a weakness, it lies in the author's tendency to take personality as given: we are told a great deal about how the personality acted on the environment, but less about the reverse reaction—what doing these things did to the man who did them. This might have been a harder book to read if Hughes had delved deeper into the psychological literature on personality, but on the other hand it might have acquired another dimension of insight.

Entrepreneurial history—some of the time—tended to analyze conspicuous entrepreneurs as deviants: people who introduced change by departing from established sanctioned modes of thought and action. Hughes's findings support this point of view, though he does not use the term. On the other hand, at a different level of analysis, these patterns of deviance were themselves a recurrent pattern in U.S. economic life. Hughes's eight men illustrate this dialectic between deviance in particular aspects of behavior and conformity to the more fundamental social norms of success, progress, and rationality. Herein lies much of the interest of a fascinating, if uneven, book.

HUGH G. J. AITKEN

Amherst College

Capital Exports and Growth among U.S. Regions. By J. THOMAS ROMANS. Middletown, Conn.: Wesleyan University Press, 1965. Pp. xvii, 230. $9.50.

Dr. Romans has made a rather heroic attempt to obtain measures of income, gross investment, saving, government revenue and expenditure, and net exports for each of nine major regions of the Continental United States. The same variables are also measured for the individual states. His major purpose is to explain and analyze the sources of differential regional per capita growth

rates over the period 1929-57 by making use of data on capital flows largely confined to the years 1953 and 1957. The impact of the federal government's fiscal policies, together with private investments and transfer payments, on these interregional investment flows is assessed.

The main accounting concepts employed are described by the following identities:

(1) $X = S + Tn - (I + G)$

(2) $X_d = S + Tn - (I + G) - D$

where:

X = Private net exports
S = Saving
Tn = Net taxes
I = Gross investment
G = Government expenditures
D = Income received from foreign investment
X_d = Private net exports from domestically employed resources.

Romans uses (1) directly to compute net exports (capital flows) by estimating personal and business saving for each state, in contrast to Department of Commerce procedures which treat savings as a residual. Needless to say, very difficult allocation problems arise and many *ad hoc* procedures have to be devised for allocating business retained earnings, depreciation allowances, and investment expenditures to individual states or regions. The difference between X_d and X in interstate or interregional trade can be relatively much greater than corresponding variables in international trade, because of greater differences in property income receipts.

As long as the analysis is confined to comparisons among the nine highly aggregated regions, his attempts to link levels of per capita investment and net exports to per capita income growth are rather disappointing. Although he measures marked differences in investment per capita, these are not significantly related to the very large differences in per capita income growth (p. 45). He did find that per capita saving was positively related to the absolute level of income and the share of property income—a fact which is not too surprising. The absolute level of federal government expenditures per capita in each state is also positively related to per capita income. However, high-income states tend to pay more taxes than they receive in expenditures; but, even so, these deficits are not significantly related to per capita income growth.

When Romans disaggregates down to the state level, he usually finds stronger relationships. High-income states are net exporters of capital—particularly New England and the Mideast. Since there is a generally negative correlation between per capita income growth and the absolute level of income, it turns out that growth in per capita income is inversely related to capital exports. Nevertheless, he concludes that the degree to which differential growth rates can be "explained" by interstate net capital flows is minimal. There are other factors, such as the switch from agricultural to manufacturing employment in the poorer states, which are more important in causing the gradual

convergence of per capita state income. However, the growth in nonagricultural income per worker does tend to be much more dependent on the net importation of capital than does growth in per capita income.

In general, it is very difficult to delineate any single empirically testable theoretical model that is adequate for explaining his numerous empirical relationships. There is always the problem of identifying the direction of causation with some variables to be "explanatory" and others to be endogenous. For example, the influence of interstate capital flows on growth is small after one allows for the differences in absolute income levels. But differences in per capita income are not by themselves too helpful as explanatory variables. Can an equilibrium level of investment for each state be defined using some idea of factor proportions in a production function which has the same parameters (including technical change) for all states? Romans himself notes that using a simple Harrod-Domar-type growth model where saving—both within the state and via capital imports—plays a central role is at least superficially in conflict with export-based theories of regional development. Having exports generating income seems to conflict with the importation of capital via a trade-balance deficit. Although George Borts has recently shown that the two theories can be consistent with each other in certain circumstances, it is still difficult to know whether improved access to capital or better export prospects is the leading variable in explaining an abnormally high growth rate.

In summary, one can say that Romans has done a very useful job in presenting empirical comparisons and relationships among the states despite the numerous "identification" problems involved. Although the importance of net capital flows is still not completely clear, one should not forget that large gross capital flows may still be extremely important in promoting the efficient specialization of investment activity. The book itself, while interesting, could probably have been organized somewhat more tightly with less repetition.

RONALD I. MCKINNON

Stanford University

Problems in Economic Development—Proceedings of a Conference Held by the International Economic Association. Edited by E. A. G. ROBINSON. New York: St. Martin's Press; London: Macmillan and Co., Ltd., 1965. Pp. xxi, 626.

This volume contains the proceedings of the International Economic Association Congress on the Economics of Development held in 1962. It is not possible here to discuss individually all the 34 papers in this collection; all one can hope to do is to give a rapid review of the contents of the four main sections which make up the book.

Section I begins with a useful survey of the theories of economic growth in capitalist countries by G. Ranis, followed by two papers on economic development in socialist countries, the theoretical paper by W. Brus and K. Laski and the applied paper by K. Plotnikov. This is followed by papers on an assortment of topics: "Agriculture and Balanced Growth," by G. U. Papi; "Education, Research and Other Unidentified Factors in Growth," by I. Svennilson;

"Historical Experience of Economic Development," by H. J. Habakkuk; "International Aid," by G. Leduc; "The Role of Taxation in Economic Development" by N. Kaldor; "Inflation and Economic Growth," by D. Horowitz; and finally, "Determinants of Saving in Developed and Underdeveloped Countries," by H. S. Houthakker. It is interesting to have Habakkuk's historical approach and Houthakker's econometric approach in the same section. A reader familiar with the literature on the underdeveloped countries will not, however, find any strikingly new developments in this section. The papers on taxation and on inflation confine themselves too much to generalities and the paper on education, research, and economic development is too sketchy. Collectively, the papers do not add up to "The Determinants of Economic Progress," which is the title of this section.

Section II, "Industrialisation and Methods of Increasing Labour Productivity," starts with two solid papers on the trends of labor productivity in Western Europe, North America, and Japan by A. Madison and in the non-Western countries by W. Galenson and J. R. Ericksson. We next have J. Pajestka's paper, "Stages of Industrialisation and Labour Productivity," which stresses, unsurprisingly, the relation between labor productivity and capital accumulation. This is followed by two interesting papers, one by W. E. G. Salter developing the "embodied growth" approach contained in his book *Productivity and Technical Change* and the second by S. Tsuru suggesting that the confusion between "real" and "value" aspects has led to errors in measurement of the effects of technological progress on productivity. Lloyd G. Reynolds in his useful paper discusses the relation between wages and productivity in the underdeveloped countries, while K. G. J. C. Knowles and H. A. Turner discuss this theme in relation to Great Britain and other advanced countries. J. T. Dunlop's paper evaluating the factors affecting productivity is much too brief and general; and the same may be said of S. Carlson's paper on the contributions of management to productivity. The section concludes with a round table discussion of the problems of individual countries.

Section III, "Techniques and Problems of Development Planning," opens with two papers, one by J. Tinbergen, "Simple Devices for Development Planning," and one by H. B. Chenery, "Approaches to Development Planning." Those familiar with the previous works of Tinbergen and Chenery will find the same quality but no strikingly new developments in these papers. These papers at least try to deal with the problem of *development* planning of the underdeveloped countries mentioned in the section title. This cannot, however, be said of the papers in the rest of this section. These, although they may be regarded as valuable contributions to planning theory, have little connection with planning in the underdeveloped countries. Under this category are the papers by P. Masse on discretionary or formalized planning, three papers on regional planning by G. Sorokin, J. R. Boudeville, and E. A. Lisle, and finally a highly technical paper, "Behavioural and Technical Change in Economic Models," by Richard Stone and Alan Brown.

Section IV, "The Stabilisation of Primary Producing Economies," starts with W. A. Lewis's notable paper on economic development and world trade, arguing (a) that the current decline in the terms of trade of primary products

is the inevitable result of the abnormally favorable prices which these products have enjoyed since World War II; (b) that there is no reason to believe that the ratio of world trade to world production will decline in the long run; and (c) that the rate of economic growth of an underdeveloped country is a direct function of the rate at which its exports can be expanded where possibilities of import substitution and foreign deficits are limited. After this, there are two papers on the long-run terms of trade, the first by M. Dantwala, who takes a pessimistic view, and the second by H. M. A. Onitiri, which is a useful survey paper. Next, we have a paper on the Kondratieff Cycle by L. H. Dupriez. The section concludes with three papers by G. Blau on international commodity agreements, by D. Walker on marketing boards, and by H. Hymen on the international tin scheme. In contrast to the preceding papers, the last three papers have the merit of actually dealing with the problem of the stabilization of primary producing economies. But they are implicitly based on the assumption that primary exports are subject to a much larger degree of short-run fluctuations than manufactured exports and that therefore the underdeveloped countries generally suffer from a much higher degree of short-run export instability than the advanced countries. This basic assumption does not now appear to be as obvious and unquestionable as it used to be, in the light of recent studies by various writers such as J. D. Coppock and A. I. MacBean.

H. MYINT

London School of Economics

National Income and Capital Formation in Mainland China—An Examination of Official Statistics. By SHIGERU ISHIKAWA. Tokyo: Institute of Asian Economic Affairs, 1965. Pp. xvii, 206.

The Chinese Model—A Political, Economic and Social Survey. Edited by WERNER KLATT. New York: Oxford University Press; Hong Kong: Hong Kong University Press, 1965. Pp. xix, 233, iv. $4.00.

The Economy of Communist China—An Introduction. By YUAN-LI WU. New York: Frederick A. Praeger, 1965. Pp. viii, 225. $6.00.

Although the number of monographs on particular aspects of the Chinese economy has increased rapidly during the past few years, there have been few attempts to provide the nonspecialist with an over-all view in a single volume. Dr. Wu's study represents an attempt to fill this gap, as, in a broader sense, does the collection of essays edited by Mr. Klatt. Professor Ishikawa's analysis of national income and capital formation, although narrower in scope than the other two volumes, is a basic reference work for anyone wishing to introduce himself to Chinese Communist national income concepts and data, useful to specialist and nonspecialist alike.

Any study of the Mainland Chinese economy must, from the outset, face the question of the reliability and value of official data emanating from Peking. It would be difficult to discover two more widely differing approaches to this problem than those of Wu and Ishikawa. The latter has attempted to re-

construct Peking's own estimates, taking care at each step to properly define and use the official figures, filling in gaps with his own estimates only when no official data are available. More than any other work on China with which I am familiar, Ishikawa makes explicit and clear what is not known as well as what is.

The result of these efforts is a collection of data which present a picture of economic development in China as seen by Peking for the years 1952-59. Because little or no official data have appeared since that time, there is no way of obtaining a comparable picture for the years 1960-65. Ishikawa's work also serves to demonstrate the high degree of internal consistency between widely disparate kinds of official statistics, an important first step in any attempt to appraise their reliability.

Ishikawa, however, purposely avoids the issue of whether the underlying official data accurately reflect real trends in China's economy. At times he does evaluate his results in a way that would indicate that, for at least the years 1952-57, he feels the official figures are relatively reliable (e.g., p. 48), but he makes no real attempt to deal with such key problems as the validity of official crop output data. He also intentionally does not convert the official national income figures, compiled according to Marxian concepts, into a Western conceptual framework. The reader who wishes to use the figures for purposes of appraising the performance of the Chinese economy or for international comparisons must be prepared to make his own conversion and, more important, his own appraisal of the worth of the underlying data. Ishikawa's study, therefore, should be looked on not as a substitute for but as a complement to such other national income studies as the one by T. C. Liu and K. C. Yeh. As such, it is a significant addition to our knowledge of the Chinese economy.

In contrast to Ishikawa, Wu rejects a large portion of the official figures and substitutes his own estimates in their stead. He also attempts to give a quantitative picture of economic performance during the years 1960-63 when no official statistics were available. The estimates he derives are a combination of trends derived from official figures and statements, guesses made by observers in Hong Kong and Taipei, and a variety of other kinds of data. The problem with this approach is that even the specialist has difficulty determining what the results mean. It is one thing to establish that there are biases in the official figures, and quite another to substitute one's own estimates which must, by their nature, be based on only one of several plausible and often arbitrary assumptions.

Nor are Wu's results always even plausible. I know of no other economist, for example, who believes that grain output in China fell in 1958 (p. 146). In his conclusion (p. 203), to take another example, his estimate of the annual rate of decline in per capita consumption is reduced from −2.5 per cent to −0.75 per cent when the years 1961-62 are included. One would expect the opposite, particularly given an earlier statement by the author that ". . . it was consumption that suffered most" in the 1961 crisis (p. 158).

These difficulties with the estimates, however, mar Wu's analysis less than might be supposed, principally because he does not appear to have relied very

heavily on these figures in writing the text. Thus Table VII-3 properly interpreted would seem to lead to the conclusion that Chinese agriculture was moving clearly, relentlessly, and none too slowly toward disaster as early as 1952. But in the text Wu uses such statements as "That agricultural production lagged behind advances in the nonagricultural sectors during the First Five-Year Plan is beyond doubt" (p. 132).

The discussion is best where the data are the least controversial. Chapter 6 on the industrial sector and much of Chapter 9 on international trade and aid are particularly useful. Wu's discussion of industry makes a number of important points, not the least of which is that industrial development in China and Manchuria prior to 1949 was far from insignificant.

Wu's book is, of course, more than just an appraisal of changes in output and consumption. The book was designed for a course in comparative economic systems and as such includes a discussion of organization and planning in China as well. As a presentation of the formal structure of Chinese economic institutions and plan procedures, these chapters are useful and well organized. Analysts of China and other Soviet-type economies, however, have often pointed out the fundamental differences between the formal rules and the actual operation of the economy. Emphasis on the formal side leads Wu to exaggerate such things as the effect on resource allocation of China's failure to properly utilize interest charges on capital (pp. 57, 202). For much the same reason there is a tendency to explain Chinese policies as little more than carbon copies of similar Soviet moves (e.g., pp. 27, 74) when in fact the reasons for or intent behind the two countries' policies, however similar on the surface, were quite different.

None of these remarks should detract from the fact that Wu has done a useful service by pulling together so much material into a single small and readable volume.

In the collection of essays edited by Mr. Klatt, only four of twelve concern themselves with economics. The remainder deal with everything from the role of mass media in China to Peking's policies toward Tibet and Sinkiang. The volume was apparently designed primarily to be read by "leaders of developing countries" rather than by professional economists or China specialists.

The brevity of most of the essays, together with their vast scope, causes a number of them to be rather superficial. Klatt's own essay on Sino-Soviet economic relations is a worthwhile summary of the subject, but his noble attempt to cover all of China's agricultural policies and problems from the 1930's to the 1960's in eighteen short pages is somewhat less successful. Other essays, particularly that by Funnell on changes in social values, are too polemical in tone to be genuine scholarly contributions.

The best essay in the volume is that by A. M. Halpern on the way in which the revolutionary experiences of the Chinese Communist leadership have shaped their attitudes and goals in the area of foreign policy. Also well worth reading are Joseph Levenson's summary of Peking's policies toward the various religions within China's borders and C. T. Hu's presentation of the regime's efforts in the field of education.

It is unfair, however, to criticize a book for failing to meet an objective for which it was not designed. The limited usefulness of these essays for the professional economist results from the fact that the book was not written for him.

DWIGHT H. PERKINS

Harvard University

The Rate and Pattern of Industrial Growth in Communist China. By KANG CHAO. Ann Arbor: University of Michigan Press, 1965. Pp. viii, 188. $6.75.

How fast has Communist China's industry grown? The official statistics show an extremely high average annual rate of growth of 29 per cent over the period 1949-59. While hardly any Western scholar takes this claim seriously, few have inquired into the nature and limitations of the official industrial production index. Fewer still have provided us an independent measure of this rate. Professor Chao's book is a significant contribution to the study of these important problems. Chao's new indexes of industrial production are unquestionably more meaningful than the official one. His excellent critique of the official index is illuminating. In addition, the wealth of data on physical output, prices, employment, wages, and productive capacity presented in this volume should be of great use to other researchers.

The book consists of four main chapters. The first discusses briefly the various formulas of production indexes. The second examines the major deficiencies in the way the Communist Chinese statisticians compute their industrial production index. The third explains in detail the construction of the two new indexes, one including and the other excluding handicraft output. The fourth evaluates the new indexes, comments on the rates of growth, and compares them with an estimate by Liu and Yeh and with those of other countries. The book also contains three appendices dealing with the effects of industrialization and production indexes, the industrial ministries of Communist China, and statistical tables.

The more important results include the following: during 1949-59 industrial output including handicrafts grew at an average annual rate of 21 per cent, as compared to the official claim of 29 per cent; the rate for factory production was 14 per cent, as compared to the official claim of 19 per cent over the period 1952-57; the annual rates of increase were rather uneven with much higher rates during 1949-52 and 1957-59; the subindexes for 13 industries show widely divergent rates of growth and, as may be expected, the producers' goods industries grew much faster than the consumers' goods industries.

On the whole the sources and method of constructing the two new indexes are set forth clearly. The index of factory output, the more important of the two, is essentially based on output series for 74 industrial products grouped into 12 industries. Four of these industries are each represented by a single and more or less complete commodity series. For each of the other eight industries, the output series are expressed as relatives with 1952 as base, and are combined in an arithmetic mean, using as weights factory or wholesale prices of January 1, 1952. The 12 indexes, together with the official index of gross

value of nonferrous metal output, are then aggregated to obtain a general index of industrial production, using as weights the wage bills of productive workers in these 13 industries.

The quality of the new index depends on: (1) the reliability of the 74 output series, (2) completeness of product coverage, (3) representativeness of the product coverage relative to the total product mix, and (4) the appropriateness of the weighting system. Turning first to the weights, one finds that the author is on solid ground. The wage bill weights, as an approximation to value-added weights, are conceptually far more acceptable than the official gross value weights. To compare the official and the new index, one can rewrite the official formula as $\Sigma P_0Q_0 \cdot Q_1/Q_0$, and Chao's formula as $\Sigma W_0 \cdot q_1/q_0$, where P_0Q_0 and W_0 represent, respectively, the gross value and wage bill weights for a given industry expressed as a percentage of total weights, and Q_1/Q_0 and q_1/q_0, respectively, the official and Chao's output index for that industry. Given the two sets of weights and output indexes for 1957 with 1952 as 100,[1] one can compute the following four over-all indexes for 1957 with 1952 as base:

	(Per cent)
(1) Official weights combined with official output indexes	241
(2) Chao weights combined with official output indexes	251
(3) Official weights combined with Chao indexes	191
(4) Chao weights combined with Chao output indexes	196

Interestingly, when different weights are combined with the same output indexes, as in cases (1) and (2), or (3) and (4), the results are fairly close. But when the same weights are combined with different output indexes, the results differ widely as in cases (1) and (3), or (2) and (4). Apparently the difference between the official and the new over-all index is more closely related to differences in the two sets of output indexes for the individual industries than to differences in the two sets of weights.

Chao carefully considers the various possible distortions of the physical output data due to misreporting and mishandling. He goes further and applies "reliability tests" to some output series. First, a consistency check is made by comparing reported output with estimated output derived from technical input-output coefficients or capacity utilization rates and data on input or capacity. These checks show relatively small discrepancies. Second, the technical coefficients and utilization rates are compared with sample means derived from miscellaneous data for individual plants in Communist China and with the coefficients and utilization rates of other countries. When they are found to be not "abnormally high or low," Chao concludes that the consistency checks become reliability checks. It seems to me that these ingenious tests are at best inconclusive. To begin with, numerous questions can be raised about the comparisons in the second step. For example, how meaningful are compar-

[1] The weights and output indexes are given in, or computed from data from: Chao's book, pp. 12, 81, 88, 96, 139, 155; *Ten Great Years*, Foreign Languages Press, Peking 1960, p. 92; J. P. Emerson, *Nonagricultural Employment in Mainland China*, U.S. Government Printing Office, Washington, D.C. 1965, p. 208.

isons between national average coefficients and sample means of different time periods when they may or may not remain relatively constant over time?[2] How meaningful are the international comparisons when so little is known about the levels of technology in different countries and particularly when there is no logical basis to expect similar rates of utilization of equipment in different countries. But even if one accepts the coefficients as reliable, two other conditions must be met before the consistency checks can become reliability checks: (a) the coefficients must be independent of the output statistics to be checked, and (b) the data on input or capacity, which are applied to the coefficients and utilization rates to derive the estimated output, must also be reliable. Regretfully, these crucial problems have been neglected.

What about the product coverage of the new index? Chao compares the calculated gross value of industrial products included in the new index with the official gross value of industrial production for 1952, and concludes that "the fairly high ratio (74.6 per cent) of calculated gross value to official gross value for all industries indicates that our new indexes have a solid base so far as coverage is concerned" (p. 81). While I have no doubt that the degree of coverage is respectable, I have some reservations about the "fairly high ratio" of 74.6 per cent. This ratio appears to have been overstated for two reasons. First, the denominator of this ratio, the official gross value, is computed by the "factory-reporting method," according to which the gross values of certain products produced and consumed by the same enterprise (e.g., cotton yarn in some textile enterprises) are not counted, and therefore the total is necessarily lower than the sum of the gross values of all individual products. Second, the prices used in computing the official figures are averages for the third quarter of 1952, whereas those used by Chao are prices of January 1, 1952. There are indications that the official prices are generally lower than the Chao prices. According to Chao, the "two sets of prices are drastically different because of the nation-wide 'Five Anti' campaign initiated in mid-January of 1952 and concluded sometime in the third quarter of that year. . . . The first half of 1952 saw a serious drop in commodity prices in most urban centers in China" (p. 32). Another indication is that the prices used in computing the official index are factory prices, whereas Chao uses wholesale prices for consumers' goods and factory prices for producers' goods. The wholesale prices are presumably higher than factory prices since they include distribution costs. In at least one industry even the factory prices of some producers' goods used by Chao are higher than the official ones.[3] Because of these complications the official gross

[2] Six out of 12 comparisons are of this nature (pp. 69-71). One can cite statistics showing rather wide variations in some of these coefficients over time. For example, the utilization coefficient of open hearth furnaces, in tons per sq. meters per day, ranges from 6.67 in 1956 to 8.21 in 1959. *Hsin-hua pan-yueh-k'an* (New China Semimonthly), No. 14, 1957, p. 146, and *New China News Agency*, Peking, Feb. 2, 1960.

[3] Taking the output of the five products included in ferrous metal for 1952 (pp. 120-21) and the respective prices (p. 177), one can calculate the gross value of output for Chao's ferrous metal industry at 2,430 million yuan, which is much higher than the official figure of 1,366 million yuan (p. 81). The difference is all the more striking if one notices that the official figure includes, whereas Chao's total excludes, the gross value of iron ore. Be-

value and Chao's calculated gross value are not strictly comparable. Proper adjustments to place them on a comparable basis would lower the ratio of Chao's calculated gross value to official gross value.

Assuming that the author's estimate of the coverage is correct, one finds that for the following four industries the coverage ratio is relatively low: petroleum (14 per cent), paper (22 per cent), metal processing (23 per cent), and daily-use commodities (24 per cent). To what extent does the undercoverage affect the representativeness of the output indexes? Chao classifies the omitted products into military end-products and others. He correctly points out that "as long as the growth rate for the production of military end-products did not exceed the rate of growth for the metal processing industry calculated in this study, the omission of military end-products does not cause any understatement in our overall index" (p. 83). But what is the rate of growth of the metal-processing industry? The author does not provide us a figure. One can, however, reconstruct it from Chao's other figures. If my reconstructed figure is correct, this industry was not growing very fast relative to other producers' goods industries. Its average annual growth rate is 18 per cent over the period 1952-57, lower than all others except those of coal and timber (p. 97). According to the author's figures, the implicit output index for metal processing for 1957 is 232 per cent with 1952 as 100, considerably lower than the 354 per cent for ferrous metal and 370 per cent for nonferrous metal.

As for the omitted products other than military goods, the author assumes that the output of all these products grew at a rate equal to the lowest of the 13 industries (i.e., 6.5 per cent for textiles for 1952-57) and comes up with the conclusion that the omission would cause an overstatement of the over-all rate of growth by 1.4 percentage points. The argument depends critically upon the assumption of a low growth rate. The rationale of this assumption is that the Communists generally release only statistics showing rapid growth. One wonders if this assumption is valid for all the omitted products. Such major omitted items as iron ore, gasoline and other refined petroleum products, nonmilitary metal products, chemicals such as dyes and paints, nonmetallic minerals such as limestone and bricks for the rapid-growing construction industry, and printing and publishing in all likelihood grew faster than textile output which had been seriously restricted by the relatively stagnant agricultural production.

Chao also considers the question of new products which have not been included in the new index. He takes the difference between the official average annual rates of growth and his estimated rates for the metal processing and chemical industry over the period 1952-57, assumes that half of the discrepancy is due to omission of new products and military end-products in these two industries, allows an understatement of the over-all rate of growth by 0.26 percentage point for new products in all other industries, and concludes that the omission of all new goods and military end-products probably could have

cause of this exclusion of iron ore, the author's assertion that coverage for the ferrous metal industry is 100 per cent is misleading.

:aused at most an understatement of 1 percentage point in the growth rate of he over-all index. Again the conclusion is rather sensitive to the assumptions nvolved. How convincing the demonstration is must depend on one's judgnent as to the plausibility of the author's assumptions. In any event, there is ın error here, the correction of which would yield a higher degree of understatement. The estimated average annual rate of increase for the metal pro-cessing used in the calculation (p. 85) is taken from Table 24 (p. 97). It is ıctually that for machine building and not for metal processing. The latter is ower than the former, and therefore the difference between the official and esimated figure should be larger than that shown on page 85. The adjusted estinate for metal processing alone would result in an understatement of the av-rage rate of the over-all index by about 1 percentage point.

Despite all these reservations the new index of factory production appears o be quite a good one. However, as the author himself points out, the index or the periods before 1952 and since 1957 is of a somewhat different order, ›ecause of the lower quality of the statistics for these periods, relatively inomplete coverage, and an unknown margin of error introduced by the asuınption that handicraft production in 1958 and 1959 remained the same as n 1957. For similar reasons, the index of handicraft output is not comparable o the index of factory production in quality.

On the whole the study is meticulously documentated, and the author hanlles the intricate Communist statistics admirably. Yet, as is generally unıvoidable in any empirical study of the Chinese economy at the present stage, here are some misinterpretations, implicit assumptions, and omissions. Examıles: the output of cotton cloth produced in factories for 1949-55(p. 124) acually includes some handicraft output.[4] The wage rates used to derive the ıeights for ferrous metal, metal processing, and textile (p. 155) are those of ıonprivate enterprises only.[5] The prices of coal and petroleum used to calcuate the coverage ratios (p. 81) are not given. The price of metal cutting mahines (p. 157) is based on a statement that 3.7 tons of ferrous metals were ısed for every 10,000 yuan's worth of metal cutting machines and tools in 955.[6] One also wishes that there had been a list of tables and some annotaions in Appendix Table C-1 to indicate which commodities are included in he index.

This book is a pioneering study. It is important to realize that the short-:omings are largely due to the meagerness of information, a matter beyond the ›ower of any researcher. Moreover, these shortcomings by no means detract rom the significance of its contributions. The main conclusions still stand. I

[4] State Statistical Bureau, *Wo-kuo kang-tieh, tien-li, mei-tan, chi-hsieh, fang-chih, tsaohih kung-yeh ti chin-hsi* (The Present and Past Conditions of Our Iron and Steel, Power, 'oal, Machinery, Textiles, and Paper Manufacturing Industries), Statistical Publishing Co., 958, pp. 155, 166.

[5] *Ibid.*, pp. 32, 129, 175.

[6] Ching Lin, "The Proportional Relationship between the Iron and Steel Industry and he Machine Building Industry," *Chi-hua ching-chi* (Planned Economy), No. 9, 1957, ›. 12.

have no hesitation in recommending this book to serious students of the Chinese economy.

K. C. YEH

The RAND Corporation

Industrialization and Economic Development in Brazil. By WERNER BAER. Homewood, Illinois: Richard D. Irwin, Inc., 1965. Pp. xii, 309. $8.50.

Industrialization by import substitution, inflation and economic growth, and the effect of rapid development on the structure of the Brazilian economy are the principal selected topics considered by Werner Baer in this informative and provocative monograph. In keeping with the research objectives of the Economic Growth Center at Yale University which supported this study, the book emphasizes measurable aspects of industrialization and the development and testing of certain hypotheses about the growth process. In addition, the author has made available in an appendix of almost 100 pages, an impressive and valuable array of economic and social statistics for Brazil as well as an incisive commentary on the reliability of Brazilian statistics.

The Brazilian experience offers an unusually interesting opportunity for examining certain aspects of industrialization. From 1947 to 1961, the period studied in depth by Professor Baer, Brazil was in the top ranks of both the developed and underdeveloped countries in terms of over-all economic growth rates (averaging 5.8 per cent per year), in terms of rate of expansion in industrial output (9.6 per cent) and in terms of inflation. Over the period, price levels rose by an average of about 20 per cent annually—as measured by the cost of living index for Rio de Janeiro—with increases as high as 40 per cent in some years. Subsequent to the period studied by Baer, the same index showed even greater increases—52 per cent in 1962, 71 per cent in 1963, and a rate in early 1964 that might have reached 144 per cent on an annual basis if a change in government had not occurred.

The purpose of the study, according to the author, is to examine the process of Brazil's industrialization and some of its effects on different sectors of the economy. As background for his analysis of the 1947-61 period, Baer first presents a brief overview of Brazil's physical setting and historical trends. A second chapter provides a sketchy summary of industrialization attempts prior to World War II. The third chapter tells of the sustained industrialization effort since World War II from the standpoint of policies and the resulting changes in economic structure and over-all rates of growth. The background is completed by a description of Brazilian policy-making institutions.

The most impressive contribution of this study begins in Chapter 5 with an examination of the sources for financing Brazil's industrialization, and the role of inflation. The next chapter examines the linkages through which Brazil's industrialization spread and develops an explanation for Brazil's low incremental capital-output ratio. A final topic is the question of imbalances; in particular, the imbalance between agriculture and industry, and regional disparities. An epilogue comments briefly on the drastic changes in political leader-

ship and economic policy which were initiated in April 1964, after the overthrow of President Goulart.

On the role of inflation, Baer concludes that "it is difficult to notice any drastic misallocation of resources directly attributable to inflation" (p. 132). And he goes further to suggest that "some part of the Brazilian inflation contributed to the real growth of the country through its forced savings aspect" (p. 134) and not necessarily at the cost of lower living standards.

Another interesting aspect of the Brazilian experience emphasized by this study is that Brazil achieved its record of industrialization and growth without comprehensive economic planning and without any real attempt to be selective in the choice of industries to be substituted. And Baer concludes that "it is open to question whether a more selective industrialization policy along the line of the greatest potential comparative advantage could have led the country to growth rates as high as the ones that actually occurred" (p. 193).

The treatment of imbalances and bottlenecks is selective and relatively weak. In the case of agriculture, relatively little field research had been undertaken in Brazil until recently, and as Baer points out in his statistical appendix (p. 210), the agricultural data "are not very reliable." In the absence of good information, Baer's analysis heavily reflects the pessimism concerning agricultural change and the great emphasis on the need for land reform characteristic of much professional and political opinion in Brazil. Since Baer completed his study, there are indications that extensive field research recently undertaken by Professor William Nichols and Dr. Ruy Miller Paiva may present a more optimistic picture of agricultural progress than has generally been accepted in Brazil.

The analysis of regional imbalances by Baer can also be questioned. He seems to accept the traditional view that rapid growth and industrialization accentuate regional inequalities. However, he neglects two important elements. First, he does not recognize that a new trend toward a narrowing of the regional disparities between the Northeast and the South began in 1955, even though this trend is apparent in his Table 7-7 (p. 170). Second, he limits his analysis of regional disparities to the transmission of growth from one region to another through movements of goods, capital, and labor. A crucial factor not considered is the differential effect of external international market forces. Since 1955, the gap between the Northeast and the South has been narrowing because of favorable international markets for the principal export products of the Northeast, namely, sugar, cotton, and sisal. In contrast, the expansion of the South was slowed down by a deterioration in the international market for its principal export product—coffee.

This is a relatively short monograph—aside from the extensive and excellent statistical contributions in the text and appendix—for covering such a large subject as the process of industrialization. Consequently, it should not be unfair to characterize the study as an analysis of selected issues with emphasis on the measurement of *what* has occurred in Brazil rather than on *why* the industrialization effort was so successful. For example, Brazil's industrialization was based almost completely on private enterprise, both domestic and

foreign. What explains the vigorous response by Brazilian entrepreneurs in a situation where the weakness of domestic entrepreneurship has generally been emphasized? Why did foreign private enterprise respond so effectively to the incentives when "everyone knows" that continuing rapid inflation and considerable political instability, which prevailed in Brazil even before the 1961 crisis, are anathema to the foreign investor? Baer has provided an excellent foundation for such further studies of the dynamic forces underlying the industrialization process.

STEFAN H. ROBOCK

Indiana University

Essays on the Chilean Economy. By MARKOS MAMALAKIS AND CLARK W. REYNOLDS. Homewood, Illinois: Richard D. Irwin, 1965. Pp. xxii, 409. $8.50.

Two of the most important economic problems facing developing Chile are examined in the essays which make up this work. Markos Mamalakis discusses the sectoral impact of the Chilean inflation and the accompanying complex network of government decisions in his "Public Policy and Sectoral Development—A Case Study of Chile 1940-1958." The study by Clark Reynolds, "Development Problems of an Export Economy: The Case of Chile and Copper," part of which appeared about three years ago in this *Review*, focuses on the copper industry, a capital-intensive, foreign-owned enclave which has been the target of divergent analyses.

Both men center their analyses around concepts which, if not completely original, have not been previously applied to Chile. Mamalakis explores the causes and effects of the inflation in terms of "sectoral clashes," defined as "the excessive and administered antagonism and struggle for maximum privileges and advantages between an economy's sectors" (p. 5). These clashes resulted in government policies favoring industry, suppressing agriculture and mining, and remaining neutral toward construction and services.

Reynolds, in asking the basic question about benefits conferred on the Chilean economy by the copper mining companies, turns to a ratio he calls "returned value." Recognizing that the primary sources of benefits stem from expenditures in Chile on wages, locally purchased supplies, and taxes, Reynolds calculates a ratio of these local expenditures to value of production. The higher this ratio of returned value, the greater will be the participation of the export sector within the domestic economy.

Noting the basis for his findings in a 35-page statistical appendix, Reynolds documents the growth of productivity in Chilean copper mining, the more rapid growth of returned value, and the even faster growth of the government's share of returned value. Yet mining failed to serve as a leading sector to propel the rest of the economy out of its widely noted four-decade-old stagnation. Reynolds places much of the blame for this failure on government, which despite its increasing share did not invest copper tax revenues but rather used them for its expenditures on current operations. A valuable source of potential growth was thereby wasted largely on an expansion of the bureaucracy, whose contribution to growth was minimal.

One of the many values of Reynold's essay is the extent to which disaggregation enables the reader to follow the economic forces impinging separately on the three companies mining copper in Chile. Each of them used a different mining process and each, as a result, was faced by different cost conditions. The web of government regulation, too, affected each of the companies in a distinct fashion, and this in turn was reflected in their respective investment decisions. The panorama involving labor costs and labor unions, multiple exchange rates, differential tax treatments, and rates of return on investment are skillfully woven into a framework of analysis which will have value not only for development economists, but also for those concerned with industrial organization and fiscal policy.

The Mamalakis essay, considerably broader in scope, finds the same villain responsible for the Chilean economic stagnation. Chile represents "an instance of unequal sectoral treatment by government in an originally export-oriented economy and a case of intervention which did not succeed in establishing continuous and rapid growth" (p. 171). Favorable government treatment for industrial enterprises, coupled with price ceilings and subsidized competing imports in agriculture, shifted the relative rates of return in these sectors and led to the neglect of agriculture. This view contrasts with that of the structuralist school on inflation, which blames agriculture's torpor on a landed gentry unresponsive to profit incentives. The Mamalakis analysis shows that in those few agricultural sectors where profits were high, production increased dramatically.

Two flaws mar the imaginative set of conjectures exposited by Mamalakis. The first involves a somewhat heroic generalization. The stagnation in agriculture and the growth of consumer-goods manufacturing depended on the relative rates of return in these fields. Convincing empirical evidence about factors influencing these rates is adduced in support of the assertion. On the other hand, in the capital-goods sector, we may simply infer from the absence of production, says Mamalakis, that rates of return were low. Additional evidence on this point would not be at all remiss; as it stands now, the generalization of the rate-of-return hypothesis to cover this sector calls for a considerable amount of faith on the part of the reader.

The other aspect of the essay provoking some concern is Mamalakis' use of statistics. The empirical testing of his provocative hypotheses depends on the reliability of the sectorally disaggregated national income accounts. Despite Howard Ellis' praise in the book's foreword of the "range and quality of [Chile's] statistical information," Mamalakis asks his numbers, derived largely from dated secondary and English-language sources, to do more than they can. He fearlessly uses data from the national accounts which have undergone substantial revision even during the period prior to the book's publication.

These pedestrian details do not detract substantially from the refreshing view Mamalakis has provided on a subject which I, for one, had thought to be exhausted in the literature. Together with the essay by Reynolds, this book will be a standard source not only for Chilean economists, but also for any student of export-oriented and inflation-prone economies.

BRUCE HERRICK

University of California, Los Angeles

Studies in Economic Problems—A Collection of Seminar Papers of the Department of Economics, Calcutta University. Edited by S. K. BASU. New York: Asia Publishing House; Taplinger, distributor, 1965. Pp. v, 256. $12.00.

This volume consists of 19 essays by 14 authors. Virtually all of the essays deal with some aspect or other of the Indian economy. One wonders why this collection was ever published. Of course, the dust jacket refers to "the penetrating analysis, supported by figures, and the originality displayed by each author. . . ." The reverse would be a generally more apt description. Apart from everything else, no essay was written after 1961. Many of the problems are no longer of substantive interest, and the data are invariably badly outdated.

MORRIS DAVID MORRIS

University of Washington

The Structure of the Slovenian Economy, 1848-1963. By TOUSSAINT HOCEVAR. New York: Studia Slovenica, 1965. Pp. 277. $7.00; paper, $5.00.

This book by Toussaint Hocevar is an economic history of Slovenia from the time it was part of the Hapsburg Empire to its present status as a republic in the multination state of Yugoslavia. Spanning more than a century of political and social change, Hocevar's book also provides useful insights into the broader processes of economic development. The author's judgement that self-sustaining growth required an endogenous supply of entrepreneurs, skilled labor, and capital is supported by his observation that, under Hapsburg rule, Slovenian economic process was retarded by a lack of social and economic mobility resulting from ethnic division along class lines. Related to this discussion is Hocevar's hypothesis that economic development presupposes "ethnically conforming institutions," in which the use of a common language is the fundamental criterion of ethnic conformity.

The study is divided chronologically into three parts. The first of these, which describes the era preceding World War I, begins in the preindustrial 1840's with the awakening of nationalism in Germany and the Hapsburg Empire. Developments following Slovenia's industrialization with the help of outside entrepreneurs and capital include agricultural advances, demographic changes, and the rise of Slovenian nationalism, with which the growth of endogenous financial institutions was closely associated. Chapter 5 concludes Part I with a consideration of "Ethnicity and Economic Development," the central theme of the study.

Part II is concerned with the interwar period, the inclusion of Slovenia in the multination-state Yugoslavia, the growth of industry, agriculture, and business, and the evolution of new socioeconomic and political institutions on an ethnic basis. In Part III, which deals with Socialist Yugoslavia in the postwar era, the author examines the consequences of socialism for agriculture and industry, and discusses the role of Slovenia within a multination state.

A wealth of historic information concerning Slovenia's industrial growth is presented, starting with the establishment of social overhead capital in the form of railroads, coal and steel industries, and hydroelectric power. Subse-

quent developments include a gradual shift in emphasis from basic industries to the manufacture of more sophisticated products and a rise in the significance of service industries.

The role of agriculture in Slovenia's economic development is also appropriately emphasized. With the abolition of the feudal system in land tenure in 1848, peasants began to respond to the incentives of an increasingly monetary economy which encourages modernization and intensification of farming methods. New markets for farm products followed urbanization, improved transportation, and the liberalization of trade, while an expanding industrial complex absorbed some of the oversupply of farm labor.

Economic development in Slovenia, as elsewhere, was accompanied by a disruption of traditional patterns of living, particularly among the peasants. Similar to the experience of the United States and Western Europe, rural-urban migration in Slovenia tended to swell during prosperity and to subside during periods of depression. Economic progress was frequently interrupted when business depressions which originated in more highly developed countries were transmitted to less developed economies via foreign trade. Agriculture in Slovenia also suffered when farm prices fell during the latter part of the nineteenth century under the impact of growing wheat supplies from North America and Russia.

In the decade 1891-1900, emigration from Slovenia rose to over two-thirds of the natural increase in population. Hocevar's view that this represented a regrettable loss of manpower and investment in human resources does not take account of the possibility that, without a sufficiently broad industrial base, emigration may actually contribute to economic development by reducing population pressures on limited natural resources.

Following World War II, when Slovenia had been a part of Yugoslavia for a quarter of a century, a complete revamping of social, economic, and political institutions took place under socialism. The new regime, after making an unsuccessful attempt to concentrate decision-making authority in Belgrade, was unable or unwilling to impose the police-state methods of the Stalin era on its people, and reluctantly resolved to decentralize. The complete reversal of farm-collectivization policies which followed may be viewed as a victory for peasant individualism and a defeat for the Communist rulers.

Despite these setbacks for the regime, economic progress under socialism has been significant. Production has increased through ruthless exploitation of natural resources, continuous operation of the industrial plant, and foreign loans. Slovenia, the most developed area of Yugoslavia, has been called upon to contribute a vastly disproportionate share to the national economy via large-scale transfers of capital to other Yugoslavian regions.

Hocevar contends that such redistribution of income among nations in a multination state should be severely restricted if not eliminated, both on ethical grounds and "because large-scale transfer of income from Slovenia reduces her potential product by more than it increases the product in the region which is the beneficiary of income transfer." He suggests that, instead of retarding the development of Slovenia in this fashion, the central government should seek massive foreign aid to raise and maintain consumption and social

standards in less developed areas at levels comparable to those in the more developed regions. It seems questionable, however, that his recommendation would be accepted either by the Belgrade government or by potential donors of foreign aid, for the redistribution of income within a sovereign nation is accepted practice in many countries and is subject, as often as not, to political rather than to cost-benefit considerations.

Hocevar's book makes a valuable contribution by bringing together an abundance of source material on the economic history of Slovenia which is otherwise not readily accessible. His thesis, which points to ethnicity or nationalism as a potentially constructive force in economic development, merits thoughtful examination. His position would have been strengthened by a consideration of the works of Rostow, Myrdal, and others who emphasize the role of emerging nationalism in economic development. Although Hocevar does not generalize from his findings, his discussion may have useful implications for studying the problems of economic, social, and political integration in other ethnically diverse countries such as Belgium and Canada, and in potential supranational entities such as the European Common Market. Hocevar's study is a sharply focused, scholarly work on the economic history of Slovenia and an investigation of some of the key factors in Slovenian economic development.

H. W. BAUMGARTNER

State University College, Oneonta, New York

Das Wachstum der deutschen Wirtschaft seit der Mitte des 19. Jahrhunderts. By WALTHER G. HOFFMANN WITH FRANZ GRUMBACH AND HELMUT HESSE. Berlin, Heidelberg, New York: Springer Verlag, 1965. Pp. xxvi, 842. DM 138.00.

In August, 1965, Professor Hoffmann published a detailed account of the growth of the German economy since the middle of the nineteenth century. This monumental work is the most comprehensive empirical study to date of long-run German economic growth. The preparation of this study took several years and occupied more than a dozen co-workers. The effort was worthwhile. Hoffmann's work, which is 842 pages long and contains 26 graphs and 325 tables, fills gaps in our information which have until now dismayed many a student interested in the long-run development of the German economy. The bulk of existing statistical information on the growth process has now been collected in one volume.

Some of the data presented in the book, most of which are time series, go as far back as 1817. For the years before 1871 statistical information available on the area of the Reich as a whole is poor. To establish time series for the earlier period required a very close study of the statistics published by the statistical offices of the various independent German states which existed before 1871. Needless to say, until now these scattered data were either not accessible to most research workers, or they were useless without an intimate knowledge of the contemporary political and institutional structure.

Primary statistics which were collected in the nineteenth century, and for that matter during much of the first part of this century, give useful informa-

tion for a variety of purposes, yet they are generally of little use for modern growth analysis. The necessary raw material for the latter are time series on aggregate output, employed labor, and productive capital. These series are now available. They were established for the economy of the Reich as well as for different sectors. For the period after World War II, the series refers to the area of West Germany. East Germany is unfortunately not included in the study.

The extension of the study into the 1950's provided a convenient check on the methodology employed in transforming 19th century primary data into time series useful for growth analysis. The derived time series for post-1948 can be compared with official time series. It was found that the two sets of data are both consistent and similar, which is reassuring. However, the authors advise the use of official West German data and not the series given in this volume for postwar studies.

The book falls into two parts. The first, consisting of 165 pages, summarizes the main results of the study. Among the topics discussed are the origin of national product, population growth, employment, and labor productivity. Other topics are the growth of the capital stock, technological progress, production in different sectors of the economy, income distribution, and the use of the national product. The first part ends with an investigation of the development of foreign trade. A final section is devoted to a comparison of different long-run national income estimates for the German economy.

The second part of the book provides a finer breakdown of the aggregates analyzed in Part I. The data on the capital stock are very detailed. There are remarkably good data on changes in the structure of consumption, the agricultural sector, as well as on some industrial sectors. Students of public finance can find time series on the long-run growth of public expenditures, while those interested in investment financing are provided with detailed data on the assets and liabilities of different types of financial institutions. There is a long-run series on currency in circulation. A long-run series for the total money supply, for which many readers may look, could, however, not be established.

What are the most important findings of this study? The primary intent of the authors was to compile data on the progress of long-run economic growth. In this they fully succeeded. There is indeed a mountain of data waiting for further evaluation. From what the authors themselves have analyzed, the following over-all picture emerges.

In 1913 constant prices the net social product increased annually by 2.6 per cent between 1850 and 1913, and 2.8 per cent between 1872 and 1913. (This compares with an annual increase of 6.6 per cent for West Germany between 1950 and 1959.) Population increased after 1820 from .8 per cent to 1.4 per cent. If we assume that the average population increase was around 1 per cent, the long-run increase in real per capita social product was 1.8 per cent before 1913. It is not possible to give a detailed account of the many underlying structural, institutional, social, and technological changes influencing this expansion within the compass of a review. The interested reader will find abundant material on this in various chapters.

The growth rates cited above reflect rapid capital formation (the capital

stock in 1913 constant prices increased 3.1 per cent annually between 1852 and 1913, and 8.3 per cent between 1950 and 1959) and a considerable ability to innovate and apply new technologies. One may also observe another important feature of the growth process. According to this study, labor productivity per hour worked rose 1.8 per cent per annum over the period 1850-1913. Yet from Gerhard Bry's 1960 National Bureau study of wages in Germany, one can estimate that real wages in industry (certainly the highest-paid sector) rose on the average, at most, 1 per cent per annum during the period 1871 and 1913. The rate was presumably less for the whole period 1850-1913. The German takeoff into economic growth was thus accompanied by a serious lag in real wages, something which was witnessed again in the *first* phase of West German economic growth after 1948. This study finds (p. 87) that, using a wider definition of labor income than official West German statistics, the labor share in income fell in both periods. This will explain part of the high rate of capital formation and the ability to implement new technologies quickly. There remain however a number of puzzles to be solved.

Never before in German history has economic growth been so rapid as that in West Germany after the last war—and, we may add, also in East Germany, though there under a different order. There must have been some kind of breakthrough to overcome a low level 2.6 per cent (and in the interwar period even lower) real growth rate. A number of explanations have been offered for this. Some authors have given as a reason for West Germany's economic revival and growth the return to a free-market system, others the full integration into the world market, and again others—among them this reviewer in a recent book—public policy with respect to the volume and structure of capital formation. The reader of this study is left to wonder what Hoffmann might have to say—perhaps in another book—on this challenging subject.

In view of the importance of this book the reviewer hopes that it might be possible for the authors to produce a special, separate summary of the sources used in this study and, in particular, a description of the methods of derivation of the time series. This would greatly enhance further research. Further, for the benefit of English-speaking economists, an English edition would be very desirable.

All in all, this book is a major contribution to the understanding of the long-run economic growth of an important industrial country. For this work the economic profession will be thankful to Professor Hoffmann.

KARL W. ROSKAMP

Wayne State University

Statistical Methods; Econometrics; Social Accounting

Toward Income Equality in Norway. By LEE SOLTOW. Madison and Milwaukee: The University of Wisconsin Press, 1965. Pp. xviii, 155. $10.00.

This book is a bold and imaginative exploration of a fantastically rich body of data for the study of the size distribution of income.

The basic source materials consist of annual assessment books of eight Norwegian towns ranging in population in 1960 from 2,000 to 28,000. The books

contained each resident's name, address, vocation, income, and wealth. Four of the towns were in a county marked by rapid population growth and industrialization during the past century (Östfold) and the other four from a county with slower growth and less industrialization (Vest-Agder). Stratified samples of at least 40 male names including all of the upper tenth of income earners were generally taken for each town in each of the 15 years selected for study by the author within the period 1840 to 1960 (although there were missing cells because many books for the 19th century dates were lost). There were, of course, a number of data problems such as changes in the definition of income and under- and overreporting of incomes which appear to have been handled in a careful, thorough, and judicious manner. The Gini concentration ratio (equivalent to the arithmetic mean difference between incomes for all pairs of individuals) is the measure of inequality mainly employed in the study.

Professor Soltow uses these materials and this measure to study the long-run trend in the distribution of income in the eight cities (both in over-all terms and for specific income groups), the relationship between wealth and income changes, lifetime income distributions, the relation of sociopolitical changes to the income distribution, and the redistributional effects of public finance.

The evidence points to a sharp decline in inequality. Much of the decline had occurred by the turn of the century; city concentration ratios of around 0.60 and 0.70 in 1840, 1845, and 1855 had been replaced by ratios of around 0.40, and in Sarpsborg, the Östfold city with the greatest population growth and industrialization, the ratio was less than 0.30. The Great Depression caused inequality to rise but the downward trend was resumed after the war and by 1960 the concentration ratio was around 0.30 for seven of the cities, and a little over 0.40 in the eighth.

An important issue about which little is said concerns the extent to which these findings relating to eight towns may be applied to all of Norway. At one point at least (p. 61) the author suggests that the findings may be generalized in this way, and this does not on the whole seem implausible to the reviewer. However, there are important sectors omitted from the study. About 6/7 of the Norwegian population was rural in 1865 and 2/3 was rural in 1960[1] (although only 1/4 of the rural labor force was engaged in agriculture). Also, Oslo, to which the author makes no reference, included about 1/6 of Norway's urban population at the beginning of the period and 2/5 at the end.

The author delves into the history of the country and the individual towns to try to find the reasons for the decrease in inequality. He concludes that the important factors included (1) the breakdown of monopoly positions (particularly with the end of mercantilist restrictions); (2) the weakening of the influence of property incomes as property ownership became more dispersed, more public, and less lucrative, at least with respect to rents and interest pay-

[1] A question about the behavior of the concentration ratio in the rural population is raised by the lack of any decline in inequality between 1900 and 1950 (the terminal years for which data are available) in the town of Farsund, which in size, growth, and occupational distribution may be closer to the rural areas than the other cities.

ments; (3) a diminution in seasonal unemployment due to a decline in the relative importance of the timber and shipping industries with the rise of factory employment; (4) public policies such as transfer payments, progressive taxes, and full employment; (5) increasing equality in educational opportunity; and (6) reduced differentials for ability.

The data enabled the author to explore a variety of other interesting matters. In a study of specific income groups, based on Sarpsborg data, he shows that income dispersion declines sharply when the two bottom tenths and the top tenth of the distribution are eliminated, but remains relatively constant when further layers of the distribution are peeled away. Over time, the mean incomes of the top tenth dropped from 5 times the over-all mean income in 1855 to 2.5 times in 1960. The author finds, however, that even for the middle classes (from the 20th to the 80th percentile) the redistribution effect was not as important as the growth effect; their percentage gain in relative income was only about half of the over-all rate of growth in real per capita income. The inequality within the top tenth has not tended to diminish and, relative to inequality within other tenths, it has increased.

One chapter is devoted to a study of the relation between wealth and income for 439 men in one city who were between 55 and 64 years of age in 1960 and for whom income data for 1930, 1938, 1950, and income and wealth data for 1960 were available.[2] When 1960 wealth is regressed against the incomes for each of the four years, the coefficient for 1960 is, as would be expected, larger than that for the earlier years, but not overwhelmingly so. The author estimates that the elimination of the above-average wealth of the top tenth of the income distribution (which had 45 per cent of total wealth and 24 per cent of total income) would reduce the 1960 concentration ratio for the group under study from 0.243 to 0.188.

Through the device of transition matrices using a 2×2 cross-classification table, the author estimates that only small increases in inequality would occur if the relationship between father's 1930 income and son's 1960 income persisted through time for many generations. On the other hand, the persistence of changes for a given group of men as they grew older would in itself tend to produce marked increases in inequality.

For his study of lifetime income changes the author was able to analyze income data for every other year from 1928 to 1938 and from 1948 to 1960 for 121 men who were 55, 60, and 64 years old in 1960. He finds that the concentration ratio declines from the average annual ratio by 4.4 per cent when the accounting period is extended from the first year alone to the first two years, by 10.3 per cent when the first three years are included, and by 26.9 per cent when all the years are combined. These changes are somewhat larger than the reviewer found in an earlier examination of longer-than-one-year distributions for Delaware, Wisconsin, the United States, and the United Kingdom,[3] but differences in places and methods could easily account for this.

The author is continually conscious of the relevance of the income distribu-

[2] Actually, wealth data were available for only 325 of the men.

[3] *Structure of Income*, pp. 268-79.

tion to the functioning of the economy and the society at large. He concerns himself at a number of points with the impact of the changes in inequality on saving and investment and thus on economic growth. Among his more important findings here is that the increase in equality did not come at the expense of high investment. The fraction of gross product invested in Norway has been very high relative to the investment ratio of other countries owing to a policy of high taxation and high public investment. The increase in public capital has been almost as large as the rise in private capital. The Norwegian tax system is in general much more progressive than that of the United States.

Not all of the author's efforts at explanation seem felicitous to this reviewer. For one thing, Soltow attempts to explain the impact of the economic changes outlined above in terms of Rostow's stages of growth, using Rostow's dates for Sweden. Both the movement of the concentration ratio over time and the timing of the economic changes make this a rather unconvincing exercise. Several different sets of "distinct periods" could be justified as easily as those selected by the author from the chart showing the movement of the concentration ratios over time, and a number of changes assigned to one period or another (e.g., welfare legislation) really occurred over two or more "periods."

Secondly, he regresses the concentration ratio against city size and finds less inequality in larger cities and greater decreases in inequality in faster-growing cities. His explanation is that high incomes tend to be diluted by an increase in the number of incomes. This is a far from adequate explanation and is not necessarily correct even on a mechanical level. The concentration ratio is likely to decline when new incomes are added that are within the existing range of incomes and to rise when the new incomes are above or below the existing range. Immigrants might conceivably be accepting incomes lower than those commonly paid to natives, and if this were the only factor at work, inequality might rise.[4]

Third, the author regresses the concentration ratio against an "occupation index" derived by assigning arbitrary values to different occupations, such as one to artisans and five to factory workers. The regression coefficient is highly significant (though here as in most of his other regressions the author fails to give us the correlation coefficients), but it is difficult to assign any meaning to the index. One wonders why he did not regress the concentration ratio against some more general and less arbitrary indicator of economic development, such as some measure of real income.

Finally the author is inclined to regard his measure of income dispersion as a measure of the amount of incentive in the economy; the diminution in inequality, it is implied, has reduced incentives, *pari passu*. Of course, some dispersion of income is necessary to maintain incentives but there is no reason to suppose that changes in the size of pecuniary incentives can be gauged di-

[4] Incidentally, the rise in the concentration ratio for Vest-Agder County from 0.567 in 1840 to 0.732 in 1845 raises a question about the author's rejection (pp. 45-46) of Kuznets' speculation about an early stage of increasing inequality. Also the effect of the shift from the rural to the urban sector, which is not reflected in Soltow's inequality measures, was an important factor in leading Kuznets to suggest that inequality might increase before it declined.

rectly from the changes in dispersion. This is not, of course, to dispute the author's finding that the growth in equality in Norway has been associated with sharply reduced age and skill differentials.

However, the positive virtues of Soltow's volume far outweigh these criticisms. His book provides valuable insights into the associations between income distribution and economic growth. Our knowledge of these relations can grow only through further work like this, which will include the rural sector of Norway, and Oslo, and beyond this other countries. Meanwhile we have here an important contribution to the study of income distribution.

IRVING B. KRAVIS

University of Pennsylvania

Supply and Costs in the U.S. Petroleum Industry—Two Econometric Studies. By FRANKLIN M. FISHER. Baltimore: Johns Hopkins Press for Resources for the Future, Inc., 1964. Pp. xiii, 177. $5.00.

In this volume, Professor Fisher reports results from two imaginative empirical studies which he made of costs in the U.S. petroleum industry. The first concerns the relation between petroleum well-head prices and the number of new wildcat wells, the success ratio in wildcat drilling, and the average size of new discoveries. The second considers the relationship between drilling cost and well depth. Both studies are excellent examples of insightful, balanced, econometric analysis in which very crude data end up supporting a plausible set of hypotheses about the petroleum industry.

The principal conclusion in the first study is that although the price elasticity of wildcat drilling is quite high, 2.85, the price elasticity of the total volume of new discoveries is only about 0.31. The average size of a new discovery in a successful wildcat declines when prices rise, and the success ratio, the ratio of productive wells to total wells drilled, may also fall. While the theoretical explanation for these results is very similar to Ricardo's extensive margin, Fisher's empirical estimates are interesting.

Are the estimates reliable? Fisher's data consist of annual observations on each of the five national petroleum districts during the period 1946-55. Each of the three dependent variables is related to a deflated well-head price, geophysical and core-drilling time, average discoveries of natural gas, lagged values of the success ratio, the size of new oil discoveries, and drilling depth and dummy variables. Sampling across districts as well as over time is a convenient device for increasing the number of observations; well-head prices apparently vary considerably across districts on a given date. A minor shortcoming of this study is that Fisher neglected to display the small volume of data which he examines.

His analysis requires an assumption that intertemporal and interdistrict price variations are similar in their effect upon wildcat drilling and new discoveries. Fisher states that he is primarily interested in "long run" responses, although he declines to explain what long run means or why they should be interesting. From a policy point of view I would prefer to know how price changes will affect wildcat drilling during the next year or two, not in the dis-

tant future. Whether or not short-term predictions are necessary for policy, he concludes that sampling across districts on a given date tends to overstate estimates of long-run price effects.

In addition he uses lagged values of the success ratio, size of new discoveries, and drilling depth to take into account "district effects." (For some reason lagged new wildcats were ignored.) Lagged variables were also intended to measure "the underlying process which determines prospect characteristics and as indications of what wildcatters can expect, they have economic influences" (p. 21). The use of lagged endogenous variables will yield biased estimates of parameters if residuals are not serially independent. There can be little doubt that his residuals pertaining to a district will exhibit positive autocorrelation.

He also reports an attempt to allow for district effects with dummy variables, but does not publish the estimated equations on the grounds that they "make results strictly applicable only to the short run . . ." [and] ". . . the equations already presented may be taken to contain the most valid results" (p. 32). When dummy variables are included, the price elasticity of new wildcats falls to 1.5, which remains significant although all standard errors rise. Readers should be given the opportunity to decide which equations contain the most valid results.

The second study, concerning the relationship between drilling costs and depth, attempts to (1) estimate an obviously critical cost relationship in the petroleum industry, (2) determine how technological change affected this relationship over a four-year period, and (3) apply nonlinear estimation techniques. The basic structural equation is:

$$Y = K(e^{aX} - 1),$$

where X is the depth of the well in feet and Y measures cost per well in dollars (excluding overhead). Fisher's estimates suggest that (1) for a large number of drilling areas, costs at least double every 3500 feet, (2) technological improvements have more than offset any tendency towards diminishing returns in drilling and were particularly favorable to shallow wells, and (3) his nonlinear function fits the data well.

His data are from the 1955, 1956, and 1959 Joint Association Surveys of Drilling Costs. The function was fitted for each of a number of drilling areas. Apparently to prevent disclosure, these surveys could only be used in a very condensed form; a maximum of nine points was available for fitting any curve. Nevertheless, the approach and results are suggestive and should stimulate more work in this area.

In conclusion, it is easy to agree with the author that econometric analysis of trade association data should help when evaluating policy questions. However, crude data can at best provide weak evidence for such questions. Students of the petroleum industry can seriously challenge and refine Fisher's conclusions only by applying a similarly imaginative approach to better data.

DONALD HESTER

Yale University

Le coût de la vie à Paris de 1840 à 1954. By JEANNE SINGER-KÉREL. Paris: Librairie Armand Colin, 1961. Pp. 560.

The title *The Cost of Living in Paris from 1840 to 1954* seems to suggest an important but dull book, boringly loaded with statistics. Perhaps the following thoughts offer some comfort in approaching this 560-page book. To the extent that the cost of living in Paris is (or was) representative of the French cost of living, such a study would be of crucial importance to those wishing to study the process of French growth. The cost-of-living index of major cities is used as a price deflator for many a developing country. Numerous economists may find such indices useful. Some may be interested in French growth per se, others in testing the hypothesis "history repeats itself" in the context of a comparison between past French growth and present developing countries' economic change.

The book is well organized, a marked quality in such an endeavor. This attribute will make the book easily usable by the non-French-speaking researcher, and its main use will be as a statistical handbook. The first six chapters report the existing indices for various subperiods within the whole period and compare them with foreign indices. Then the methodology is discussed. In the two following chapters the indices are given, and their movements are compared with indices estimated from other sources. Then follows a study of individual items composing the whole. A chapter on the relative variations of prices within the whole ends Part I. The rest of the study incorporates four annexes. Annexes I to III cover notes, and Annex IV gives the data for the indices themselves. The care, precision, and conciseness with which the study is handled are highly commendable.

Familiarity with Chinese ghost stories might help sustain one's patience while exploring the maze of difficulties raised by index numbers. The Bureau of Labor Statistics' definition of the cost-of-living index is ". . . (in such an index) we would try to measure the changes in the cost of an equivalent market basket of goods and services whereas in a price index we try to measure the same market basket."[1] Presumably the first problem to face in such an endeavor is the form to choose for the index. The author chooses to make a set of Laspeyres indices with and without chaining. The Laspeyres index uses base-year weights. By making a series of binary comparisons between adjacent periods, the impact of the items excluded would be reduced. The Paasche index uses current-year weights. Either index introduces a bias, and it is believed that the "true" index would be the geometric average of the Laspeyres and the Paasche indices, i.e., Fisher's ideal index.[2] In fact the appropriate "true" index might be a constant-utility or welfare index. Whether or not such an index is meaningful or achievable is another question, which would be answered by P. A. Samuelson by an emphatically unambiguous no.[3] The problem of welfare is pervasive, even for the Bureau of Labor Statistics, as is ex-

[1] Sidney A. Jaffe, "The Consumer Price Index—Technical Questions and Practical Answers," *Am. Stat. Assoc., Proceedings*, 1959.

[2] See Y. Barzel, "Some Observations on the Index Number Problem," *Econometrica*, July 1963, *31*, 391-99.

[3] P. A. Samuelson, "Evaluation of Real National Income," *Oxford Econ. Papers*, New Ser. II, Jan. 1950, 1-29.

emplified in the following quotation: "There is one very important recommendation with which the Bureau of Labor Statistics cannot agree, even with modifications. This is the recommendation that the Consumer Price Index be reoriented gradually toward a 'welfare' or 'constant-utility' index. We would see some value in having a 'true cost-of-living' or constant utility index if techniques can be developed for defining such an index unambiguously, and then for compiling it objectively."[4]

The difficulties of welfare evaluation have been reduced in the book by the use of classes of spenders, i.e., workers (wage earners) versus the bourgeoisie. In this manner two sets of indices have been obtained. Each index comprises 213 items. Once the actual indices are obtained, their behavior over time is analyzed and explored. International comparisons with foreign indices (U.S., U.K., Sweden, Denmark, Belgium) are made in order to isolate the international influences from the domestic ones. Since M. Gilbert and I. B. Kravis' study for the OEEC in 1950,[5] it has become customary to consider the purchasing power parity problem in international comparisons. There is no trace of this in the book. The trend movements in the indices covering 213 items are compared with those of partial indices from other sources.

In order to isolate the important domestic influences on the cost of living, various coefficients (dependence, correlation, etc.) have been calculated between the cost of living and wholesale prices and wages, respectively. Presumably the idea is to isolate cost-push influences (wages) from demand-pull pressures (wholesale prices). Generally the results show that these influences vary from subperiod to subperiod. Besides the statistical computations of coefficients, there does not seem to be a satisfactory economic analysis of these aspects. There is also the problem of price variations owing to technological change. This aspect is not mentioned in the book.

Potential uses of the indices as suggested by the author in the introduction are distribution (debitor vs. creditor), capital formation (wages vs. other revenues), public debt and monetary policy, time perspective of contractual behavior, attitude towards risk, spending vs. savings. Judgment must be exercised in solving individual problems. For instance the cost-of-living index can hardly be used as a deflator for real income comparisons (see Samuelson). On the other hand, since the author has painstakingly collected an admirable amount of expenditure data, one wonders whether related research could not be pursued—for instance, the testing of revealed preference axioms (i.e., the "rationality" of consumers) à la Koo-Dobell,[6] as well as the investigation of the shapes of Engel curves.[7]

SIMONE CLEMHOUT

University of California, Berkeley

[4] Joint Economic Committee, Tuesday, May 8, 1961. Quoted in T. Yamane, *Statistics, An Introductory Analysis*, New York 1964.

[5] M. Gilbert and I. B. Kravis, *An International Comparison of National Products and the Purchasing Power of Currencies*, OEEC, Paris 1950.

[6] A. Y. C. Koo, "An Empirical Test of Revealed Preference Theory," *Econometrica*, Oct. 1963, *31*, 646-64; and A. R. Dobell, "A Comment on A. Y. C. Koo's 'An Empirical Test of Revealed Preference Theory,'" *Econometrica*, April 1965, *33*, A. Y. C. Koo, "Reply," *idem*.

[7] C. E. V. Leser, "Forms of Engel Functions," *Econometrica*, Oct. 1963, *31*, 694-703

Economic Systems; Planning and Reform; Cooperation

The Best Use of Economic Resources. By L. V. Kantorovich. Cambridge, Mass.: Harvard University Press, 1965. Pp. xxxiii, 349. $15.00.

The English translation of *The Best Use of Economic Resources* was eagerly awaited by bourgeois economists for a number of reasons. Economists with a special interest in programming and production theory wanted to honor the discoverer of linear programming by reading his own presentation of his discovery and to gain the insight that only the words of an original thinker can convey. Students of trends in Soviet economic thinking wanted to study at first-hand the position of Academician Kantorovich, winner of the Lenin Medal for meritorious scientific achievement and most eminent exponent of formal, mathematical methods of planning. The second group of students will find in, and particularly between, the lines of this book many valuable clues to the state of current economic thinking in the Soviet Union; the first group, those interested primarily in the economic significance of linear programming, will be seriously disappointed.

The book is neither text not treatise, but a tract addressed to Soviet economists and planners, exhorting them to more rational methods of economic calculation. What are these more rational methods? That is the key question, where both Kantorovich and his reviewer must proceed delicately.

For it must be recognized that Soviet methods of economic calculation are already based on a scientific and objective foundation, the Ricardo-Marx labor theory of value. This cannot on any account be denied or contradicted. Yet the labor theory of value, like any other doctrine, can be applied correctly or incorrectly, and it is Kantorovich's contention that it is being applied incorrectly in much contemporary analysis, leading, thereby, to substantial economic waste.

To summarize the situation: The labor theory of value holds that the value of anything is the amount of socially necessary labor time needed to produce it. If this statement be taken as a definition of value, it is innocuous but not very helpful. The doctrine, therefore, holds more: that goods and services tend to exchange at prices that are proportional to their values as so defined. These assertions are as true of labor-time as of any other commodity and, since less than a man-year of labor is required to support a workingman and his family for a year, a residue called surplus value arises whenever a laborer is employed. Under bourgeois capitalism this surplus value becomes the property of the employer. So it is that the theft of surplus value, i.e., the exploitation of workingmen, is inherent in the capitalist system of organization. I recall this familiar argument because it in effect defines the word "value." That word must be interpreted in a way that does not undermine the intellectual edifice that has been erected upon it.

One important amendment has to be made to my sketch of the labor theory of value. When I called it the Ricardo-Marx labor theory of value I meant early-Ricardo and early-Marx. Late-Ricardo does not concern us. But late-Marx, the Marx of Volume III, perceived that things were not quite so simple.

The organic composition of capital, that is the proportion of constant capital (plant, machinery, etc.) to variable capital (labor costs), is different in the production of different commodities. Only variable capital gives rise to the surplus value that can be appropriated by the capitalist exploiters. It would follow, therefore, if prices were proportional to values, that rates of profit would be different in different industries, which clearly is not so. So the notion that commodities tend to exchange in proportion to their values has to be relinquished. It is still true of labor, but when it comes to the commodities produced by labor under the aegis of capitalist employers the law of supply and demand takes over and prices adjust so that the rate of profit is the same in all industries. This amendment, however, does not affect the essence of the demonstration that exploitation of the workers is inherent in capitalism. For the effect of these price adjustments is merely to shift the attribution of surplus value from industries with higher than average organic composition to those with lower than average organic composition. On the average it is still true that commodities sell in proportion to their values. Just which capitalist appropriates the surplus value is not important; what is of the essence is the relationship between the classes and the fact that the capitalists, as a class, exploit the laborers as a class. Thus the late Marx did not hold quite so simple a version of the labor theory of value as the early Marx, but the essence of the early argument persisted. We shall see that Kantorovich has elaborated the labor theory of value even farther, perhaps dangerously far. But to do this we have to recapitulate two other threads of doctrine.

First it has to be remarked that vulgar, bourgeois economists do not reason in the way outlined above. They cannot, of course, ignore the fact that the goods produced by a worker sell for more than the wages he receives for producing them, but they interpret it differently. They attribute the surplus to the contributions of other factors of production, such as land, plant and equipment, and so on. By this intellectual device (the so-called marginalist or neoclassical theory of value), they purport to impute the entire price of a commodity to one or another of the factors of production that cooperate in producing it, so that the rate of profit (the excess of price over the costs of the factors of production, expressed as a percentage) tends to be zero. It is precisely against this attribution of value to factors other than labor that the staunch Marxist economist must stand firm.

The remaining doctrinal thread comes from linear programming itself, a mode of economic analysis which Kantorovich discovered in 1938-39 and George Dantzig rediscovered, in a capitalistic environment, in 1947. The odd thing is that Kantorovich's discovery had virtually no impact on economic thinking, in the USSR, or elsewhere, while Dantzig's rediscovery set off an enormous vogue. There are several reasons for this. Linear programming has both practical and theoretical implications. On the practical side, Kantorovich's discovery was a bit early and perhaps in the wrong environment, while Dantzig's rediscovery occurred in exactly the opportune time and place. As to timing, the implementation of linear programming requires extremely onerous arithmetical calculations, computations that can hardly be envisaged without the assistance of an electronic computer. The year 1939 was too early, but by

1947 adequate computers, though not yet on hand, were clearly in prospect. The first illustrative application of linear programming in this country was done laboriously by hand; the second was mechanized. As to appropriateness of place, the conclusion is a bit more conjectural. The method cannot be applied, even now, to the planning of so vast a system as an entire economy. It can handle a plant or, at most, a firm with a few plants. Hence it is adapted to a system of economic organization in which plant managers have the incentive, the resources, the authority, and the responsibility for devising and improving their operating plans. This seems to be more nearly true of a decentralized capitalist system than of a thoroughly rationalized socialist one. At any rate, capitalist managers quickly began experimenting with linear programming; socialist managers did not.

On the theoretical side there is a fundamental difference between the contexts in which the two discoveries occurred.

The principal significance of linear programming for economic theory is contained in the duality principle, which was discovered by A. W. Tucker, D. Gale, and H. W. Kuhn shortly after Dantzig announced his finding but was known to Kantorovich long before that. According to this principle, the direct solution of a linear programming problem yields an economic or business plan that employs the available resources as efficiently as possible and, at the same time, determines certain auxiliary variables (variously called dual variables, shadow prices, and by Kantorovich, objectively determined valuations) of great economic significance. One of these objectively determined valuations, to use Kantorovich's term, corresponds to each of the economic resources involved in the problem, and its value tells how much the economic value of the output could be increased if one additional unit of that resource were available. In slightly different words, the objectively determined valuations tell how much the economic unit for which the plan is constructed should be willing to pay for one additional unit of each resource and thus, in this bourgeois sense, correspond to the values of the resources. Furthermore, if the objectively determined valuations be interpreted as resource values per unit, the total value of the resources available to an economic entity is precisely equal to the value of its output.

Thus it appears that these objectively determined valuations have many of the properties of the marginal value products employed in bourgeois economic thinking. Indeed, it was early recognized that the objectively determined valuations are nothing but marginal value products,[1] and it was this insight that led to the great burst of interest in linear programming on the part of capitalist apologist economists. For it meant that the whole elegant mathematical apparatus of linear programming could be brought to bear on the problems of value (in the bourgeois sense) and general equilibrium. Linear programming became a powerful analytic tool for perfecting the Walrasian theory of general equilibrium in the hands of L. W. McKenzie, H. W. Kuhn, and a number of others, culminating in G. Debreu's *Theory of Value*.

By the same token that objectively determined valuations correspond to

[1] For a lucid, but not particularly early, exposition of the relationship, see T. C. Koopmans, *Three Essays on the State of Economic Science*.

value in the bourgeois sense they appear, at least superficially, inconsistent with the concept of value disclosed by scientific Marxist analysis. This explains why linear programming was greeted with far more enthusiasm by bourgeois economic theorists than by Marxist theoreticians. One of Kantorovich's main tasks is to show that this appearance of a conflict between objectively determined valuations and the labor theory of value is only superficial. If the labor theory of value is properly construed, he maintains, it will be seen that the computation of objectively determined valuations is the scientifically correct way to calculate values in the true Marxist sense. For example:

> The problem naturally arises as to whether they [objectively determined valuations] are in contradiction with the labour theory of value, according to which, under socialism as well, the value of products must be determined by the socially necessary expenditure of labour.
>
> Analysis of this problem shows that the structure of o. d. valuations associated with an optimal plan is in complete agreement with the labour theory of value; furthermore, the methods for finding these valuations provide an approach to calculating the full social expenditure of labour (p. 290).

This is one of the two main theses of the book; it is reiterated on pages 47, 62, 75, 105, 107, 121, and a number of other places that I neglected to note. The other main thesis is "... that a system of production valuations correctly constructed and conforming to real conditions is an effective means of analyzing the best use of available resources. . . . Such a method is superior to existing ones because many factors usually disregarded or only considered qualitatively are accounted for by quantity; as a result the choice of the solution conforms more fully to the national economic interest" (p. xvii). This conclusion is unarguable, and I shall not discuss it.

How, then, can the objectively determined valuations, which as a matter of implacable mathematics correspond to bourgeois marginal productivities, be said to be consistent with the labor theory of value? In the first place, Marx was perfectly explicit that "socially necessary labor time" includes both direct and indirect labor time, that is, along with the labor directly expended in the production of the commodity, the labor embodied in the machines and tools employed in producing it, the labor embodied in the machines and tools used to make those machines and tools, etc. Kantorovich insists, correctly, then, that the value of a commodity incorporates some components in respect of the capital equipment used along with labor time in producing it. But he goes beyond that.

Two issues have to be faced, corresponding to the two nonlabor members of the classical trinity of factors of production. When a linear programming problem is solved, it is likely to turn out that both capital and land have positive objectively determined valuations, and these have to be reconciled with the labor theory of value. Kantorovich handles both problems by the same device which is, unfortunately, nowhere stated very compactly. It amounts to valuing a unit of capital or land in accordance with the amount of labor time that can be saved, at the margin, by the use of a unit of that type of capital or that quality of land. For example:

> In problems of the use of equipment the amount of use in the manufacture of a given product or in any operation must be taken into account by making an allowance for the hire valuation of the equipment. Its magnitude, equal to the saving of labour obtained from an additional unit of equipment in an optimal plan, is determined by all the actual conditions... (p. 85).

And again:

> In solving problems of the use of natural resources which are more efficient but in short supply, their use must be determined by allowing for differential rent. The magnitude of the latter is determined by the saving of labour obtained from the use of these resources in the optimal plan (p. 99).

Furthermore, these imputed values (please forgive the bourgeois term) should be reflected in the prices of the commodities produced: "Consequently, the calculation of the rent should play an important role in questions of price formation" (page 99).

With these conclusions I can have no quarrel; the logic of linear programming (not to mention bourgeois economics) enforces them. But can the labor theory of value be extended this far? I maintain that it cannot or its major implication that exploitation of the workers is inherent in capitalism will be impugned.

Simply as a matter of doctrine, the concept that the value of anything is the amount of labor time that its possession enables the owner to save is not to be found in either Ricardo or Marx. It is much closer, indeed, to Adam Smith's concept that value is equivalent to a command over labor. It is closer still to the modern bourgeois, neoclassical view that the value of any productive instrument is equal to its marginal productivity, the only difference being the purely verbal one that Kantorovich has chosen to use labor time as the *numéraire* in terms of which to express the value of everything else. But who said what is not important; we are concerned with logical implications.

And the logical implication is this: If factors of production are valued in accordance with their objectively determined valuations, properly computed by Kantorovich's methods, then it will turn out, because of the principle of duality, that they absorb the entire value of the output. To be sure, the value of output will exceed the direct labor cost, it will even exceed the direct-plus-indirect labor cost. But the excess will not be due to capitalist exploitation or to the fact that workers can produce in a year more than is necessary to sustain them. The sustenance requirements of workers do not even enter into the computation. It will be due to the fact that the cooperating factors enhance the quantity (and thereby the value) of the output. Once these cooperating factors are credited with the values of their contributions, as Kantorovich recommends, no surplus value and exploitation remain.[2] It is because linear programming proves this so easily and clearly that bourgeois economic theorists are enthusiastic about it, Kantorovich also has good reason to be enthusiastic

[2] This argument abstracts from complications such as increasing returns to scale and indivisibilities, which Kantorovich also is content to ignore.

about it, but if this is the labor theory of value *Das Kapital* will have to be rewritten for it will not sustain the argument of that book.

In addition to expounding the economic implications of linear programming, Kantorovich includes a long chapter on the valuation of capital and the use of discounting. I do not have space to discuss this chapter except to note that on the whole Boehm-Bawerk would have approved of it. Kantorovich notes, for example, that in computing the labor-saving attributable to a unit of capital, labor saved in the future should be discounted in relation to labor saved in the present. "Such a reduction in the valuation of labour is, in actual fact, consistent with the statement that a rational expenditure of labour today enables one to obtain a much higher saving of labour in the future; and hence one must assign a higher valuation to a unit of labour for any given year than for the following one" (pp. 193-94).

The exposition throughout is in terms of artificial numerical examples which illustrate economic principles, but are too simple to even suggest the rationale and technique of linear programming computations.

Throughout this review I have dealt with Academician Kantorovich as a serious scholar who has offered a serious book, as is due a man of his eminence and accomplishments. But I should not be candid without confessing that at times I found it hard to believe that Kantorovich so seriously misconstrued the implications of the powerful technique that he has done so much to advance. It appeared to me that he was asserting the Marxist orthodoxy of the linear programming in the hope of reducing some of the resistances to his recommendations. This is the light that the book throws on the state of economic thinking in the Soviet Union.

ROBERT DORFMAN

Harvard University

Soviet Economic Processes. By EDWARD AMES. Homewood, Ill.: Richard D. Irwin, Inc., 1965. Pp. 257. $3.00 (college price, soft cover).

The aim of this book is to provide "an explanation of Soviet producing enterprises and the nature of the general economic equilibrium in the Soviet system." The basic building blocks for such a task are a microeconomic theory of the Soviet firm and an input-output type model of the financial flows between groups of institutions. From a set of relatively simple assumptions a series of models are constructed, and a set of testable hypotheses is derived. Relevant institutional materials are presented to give flesh to the models and to show the relevance of some of the conclusions. Although many objections can be raised against the various models, it is the most original treatment of the Soviet economy which has appeared in some years.

The microeconomic theory of the Soviet firm is built on the assumption that such firms are output maximizers, subject to the restraint that total revenues must be equal to, or greater than, total costs. From this assumption the author derives a series of propositions about the relation of average and marginal costs, the optimal use of productive factors, and the response of Soviet enterprises to changes in input or output prices. The role of enterprise plans is introduced through the concept of multiple prices in an interesting way. Re-

straints on the use of inputs which are placed on the firm by the plan can be shown to be a type of multiple-pricing arrangement. Similarly, the differential treatment of "above and below plan profits" can also be interpreted as a type of multiple pricing. The author then shows how multiple-pricing phenomena can still be handled within the previous models so that it is possible to analyze the joint roles of plans and prices in determining the level of output and the use of productive factors. Although the author also constructs a general equilibrium model from this basis, he stops short of proving that such a system is determinate.

The major advantage of such an approach lies in the attempt to show rigorously the interrelations between prices, plans, costs, and plan fulfillment. The author frankly recognizes that his microeconomic models are oversimplified, especially because of the neglect of the different complex incentive schemes, of risk factors, and of the interrelations of this year's plan fulfillment with next year's plan. But intensive examination of the implications of his models allows the author to discuss a number of important and hitherto neglected facets of the actions of the Soviet firm. Moreover, although the propositions generated from the models can be found scattered throughout the Western literature, the use of the models permits the logical interrelationships of these propositions to be more easily perceived.

In the chapters dealing with Soviet macroeconomics, the author starts with a simple four-sector (households, state budget, retail outlets, and enterprises) schema, defines a matrix of monetary flows between these sectors, and, with the assumption of certain fixed coefficients defining these monetary flows, derives a series of propositions about the relations between prices and output. Then, by assuming that the Soviet state is a monolithic entity with a real-valued preference function, he derives a series of welfare economic propositions about planning from a model related to this flow matrix.

In the macroeconomic area I believe that the advantage of the author's approach lies less in the formal models which are developed than in the analysis of the factual and institutional materials which must be covered. By looking at the Soviet economy as a series of financial flows, the author analyzes a series of institutions and the interrelations between these and other institutions which are seldom discussed in the Western literature on the Soviet economy. I found his discussion of the construction enterprises and of the investment process particularly informative.

However, the usefulness in explaining Soviet reality on the basis of certain of the models is often not very clear. For instance, in the discussion of financial flows, the author assumes certain fixed coefficients relating financial payments through the state budget and other output flows. But these "coefficients" are really quite variable and are the result of conscious policy decisions. Of course, the author rightfully points out that open input-output models are more useful in looking at Soviet reality, but he spends relatively little space analyzing these. On a different level, assuming the state to be a monolithic entity and positing some real-valued state utility function allows a number of interesting formal propositions of welfare economics to be made, but does not seem directly related to the stated purpose of the book.

Such a macroeconomic approach also poses a number of aggregation problems. For instance, relating output and plan goals provides insights on the microeconomic level. But applying such a function on a macro-level, especially when the interrelations of enterprise plans and capacity restraints are not fully clear, raises a number of difficulties.

Aside from the macroeconomic and microeconomic models there are a number of fascinating analyses of a number of seldom-discussed problems. The author presents a series of novel notions about the relations between administrative tiers, span of control, volume of decisions to be made, and the effects of introducing a probability element into the execution of plans. He also presents a very thoughtful discussion of the interrelations between physical and financial planning.

Finally, it must be noted that although the book has a number of profound insights, the reader also discovers some disconcerting errors. Confusion between increasing short-run average costs and long-run decreasing returns to scale (pp. 116-17) vitiates a whole series of propositions, the validity of which depends on the size of a particular variable. Similarly, in a geometric demonstration about the effects of rationing (pp. 94-95), he neglects the effects of a different placement of indifference curves (which would lead to directly opposite conclusions from those derived). There are also several small factual errors (e.g., "adjusted world market prices of 1950" have not been used in intra-East Bloc trade since around 1955).

But none of these criticisms is meant to detract from the author's important contribution—the first full-scale rigorous attempt to present a general theory of the operation of the Soviet economy. Whether one agrees with the usefulness of the models or not, the author's provocative approach and skillful integration of theoretical and factual materials make the book extremely stimulating reading.

FREDERIC L. PRYOR

Yale University

Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Development of British Monetary Orthodoxy, 1797-1875. By FRANK WHITSON FETTER. Cambridge: Harvard University Press, 1965. Pp. xiv, 296. $7.25.

This is a book in a generation. It surpasses in scope and documentation the comparable chapters in Viner's long-distinguished *Studies in the Theory of International Trade,* and it integrates a lifetime's flow of scholarship which has made Frank Fetter supreme in his field. It is most carefully constructed, with a terse, direct style that is occasionally arresting.

Fetter seeks to explain the formation of pre-1914 monetary orthodoxy, the influence of which persisted until the 1930's. It comprised gold monometallism with a fixed parity; a central bank, oriented to convertibility; and control of notes but not of deposits. This search involves instructive explorations of the history of theory, of monetary legislation, and of financial institutions. Thus,

the opening chapter on the monetary scene in 1797 is followed by chapters on inconvertibility, resumed gold payments, and the new gold standard of 1819; three central chapters discuss the consolidation of the gold standard, and legislative reforms; finally, two closely argued chapters scrutinize the acceptance by the 1870's of "the Bagehot principle" of acting before instead of after a crisis.

The strength of orthodoxy was its practical appeal; its ultimate weakness was its marshy intellectual base. Fetter pinpoints this in aptly suggesting that the appeal of Say's Law "probably was the result rather than the cause of monetary thinking"; the orthodox accepted its support for policies based on hostility to aristocratic government and to a myopic Bank of England. The gold standard was a deadly illustration. Although it had existed *de facto* for eighty years when suspended in 1797, eighteenth-century economists scarcely recognized the implications of monometellism; worse, their nineteenth-century successors used "economic theology rather than economic analysis" to justify the gold standard. An important explanation of this, Fetter might have stressed (he is obviously aware of it), was the infancy of professionalism, especially in economics, and the persisting English predilection for gentleman amateurism.

Orthodox adjustment theory muddled price rather than income effects. (Fetter discovers but one attempt at statistical verification.) The money-prices link was regarded as long term; short-term monetary action was none the less believed, correctly on the evidence, to have price effects, on stocks of goods rather than on current production, and on commodity rather than on factor prices. This weakens assumptions of nineteenth-century price flexibility; its echo of modern "stop-go" economics is a commentary on Britain's slow progress towards monetary management.

Unquestionably orthodoxy was rigid, but does Fetter inadvertently blur the rigidity of its chief inspirer, Ricardo? He doubtless underestimated the consequences of restoring sterling's old parity in 1819, but does Fetter grant enough weight to Ricardo's claim in a speech of 1823 that his underestimate had been of the "unnecessary . . . mischievous operations" of the Bank of England in botching convertibility? Subsequently he quotes from the same speech Ricardo's assertion that he cared less about a particular parity than about securing some sort of fixed standard. This was *ex post* wisdom—the owl coming out at midnight.

Years earlier Ricardo had advocated deflation, which brought the old parity so near by 1819 that, as in 1925, a last heave was widely preferred to devaluation. On all decisive occasions Ricardo deplored clumsiness but opted for rate rigidity.

Fetter demonstrates that central banking before 1840 was "hardly more than a footnote" to post-1844 developments. He etches subtly and for the first time the delicately evolving relationships between government and central bank. Accurately sensing the social atmosphere, he stresses both the Bank's haughty devotion to public ignorance of its activities and its counterpart in the exclusive chatting "in the clubs, at the country estates, at the meetings of old Etonians, Oxonians, and Cantabrigians, and possibly at the Political Economy Club." Unfortunately, this section displays Fetter's one weakness:

his emphasis upon policy and upon the stabilization of monetary legislation bypasses substantial changes in the monetary system. Discussion of tension between Bank and money market is inadequate without mention of the 1858 "Rule" which estranged them, or of the increasing disproportion between their respective resources. Likewise, though rightly rejecting the arrogant "Englishman's moral burden" approach to gold standard management and to international lending before 1914, Fetter underplays sterling's role as a reserve currency; may he not thereby underrate the international significance of Britain's avoidance of open sterling crises between 1866 and 1914?

Does the apparent anomaly of restricting notes but not deposits nag Fetter excessively? Majority thinking tied deposits to the supply of "hard cash," and consequently favored a single, simple control. Moreover, stress on time deposits rather than checking deposits left a weaker case for interference with apparently voluntary *ex ante* savings than for protecting the involuntary and often helpless noteholder. In fact, the government protected small depositors through the trustee savings banks and later by the Post Office Savings Bank.

This masterly book deploys an enviable knowledge of a massive literature. Two points invite correction. Like others Fetter neglects *Protection to Agriculture* in discussing Ricardo's interest theory; and Jevons *did* publish a suggestion for a better (tabular) standard, in Chapter 25 of his *Money*.

Here is outstanding scholarship, definitive, and durable. Fetter has so thoroughly reorganized our view of monetary orthodoxy that much rethinking of hitherto received truths is in order.

L. S. PRESSNELL

London School of Economics

Government Securities Market. By IRA O. SCOTT, JR. New York: McGraw-Hill Book Company, 1965. Pp. xiii, 239. $12.50.

Professor Scott has produced a highly informative, useful, and much-needed book on the dealer market in U.S. Government securities.

Chapters 1 and 2, which describe the composition and characteristics of government debt and the portfolio policies of the major investor groups, set the background for the description and analysis of dealer operations in Chapters 3-5. A strong feature of the background chapters (and Chapter 3) is the breadth of coverage and the fact that the author has brought together in one place detailed information on such technical matters concerning government securities as their terms and methods of issue, computation of interest, and tax characteristics.

Chapter 3 contains a description of the "Day-to-Day Operations of a Government Securities Dealer" from the vantage point of a professional economist who has (in his own words, but in a different context) "stood many times at a trader's side discussing complex questions of economic policy. . ." (p. 78, fn. 6). Scott covers some highly technical material—for example, in his treatment of how dealers make a market and the problems of financing dealer positions—with great expertise and in a highly exciting manner. The various phases of dealer operations are often described with, to say the least, a grand flourish. Not only do we see the mass of cold figures and computations that are essen-

tial to the firm's operations, but we also follow the trader as he "takes his lunch—bites of a cold sandwich and gulps of black coffee— . . . [and] takes dessert—tidbits of gossip and a spicy story from another trader . . . —while an itinerant bootblack shines his shoes" (p. 79).

The analytical core of the book is found in Chapters 4 and 5. In defining dealer performance, Scott states that the quality of a dealer's market is judged by the quantity of securities for which bid and offer quotations are firm and by the narrowness of the spread. Because of difficulties of measurement, however, the size of gross positions is used to measure market performance, since dealer positions tend to be larger when spreads are narrow and commitments are large. The determinants of position policy are classified in two categories, those affecting the expected return and those affecting the variance of that return. His discussion of the individual factors in each category is generally excellent.

Scott next constructs an abstract model of dealer position policy. The model is based upon the "expected-utility hypothesis, according to which the decision maker is assumed to maximize utility, which, in turn, depends upon a subjective evaluation of the function's moments" (p. 132). Because of the absence of information on dealer expectations, the model actually tested in Chapter 5 is one in which the independent variables selected serve as proxies for expected returns and the variance of expected returns.

Twelve dependent variables are used—aggregate long positions, short positions, and net positions in each of four maturity classes. Seven independent variables are used in each of the twelve cases. The empirical results are extremely difficult to summarize. In general, however, Scott seems to get his best results with a measure of the past rate of change of interest rates used as a proxy for interest-rate expectations (expected returns). This is especially true in the longer maturities. In addition, a measure of financing costs and the volume of dealer transactions, as an indicator of uncertainty, have some explanatory value.

In Chapter 6, the author considers some important questions of public policy largely from the point of view of the impact of these policies on the performance of the dealer market. On this basis, the Fed's bills-only policy receives good marks as it contributes to an improved market performance through the reduction of uncertainty. Included among the numerous suggestions for improving market performance are the extension of discount-window privileges to nonbank dealers, so as to provide them with needed financing facilities, and the pursual of a Treasury debt management policy aimed at a more regularly spaced maturity distribution of the marketable debt and a reduction in the number of issues outstanding.

Several critical remarks can be made. At times, Scott lapses into a somewhat uneven writing style and several technical passages are difficult to follow. Despite the 1965 publication date, considerable portions of the book clearly were written several years earlier, with the result that developments since 1961 or 1962 are treated only sketchily at best. For the academician, one of the most striking drawbacks of this slim volume undoubtedly will be its price—a healthy $12.50.

These minor shortcomings should not be allowed to detract unduly from an otherwise excellent book. The footnote references are thorough, and a 46-page bibliographic appendix is quite useful. Scott makes a valiant effort both to define the technical terms used and to provide, where possible, useful illustrations.

In general, the book makes an important contribution to our knowledge of the dealer market—although, as Scott himself points out, the empirical results of Chapter 5 are only tentative, being based on weekly data for a 14-month period beginning in October 1957. As position data are now available for more recent years, it is hoped that others will be encouraged by Scott's work to undertake a more thorough empirical study of the factors affecting dealer position policy.

THOMAS R. BEARD

Louisiana State University

Public Finance; Fiscal Policy

The Logic of Collective Action—Public Goods and the Theory of Groups. By MANCUR OLSON, JR. Cambridge: Harvard University Press, 1965. Pp. 176. $4.50.

The central thesis of this book can be stated in a series of propositions. An organization is a provider of some collective good to a particular public. In the nature of a collective good, no member of that public can be excluded from enjoying it. Hence any individual member of that public or group has no rational reason to join the organization voluntarily and to contribute to its costs of operation and supply. On the contrary, the rational individual will refrain from joining unless he is induced by sanctions or side benefits.

Mancur Olson recognizes that if this thesis is right it would seem to apply to organizations both large and small, and he inquires whether this is so. He finds that small organizations are often able to dispense with sanctions and inducements, and he explains this on the ground that when a member of a small group has a disproportionate interest in the collective good, the benefit he receives may be so large a part of the whole that he may be willing to pay the whole cost of providing it. Even in this case, however, the collective good will not be provided on an optimal scale, since its supply will be regulated by the satisfaction of the individual (or few individuals) paying the bill, and this is likely to be short of the point at which the group interest would benefit from the collective good—if only some were not so calculating as to try to turn it into a free good.

Hence the further conclusions that the larger the group, the less likely will be its organization, and the farther will it fall short of providing an optimal amount of a collective good.

"Some critics may argue that the rational person will, indeed, support a large organization, like a lobbying organization, that works in his interest, because he knows that if he does not, others will not do so either, and then the organization will fail, and he will be without the benefit that the organization

could have provided." But Olson argues that the analogy with perfect competition is apposite here: "in a large organization, the loss of one dues payer will not noticeably increase the burden for any other one dues payer, and so a rational person would not believe that if he were to withdraw from an organization he would drive others to do so."

The book suffers mostly from being overelaborated. It attempts to explain too much too simply, and tilts at too many analytical predecessors. The heart of the book is in the first chapter of 52 pages, and this is very much worthwhile. If the thesis is not as novel as the author perhaps believes, it has seldom if ever been stated so explicitly or so conceptually, and this is a genuine contribution. The remaining five chapters, which are applications or elaborations of the central thesis, add little and suffer by comparison.

It is easy to find points to quarrel with in this volume. A few examples should be enough to suggest the sometimes simplistic quality of the arguments. There is an underlying assumption that any participation in group action involves a cost, and hence an explanation is required as to how a latent group can be activated, since all its members, if rational, would refrain from joining in the expectation of getting a "free ride." Result: no organization, since in a large group those receiving disproportionate benefits are too few and the benefits too small a part of the whole to warrant their giving others the free ride which is hoped for. But what to Olson seems unlikely if not absurd—that some will work "free" for others—is sometimes the case; it is the activists and the cause-fighters who accept disproportionate expenses and may ask nothing much more than that latent members approve their initiative, that is, provide nominal (cost-free) support.

Exaggeration is involved in the assertion that exclusive groups (those sharing a collective good which is fixed in quantity, such as sales at a given price by an oligopoly) must be all or none. The author recognizes in a footnote that a noncooperating firm may not be able to threaten oligopoly collaboration because of its limited capacity or cost structure, but dismisses this as unimportant, despite the relatively few instances in which oligopolistic cooperation is 100 per cent inclusive. His interpretation of price warfare as an attempt to drive a rival or rivals from the industry ignores the significance of Fellner's quasi-agreement, under which not a reduction of "membership" in the group but acceptance of a joint code of behavior is the objective.

There is a tortured attempt to explain a differential function for the closed shop in small and large unions. The alleged nonoptimality of the level of provision of the collective good even in small groups rests on a shaky foundation: if a small number in a community have so strong a desire for a public park that they bear the whole cost themselves, who is to say that the community "free riders," who by hypothesis are less interested in parks, will not have their lesser interest fully satisfied by the park that the enthusiasts have provided?

These criticisms are not intended to demean the book's value but to indicate where that value lies—in a lucid if bald statement of the difficulties of securing individual support of collective objectives, rather than in the painstaking and sometimes unconvincing elaboration of the core thesis. Olson's book is a

significant and valuable contribution to the economist's attempt to come to grips with organizational problems.

NEIL W. CHAMBERLAIN

Yale University

Fiscal Neutrality Toward Economic Growth—Analysis of a Taxation Principle. By EDMUND S. PHELPS. New York: McGraw-Hill Book Co., 1965. Pp. ix, 113. $6.95.

This essay considers the question whether it is possible by fiscal and monetary policy both to maximize social welfare in a Pareto-optimum sense and at the same time allow the rate of economic growth in the private economy to be determined by the free choice of consumers through the market mechanism, thereby avoiding the need for a politically-determined growth policy. More loosely, the question is: Could society be assured full employment and the kind of income distribution it desired if the government took pains to be neutral with respect to the rate of economic growth (the latter being assumed to be a function of investment)?

The author argues that a combination of such fiscal neutrality and maximum welfare is theoretically conceivable both in a barter economy and in a modern money economy with a central bank, public debt, etc.—though only under highly restrictive assumptions. A major one of these is that taxes are confined to lump-sum, once-for-all kinds that do not bias the market's choices among goods and services and that do not reduce the desire to work, save, or take risks—i.e., that do not have substitution effects. But except for nonrecurring or at least unpredictable capital levies and head taxes, it is difficult to think of significant examples of lump-sum taxes nor does the author cite any. It is even more difficult to regard them as offering continuing sources of substantial government revenues without early sacrifice of their lump-sum character: a recurring capital levy, for example, would soon function much as a tax on income from property, and even as a comprehensive income tax if the capitalized value of income from personal services were included in the tax base.

Other important assumptions are that the information, skills, and political organization are available to permit government agencies to design the mix of fiscal and monetary policies perfectly, that such policies are instantaneously effective, that all markets are perfectly competitive, that there are no external economies or diseconomies in consumption and production, and that there is no conflict between the welfare of a present and subsequent generations.

Although the author early concedes that these assumptions are far from the facts, he devotes much of the book to showing how fiscal neutrality might nevertheless be approached in practice by a combination of budgetary, debt, tax, and monetary policies designed in part to compensate for unneutral taxes and other departures of the real world from his model.

In this process, he makes a valuable contribution to the theoretical literature by examining various circumstances in which a balanced budget, the existence of a public debt, debt-financing by government of capital outlays, etc., might be neutral or unneutral with respect to economic growth; and by showing how some of the well-known criticisms of *laissez-faire* capitalism with re-

spect to economic growth, such as underinvestment by reason of greater private than social uncertainties and risks, apply to modern mixed economies, and how the grounds for such criticisms might be reduced by fiscal and monetary policy.

Despite his recurring advocacy of the merits of a neutral fiscal policy and the pains with which he examines the theoretical possibilities of achieving it, he reluctantly concludes in the end that "there is no hope for the optimality of so simple a policy as neutrality in a complex world," and that in such a world "there seems to be little merit in a policy of neutrality."

The tenor of these conclusions on the last page of the essay is curiously at variance with the previous stout defense of neutrality as at least an approximate goal. Moreover, since the essay's point of departure is a dissent from the views of Abba Lerner, Paul Samuelson, and others that an intelligent modern government must have a politically-determined growth policy in the sense of at least loose growth-rate targets, the essay's conclusions would seem to rob the dissent of practical significance. Nevertheless the essay is a stimulating one that fills a number of gaps in the theoretical literature.

The inclusion of this book in a series of "economic handbooks" merits the caution that much of the detailed analysis, though not the broad reasoning, is algebraic in form and presupposes more than a beginner's knowledge of economics.

LAWRENCE H. SELTZER

Wayne State University

Studies in Greek Taxation. By GEORGE F. BREAK AND RALPH TURVEY. Research Monograph Series No. 11. Athens: Center of Planning and Economic Research, 1964. Pp. 250.

This study is an outstanding contribution to the literature, both for its account of the Greek tax structure and as an addition to the all too scanty works on taxation in developing countries.

The book is in two parts, entitled, respectively, "Description" and "Analysis." Part I, however, is a good deal more than bare description, and Part II, which is mainly a study of particular taxes, contains a good deal of further description. The work ends with excerpts from incentive legislation for industrialization, which in variety and extension would appear to outdo most other countries.

After an account of the Greek (very centralized) constitution, Part I is occupied with a description of the components of the tax structure, which together amount to 22-24 per cent of the GNP. Well-chosen illustrations of the working of particular taxes make this easy reading. (For instance the Stamp Duties on a particular transaction may require the licking of 40 stamps, amounting to some 1100 drs.)

The Greek tax structure is a fascinating medley of the modern and the archaic. The income tax was reformed some ten years ago and the profits tax more recently. Moreover, fairly adequate ways of taxing farmers have been devised, by means of a graduated personal tax (similar to that of East Africa) and a sales tax on certain farm products. These sensible taxes are, however,

accompanied by an incredible array of complicated little levies which are certainly not worth their keep.

The two outstanding peculiarities of the revenue structure are (i) the practice of assigning (earmarking) almost all revenues for particular purposes, not just straight, but with little contributions from different sources; (ii) the exceptional importance of social insurance contributions (a third from workers and two-thirds from employers). These contributions and the funds that administer them form 25 per cent of the activities of the government. There are some 200 separate funds, financed in part by small assignments (in the case of the farmers' O.G.A. very largely so, implying that townspeople pay most of the farmers' insurance). Since there is no general policy in respect of the funds, fortuitous surpluses from some of them are a continuing temptation to extraneous expenditure. More serious, "the figures reveal neither the total revenue collected by the central government, nor that part which is available to be spent by it."

Like other Mediterranean countries, Greece relies heavily on taxes on outlay: for 69 per cent of the revenue, if social insurance contributions are excluded as being only partly tax. Of this revenue, customs duties are responsible for 22 per cent, general sales taxes for 23 per cent, and selective sales taxes for 55 per cent. The effects of excises and turnover taxes have also to be considered. In Table 4.3 (p. 134) an attempt is made to estimate the allocative effects of this aggregated taxation. Tobacco, alcohol, entertainments, and vehicle circulation are all highly taxed, well in excess of the industries' contribution to the GNP. At the other end of the scale such important industries as shipbuilding, building and contracting, newsprint, and fertilizers are very lightly taxed.

These differences are often due to discriminatory rates of turnover tax, an important (16 per cent of commodity taxes) but peculiar feature of the Greek tax structure. This is assessed on output less cost of materials so is not cumulative. Exports get very ample rebates at all stages. Lower rates in the Provinces and still lower in the Islands provide a fiscal incentive to decentralization. Owing to rebates and exemptions, revenue is mainly collected from some 3000 fairly large enterprises. Finally, the turnover tax is integrated with the income tax, which together with inheritance and gift taxes account for 13 per cent of the revenue (17 per cent if social insurance contributions are excluded). As mentioned above, income and profits taxes have a relatively modern structure and are well integrated. But assessments and collections are badly eroded. After no more than ten years, there are 423 causes of exemption from the income tax.

The last part of the book is concerned with priorities for reform, the need for most of which would be readily apparent if more statistics were available. Thus a family expenditure survey is needed to determine the incidence of dependents' allowances and of deductions for medical and dental expenses. In respect of commodity taxes "nobody knows anything about the joint pattern." Effective compensatory finance would therefore be impossible.

The most interesting reform suggestion is concerned with direct taxes: income tax, capital gains tax, and a tax on net wealth. Marginal rates of income

tax are high to the point of disincentive, especially at the point of initial liability. On the other hand, average rates are low, so that the whole course of the tax is little more than proportional. The suggestion is that this should be frankly recognized and the progressive element of the tax structure transferred to a capital gains and wealth tax. Apart from the limited effectiveness of a capital gains tax in a situation where most of the gains will be made in the difficult valuation field of real estate and not on the stock exchange, the second suggestion calls for very careful scrutiny.

Chapter 6 of Part II, where plans for a net wealth tax are set out, is of exceptional interest, if only because it is a valiant attempt to think up methods of valuation in semideveloped conditions. For real estate three methods are suggested: (i) future discounted earnings, (ii) original cost less depreciation, and (iii) contemporary sales values. It is suggested that in respect of the last (which clearly begs the fewest questions), the government might deliberately increase the volume of sales in order to widen the market. Where equipment is an important element in the value of the asset, it is suggested that total values can be checked against income tax returns, capitalized at an appropriate rate of interest. (One wants to know both whether income tax returns would stand up to this responsibility and how the appropriate capitalization rate would be chosen.) The difficulty of valuing consumers' durables would be bypassed by substituting a special excise (10 per cent rate of this corresponding roughly to a 1 per cent rate of wealth tax). To deal with intangibles all companies would have to furnish lists of shareholders, with their several interests, and banks the balances of their customers. Finally human capital might be included by requiring each taxpayer to capitalize his expected stream of earnings. (He would be supplied with tables for the purpose.)

It is an ingenious program, but its practicability is, to say the least, somewhat doubtful. It should be borne in mind that the much vaunted wealth taxes of the Teutonic countries turn out on examination to have become eroded to little more than taxes on real estate, whatever the original intention. (See the Symposium from countries practicing them in "Public Finance" 1960.) If the Greeks have allowed their income tax to become as eroded over ten years as has happened, there seems small hope that the much more complicated tax on net wealth would fare any better.

It is claimed that the net wealth tax would lead to a better use of resources, especially of underused land and the maintenance of excessive inventories. But surely there are simpler and cheaper ways of attaining these ends. A firm determination to abolish most of the exemptions in the income tax and an increase in the number of steps in marginal rates would go a long way, if they were backed up by better administration. The greatest need in Greece, however, would seem to be the introduction of a comprehensive annual budgetary review which would reveal on the one side the total fiscal outlay of public bodies and on the other give an account of the total tax pool, with a drastic reduction in assignments, so that the interrelation of tax incidence and effects could be estimated.

URSULA K. HICKS

Oxford University

Patterns of Public Sector in Underdeveloped Economics. By IGNACY SACHS. New York: Asia Publishing House; Taplinger, Distributor, 1964. Pp. x, 196. $7.75.

This revised and translated version of a book published in Poland in 1961 is devoted to assessing the effectiveness of alternative types of government policies for carrying out economic development in the contemporary underdeveloped world. Three main lines of argument are developed: (1) an explanation of why there is a large development gap among nations; (2) an evaluation of the relative productivity of the private capitalist and public sectors in generating accumulation; and (3) a comparison of two particular types of public sector patterns. In the first of these, called the Japanese type, the state nationalizes social overhead production and assists in development financing through such devices as state investment banks, but does not extend economic planning beyond fiscal policy and sectoral investment policy, and encourages both the import of foreign capital and the development of an indigenous capitalist class. In the second or Indian type the government nationalizes a number of strategic industries so that public sector growth will be higher than that of the private sector, thereby braking the power of monopoly capital, while the economic independence of the country is fostered, and comprehensive planning, physical as well as financial, is practiced.

The results of the analysis are that: (1)underdevelopment around the world has common origins and causation in the interaction between capitalism and the colonial systems; (2) classical patterns of development are not open to currently underdeveloped countries because of the very different world market conditions these countries face than did their predecessors, and because of the lure of a short cut to development provided by the experience with socialist industrialization; (3) the economic and political structure of underdeveloped countries makes high rates of private accumulation virtually impossible, so that the government must take the lead in mobilizing resources if there is to be rapid growth; (4) state capitalism can thus be a relatively progressive phenomenon provided the government recognizes and accepts the role it must play in development; (5) whether the Japanese or Indian type will be chosen by such governments depends essentially on the relative political power of the native bourgeoisie and the forces of social democracy. To these conclusions are added value judgments, as Dr. Sachs calls them (apparently because they are not defended in this work), to the effect that the Indian pattern should produce a higher growth rate than the Japanese pattern, that the Japanese pattern can never be wholly successful, and that the only really effective solution is neither of these but the socialist pattern.

Of the three main lines of argument the first certainly gets the weakest treatment, being well below the standard for this genre set by, say, Paul Baran. The comparison of the two government policy approaches contains a good deal of stimulating comment on interest groups in underdeveloped countries. Unfortunately Sachs has not succeeded in finding a country that actually practices the Indian pattern. Certainly India does not, as he points out. One is left with the impression that there is a good reason for this, though Sachs does not discuss it explicitly. Any attempt to try the Indian pattern creates strong

resistance from the soon-to-be-expropriated capitalists, leading to political tension and either revolution or counterrevolution, that is, very roughly, a switch either to socialist development or to the Japanese pattern. Perhaps it is in this sense that the Indian pattern is more progressive than the Japanese, rather than, as Sachs implies, because it is more productive of growth; but that is a matter for the reader's political tastes to settle.

Probably the most interesting thing about this book is the fact that it represents a fairly serious attempt to develop for contemporary underdeveloped countries an analysis of economic interest groups and their political and economic impact, in the tradition of Marx's "Class Struggles in France," but without the invective. The native bourgeoisie, if not foreign capital, are granted in principle the ability to act in ways that foster development, and a substantial part of the book (Chapters 4 and 5) is devoted to assessing the circumstances under which state policies can be made consistent with the interests both of development and of the bourgeoisie. Though many readers will find the arguments naïve, Dr. Sachs's work nonetheless represents a further step toward more effective communication between Marxist and non-Marxist economists. If the former could profit from a more extended use of the analytic and methodological apparatus of contemporary Western economics, the latter could no doubt profit by following Sachs in taking on occasion a broader look at the social and political context in which their work finds its application.

Benjamin Ward

University of California, Berkeley

Problems of Tax Administration in Latin America—Papers and Proceedings of a Conference Held in Buenos Aires, Argentina, October, 1961. Baltimore: The Johns Hopkins Press for Joint Tax Program: Organization of American States, Inter-American Development Bank, Economic Commission for Latin America, 1965. Pp. x, 574. $10.00.

This book, finally published after what seems a rather long delay, contains nine papers, together with a summary discussion of general issues. A useful feature is the publication of extended comments by Latin American fiscal experts, based on prior reading of the papers. These comments, together with the remarks of the discussants, bring into focus the gulf that inevitably exists between the preoccupations of North American experts, whose sophisticated analysis of administrative problems relates to a mature economy, and the immediate problems confronting most of the Latin Americans to whom the papers are addressed. A further dividend to the reader of these comments is the opportunity thus provided to form his own conception of the present state of administrative efficiency in various Latin American countries as seen through the eyes of the country expert, and to estimate the likelihood that recommended changes in procedures will soon be accepted. This information is not conveyed in the major papers, which are (understandably) closely geared to the particular competences of each speaker.

Most of the papers deal with technical problems of tax administration, but several concern themselves with broader questions of taxation and the charac-

teristics of particular taxes. Richard Musgrave discusses the problems of estimating the distribution of the tax burden. The nexus between Musgrave's study (which indeed devotes several pages to specifically Latin American problems) and tax administration is that a high standard of administration is a precondition of dependable studies of the distribution of the tax burden. John Due summarizes the problems of administration of sales and excise taxes. Both he and his commentators limit themselves to a general, rather than a "Latin American," approach to the question. Lyle Fitch deals with concepts and administration of taxes on property, and his commentators, as well as the discussants, devote attention to relating general considerations to specifically Latin American problems. In general, the discussants throughout the Conference show an especially lively concern for relating principles to local problems.

The rest of the papers are closely geared to the title of the book. Joseph Crockett discusses Latin American difficulties in tax administration, difficulties that make familiar reading to anyone who has had the opportunity to visit a Finance Ministry in any Latin American country. These are limited enforcement powers, inadequate supervision of the taxpayer, prevalence of noncompliers, complicated tax forms, too much faith in audit as contrasted with a full investigation of the returns selected for examination, and so on. In a comment, Mr. Piedrabuena points out that a training school for tax personnel was already functioning in Chile in 1961, under collaboration between the International Cooperation Administration and the U.S. Internal Revenue Service. A useful addition to the book would have been a statement of the substantial progress that has been made by these missions in other Latin American countries as well since the date of the Conference. The reviewer has had the opportunity to observe the dedication of the IRS people at work on this task in Lima.

An interesting paper by Marius Farioletti, Assistant Director of the Planning and Analysis Division of the IRS, is concerned with "Statistical Records for the Management and Control of Tax Administration." Useful advice is given on the rules-of-thumb that should be followed by administrators occupied with developing a system of records, and although reference is almost entirely to the problems as they are met in the United States, they are problems that tax experts in less developed countries soon will want to be thinking about. He stresses the nature of the "tax administration gap" (pp. 141ff.), and its components, as well as the costs and advantages of partially closing it.

Stanley Surrey discusses automatic data processing, rightly stressing the extent of the preliminary work that must be done before ADP can be introduced. A commentator from one of the smaller countries interestingly suggested that some of them might have to *share* a computer. Chadwick Haberströh deals with trends in management technique, stressing that a problem in Latin America is the attempt to impose impersonal tasks on organizations "while the matrix of their society remains person-oriented" (p. 226). Attention is called to the advantages of the "flexible, task force style of administration" (p. 235). Incidentally, Haberströh deplores limiting attention to the measurement of tax evasion (p. 241), and indicates the need for consideration of other

administrative functions as well (among them, ways in which the administra tion itself might improve on the equity treatment of taxpayers). Accountin and auditing procedures are discussed by Charles Taylor, and further discus sion of controlling income tax evasion is provided by Oliver Oldman.

Throughout much of the volume run two themes. One is that any nation tax system must be related closely to economic and political institutions an to the tax morale of the public. The other is that the equity and effectivenes of a nation's tax system are likely to provide an index of the efficiency in gen eral. Perhaps it may be suggested that another contribution to effective ac ministration would be to limit the extent of use of tax incentives to attract ir dustry and to stimulate investment which are often of doubtful effectivenes in any event.

KENYON E. POOLE

Northwestern University

International Economics

Reforming the World's Money. By SIR ROY HARROD. New York: St. Martin Press, 1965. Pp. viii, 181. $5.95.

Monetary Reform and the World Economy. By ROBERT V. ROOSA. Ne York: Harper & Row, 1965. Pp. 1x, 173. $1.95.

Both of these books stress the need for international monetary reform. The differ, however, about the immediacy of the need. Sir Roy Harrod emphasize that the succession of near-crises in major currencies in the early 1960's mean that there is currently a large shortage of reserves. Robert Roosa, in contras believes the system has performed reasonably well and that negotiations ar necessary to provide for the increases in reserves necessary to finance growin trade and payments. Roosa has been a prime mover in negotiating new finar cial arrangements—the General Arrangements to Borrow, the currency swap among central banks, the new U.S. Treasury securities denominated in foreig currencies ("Roosa bonds"), and in devising "rescue packages" for foreig currencies in distress (the pound, the lira, and the Canadian dollar); it woul be unexpected if he indicated that the existing set of arrangements was ser ously lacking in its ability to meet current needs.

Harrod pronounces the *ad hoc* arrangements set up in the last few years ex cellent, but he doubts they can be enlarged and formalized to the extent neces sary to finance protracted payments imbalances. Roosa's view of the immedia cy of the need for reform appears conditioned by his recent experiences nego tiating with foreign central bankers. The key words for Roosa are coopera tion, confidence, stability, sovereignty, and flexibility. Harrod challenges th central banker's view of the world: "The trouble is that the training and hab tuation of central bankers is to be pragmatic and to move step by step. The distrust ideas of a general character, and this way may be wise in the norma run of things, since it is quite correct to think that the most general ideas ar dangerous." But for Harrod, obtaining the desired increase in reserves fal outside the normal run of things. Harrod wants to revive the atmosphere tha

prevailed at Bretton Woods and suggests that this might be done by demonstrating the costs of a generalized reserve shortage in terms of foregone growth.

The Roosa volume, an expansion of the Elihu Root lectures in 1965, states the conditions for monetary reform and provides a comprehensive catalog of the possible ways to create new international reserves. The essential conditions include maintenance of the $35 gold price, fixed exchange rates, and a continued U.S. role as world banker. He suggests ways to improve the functioning of the system short of reform—partly by the expansion of the *ad hoc* arrangements developed in the last several years and partly by greater use of IMF financial resources, made possible both by an enlargement of IMF quotas and easier access to foreign currencies in the Fund. On the reform agenda, Roosa proposes creation of a new asset ("The Fund Unit Account") within the IMF structure; this asset would be similar to the Composite Reserve Unit proposed by Edward M. Bernstein, and would supplement holdings of dollars, sterling, and other assets in international reserves. Roosa suggests rules to be used in determining which countries can acquire such assets initially and which countries must accept them in settlement of imbalances, and in what amounts. He also proposes creation of new secondary reserve assets, in effect, long-term "Roosa bonds," to be transferred between central banks. Roosa's theme is that there are a number of ways to create assets to meet the need for reserves, and it is self-defeating to discard any possible ways. Reserve needs can be met best by holdings of gold, dollars, sterling, and other currencies; by central bank currency swaps; by long-term and medium-term loans between central banks; and by claims on the IMF in the form of Fund Unit Accounts, and larger member-country drawing rights.

Harrod stresses that since the adjustment mechanism works slowly with fixed exchange rates and since flexible exchange rates would lead to international monetary chaos (a view shared by Roosa), a large increase in reserves is needed if direct restrictions on international payments are to be avoided. Much of the needed increase in reserves could be met through a world-wide increase in the price of gold. (Roosa wants to maintain the $35 gold price, but his argument appears directed largely against a devaluation of the dollar in terms of other currencies rather than against a devaluation of all currencies in terms of gold.) Harrod explores the potential of mutual currency support arrangements and deems them much more useful to the United States and several other countries with high international credit standings than to the rest of the world. He argues that it is time for a review of the IMF Articles of Agreement, since they were hastily drafted in an atmosphere dominated by the prospect of continued European inconvertibility. Harrod examines a variety of ways to gear the IMF to meet reserve needs better and recommends that quotas should be enlarged, either on a once-and-for-all basis or on an annual and automatic basis, and that the Fund should be empowered to make investments in member countries so that their claims on the Fund will be increased. The dollar and sterling would continue to be held in international reserves, along with gold and the claims on the Fund.

Finally, and most importantly, Harrod wants to convert the IMF from op-

erating on an overdraft basis to operating on a deposit basis. Payments imbalances would be settled by changes in the entries in the Fund's deposit ledger. If, for example, Great Britain had a payments deficit, it might draw a sterling-denominated check on its deposit at the Fund, and present the check to the U.S. Treasury in exchange for dollars. The dollars would be used to support sterling in the exchange market. The United States would then transfer this check to the Fund. The Fund would debit the British account, denominated in sterling, and credit the U.S. account, denominated in dollars. Later the United States could draw a dollar-denominated check on the Fund to present to other countries to obtain their currencies to help finance a U.S. deficit. Harrod's proposal for reform is similar to Keynes's Clearing Union and Triffin's Central Bank in that payments imbalances would be settled by transfer of deposit claims on the Fund; it differs from their proposals largely in that each country's deposits in the Fund would be denominated in its own currency. And these deposits in the Fund closely resemble Roosa's Fund Unit Account. Roosa, however, advocates that the Fund still hold currencies of its members; in effect, he wants to create a deposit window within the Fund alongside its overdraft window, while Harrod wants to replace the overdraft window with a deposit window.

Harrod believes that the world's need for liquidity could be met by adoption of his proposals for reform of the IMF. An international central bank or a new international currency are not necessary; neither is an increase in the gold price.

The major difference between Harrod and Roosa is not in the choice of techniques for generating new international reserves, but rather the disagreement on the tests for the adequacy of reserves. Both acknowledge the relation between the process of adjustment to imbalances and the need for reserves to finance imbalances. Neither, however, really specifies the nature of this relationship. When Roosa suggests that the recent rapid expansion in world trade in recent years denies a reserve shortage, he ignores the heavy emphasis on restrictive measures that are increasingly characteristic of policies for balance-of-payment adjustment. Harrod views the several recent near-crises in the system as evidence of the over-all shortage of reserves; these crises, however, may reflect inadequate efforts to achieve payments balance on the part of countries with large persistent deficits, rather than a shortage of reserves available to the world.

The core of the reserve adequacy issue involves the relative obligation on countries with payments deficits and those with payments surpluses to take measures to reduce the imbalances. Harrod believes that adjustment to imbalance should consist largely of price rises in countries with payments surpluses; reserves must be adequate to finance the imbalance until the adjustment occurs. For Harrod, growth, unrestricted imports, and larger reserves are preferable to price stability, import restrictions, and smaller reserves. Harrod challenges the central bankers' view that the shortage of reserves provides a restraint on inflation in the countries with payments deficit. From the view of central bankers in the surplus countries, however, a reduction in the imbalance, whether attributable to deflation, devaluation, or import restrictions by the deficit countries, reduces the size of external inflationary pressures. But if, as

Roosa suggests, the creation of reserves depends on agreement among central bankers, then the desire by surplus countries to limit external inflationary pressures forces the burden of adjustment on the deficit country. The resolution of this issue depends on agreement on how this burden shall be divided, and not on some abstract measure of reserve adequacy.

ROBERT Z. ALIBER

University of Chicago

Government Risk-Sharing in Foreign Investment. By MARINA VON NEUMANN WHITMAN. Princeton: Princeton University Press, 1965. Pp. xii, 349. $8.50.

Professor Whitman has written a lucid and perceptive account of six programs that share the risks of foreign investment with private U.S. banks and business firms. The programs are the Investment Guaranty Program operated by the successive U.S. foreign aid agencies; the World Bank; the International Finance Corporation; the Inter-American Bank; the Export-Import Bank; and the Development Loan Fund, a separate U.S. government agency between 1957 and 1961 and thereafter a part of A.I.D.

Each is assessed against a single criterion—its direct participation in the risks assumed by private capital. It is this criterion that gives the study its focus and its interest. The results are particularly illuminating in the case of the first five programs which were indeed designed to encourage U.S. private investors and traders to participate more actively in international economic life.

The concluding chapter offers tabular evidence on the relative importance of each program in stimulating the flow of private U.S. capital. The author concludes that none of them has been a meaningful catalyst, since two to four public dollars have been required to move each private dollar. This leads to a final observation that "viewed as a means of substituting private for public development financing on a large scale, the activities here discussed appear not only futile but actually dangerous to the national interest in their very inadequacy."

Analysis of each of the programs occupies the bulk of the book and represents its most satisfying contribution. The author reflects with precision and insight the evolution of policies and programs under the pressure of criticism and controversy. As an inexperienced U.S. government sought to bring its economic capacity to bear on the problems of the postwar world, much creative ingenuity was used to devise politically acceptable mechanisms. With time, misconceptions have been corrected and fears have been allayed. Both policies and legislative authority have been revised constantly—largely for the better.

The author has been particularly discriminating in selecting and soliciting relevant information. Her account will be valuable for those concerned with the inevitable changes in policies and administration. It should also be useful to businessmen interested in availing themselves of the ample opportunities for participating in the high returns that prevail on investment in less developed nations, while sharing the risks with their government.

The rest of the book is tantalizing. Material relevant to a number of current

policy issues is presented. However, even where the issues are discussed directly, the evaluation is incomplete or inconclusive. Should the proposed 30 per cent tax credit be enacted and the extended risk-guaranty program substantially expanded? Both proposals have been debated for some time and A.I.D.'s Advisory Committee on Private Enterprise in Foreign Aid has recently recommended their adoption. The author discusses both but stops short of a full appraisal. She also suggests a functional reorganization of U.S. bilateral programs, separating them according to their role in promoting capital flows. The same criterion might fruitfully be used to establish a division of labor between the World Bank and the bilateral aid programs or between the World Bank and the Inter-American Bank. It is these latter organizational problems that currently occupy the center stage.

The nature of the complementarity between public and private capital flows is treated narrowly. The book's preoccupation with the direct participation of private capital in the operations of the agencies under review stimulates questions about the indirect effects on private flows of foreign aid using public funds alone. The study notes the balance-of-payments problems created for developing nations by debt service requirements. If it were not for government grants and soft loans, would such developing nations as Brazil, India, and Turkey have been able to maintain the spotless repayment record of borrowers from the World Bank? Would private investors, U.S. and European as well, have been able to repatriate earnings and capital to the extent they have? Do such service requirements increase the need for government aid or, as is commonly argued, do private capital flows decrease aid requirements? Professor Whitman's study has the merit of provoking such queries.

It may well be that the principal "risk-sharing" contributions of the U.S. government emanate from the indirect effects of its foreign aid programs. Easing foreign exchange shortages facilitates service on private risk capital and the known preference of the U.S. government for private capital movements assures reasonable treatment of private risk-takers by most recipients of U.S. aid.

JACOB J. KAPLAN

George Washington University

Economic Aid and International Cost Sharing. By JOHN PINCUS. Baltimore: The Johns Hopkins Press, 1965. Pp. xvii, 221. $6.50.

This book, originally a Report of the Rand Corporation to the Department of Defense, deals with the need for more foreign aid, the problem of measuring the "aid" in foreign aid, the task of sharing the costs of foreign aid among donor countries, and the prospects for giving aid through commodity agreements.

After a brief review of "import gap" analysis, John Pincus concludes that although they cannot be precisely measured, the foreign aid requirements of the less developed countries by 1975 will increase from present levels by at least $5 billion a year, net of debt servicing payments, if the less developed countries are to achieve per capita income increases of 2.5 to 3.5 per cent a year. This represents a large increase over present aid levels. How should the

costs of this large increase in foreign assistance be shared? And what form should the assistance take?

To allocate fairly the cost of foreign aid, one must first decide how to measure it. Pincus strongly deprecates the widespread practice of indiscriminately adding together cash grants, commodity grants, long-term loans, and medium-term export credits to reach a total level of "aid." He argues that commodity grants should be valued at world market prices, not at artificial support prices as is now done for some P.L. 480 shipments, and that loans should be entered minus their discounted future value, not at their nominal value. Rough calculations, using national interest rates to discount loan repayments, show that a nominal level of aid amounting to $7.8 billion from all donor countries in 1961 shrinks to $4.8 billion if the two indicated corrections are made.

Pincus argues that the benefits of this foreign aid to donors are too diffuse and too unmeasurable to offer a satisfactory criterion for cost sharing. He is thus driven to emphasize the donors' ability to pay, which he discusses in terms of total GNP, GNP per capita, level of unutilized productive capacity, balance of payments position, and availability of surplus agricultural production. He concludes that any search for an ideal cost-sharing formula will be will-o-the-wisp, but suggests that an ability-to-pay criterion based on GNP corrected for national differences in purchasing power offers the most fruitful starting point for negotiations among donor countries.

In correcting GNP for purchasing power differences Pincus erroneously follows the current vogue. The temptation to correct national income for differences in national purchasing power is a strong and a natural one for anyone who has experienced the large variation from country to country in how far he can stretch his consumer dollar. But in spite of its intuitive appeal, this correction is not warranted for cost-sharing purposes. With the free movement of goods, prices of traded commodities will not differ greatly from country to country. For competitive economies intercountry differences in the prices of services and nontraded goods will thus reflect intercountry differences in the opportunity cost of these goods and services in terms of traded goods and services. Ability-to-pay cost sharing should therefore be based on national income at national prices converted at equilibrium exchange rates, not at "purchasing power rates" computed on the basis of some bundle of consumption goods. Adjustments are warranted only if official exchange rates differ from equilibrium exchange rates or if there are marked deviations from competition in product or factor markets.

But this is merely a technical point. As Pincus makes clear, any formula for cost sharing can only provide the starting point for hard and basically political negotiations, except in those few cases—such as the Universal Postal Union or the General Agreement on Tariffs and Trade—where the sums are small and can be reasonably based on some indicator of benefits.

Pincus is not optimistic about the prospects for increasing aid in its present forms by the amounts he believes will be required. He therefore examines in some detail the possibilities for using international commodity agreements as a vehicle for transferring resources to the less developed countries. He concludes that only five commodities—coffee, cocoa, tea, sugar, and bananas—offer seri-

ous possibilities for transferring resources through price-raising international agreement; other commodities either have close substitutes produced in developed countries (oilseeds, rubber) or are consumed mainly by LDC's (rice). But on Pincus' estimates (the method for reaching them is not satisfactorily described, making them difficult to evaluate) these five commodities offer the LDC's in 1971 only $900 million more than they would otherwise receive from exports. Moreover, there is no assurance that these gains will go to the right countries (Pincus points out, however, that over half the benefits will flow to Latin America) or, more important for development, that they will accrue to the right groups within each country. Capturing these gains for development presumes an adequate fiscal system, often sadly lacking. It is probably more likely that capital flight will be financed with high commodity receipts than with direct foreign assistance. Finally, higher commodity receipts may encourage primary producers to maintain overvalued exchange rates, and thereby impede the diversification of production which Pincus thinks is necessary. In view of the small potential gains and the great uncertainty of translating these gains into economic development, Pincus is perhaps more generous to price-raising commodity agreements than they deserve.

Pincus has written a useful book. He has summarized much existing material on cost sharing and has broken some new ground in the measurement of foreign aid. But unhappily he fails to find the key to raising aid by the $5 billion he believes will be required. This will need a much bolder political approach to foreign aid legislation within the donor countries, based on a more experimental approach to aid giving. Aid must be justified for what it is, a program in social engineering as radical and uncertain as a moon shot is in aeronautical engineering, attended by great risk but also by great promise.

RICHARD N. COOPER

Yale University

African Primary Products and International Trade. Edited by I. G. STEWART AND H. W. ORD. Edinburgh: University Press; Aldine Publishing Co., distributor, 1965. Pp. xv, 218. $7.50.

This volume brings together papers contributed by a number of economists from Africa, the United States, and Great Britain to an international seminar held in the University of Edinburgh's Center for African Studies in September 1964. The initial impetus for it grew out of studies undertaken in Nigeria and Ghana by members of the University's Department of Political Economy. Supported by the U.S. Department of Agriculture, their work was concerned primarily with the estimation of supply and demand functions. The United Nations Conference on Trade and Development then provided the inspiration for examining the specifically international problems of African agriculture.

The individual contributions differ widely in scope and method, but nevertheless combine into a balanced treatment of three main aspects of the primary producing sectors of Africa: their internal structure and functioning, their external effects on other sectors, and the policy influences to which they have been or may be subjected. The chapters are not explicitly subdivided

into corresponding sections, but they can easily be grouped along these lines without overmuch violence to their primary intent.

1. The core of the book consists of four papers on market structure—one by Robert M. Stern on the determinants of cocoa supply in West Africa, another on the coffee industries of East Africa by J. W. F. Rowe, a third on the difficulties of supply and demand projections by H. W. Ord, and the last by V. D. C. Wright on a determinant of the demand for food imports into West Africa. With the exception of Rowe's historical and descriptive work, these papers report on various attempts to apply statistical techniques to very crude data indeed. Some relationships do turn out to be statistically significant. But the main burden of the argument continues to rest on the evident need for improved data on which to base even the simplest of inferences.

2. The need for testing hypotheses is not thereby reduced, especially where the effect of specialization in primary products on other economic sectors is at issue. Recent challenges to conventional suppositions are taken into account at various points. Thus O. Aboyade notes Alasdair MacBean's claim that export instability has not noticeably retarded investment and growth in international comparisons, but he insists that overspecialization on a few commodities has nevertheless led to "capital distortion" by reducing the adaptability of the economy to changing conditions. Gerald K. Helleiner is similarly aware of B. F. Massell's doubts concerning the stabilizing role of export diversification, but he still finds cause to champion an international payments union to diversify what risks remain.

Helleiner also observes how insulated the export sector can be from the rest of the economy in Africa, even where it is based on peasant production. Thus the ratio of intermediate input purchases to the value of total production in Nigeria was only 2.6 per cent, for example, far short of the 31 per cent found by Chenery and Watanabe for Italy, Japan, and the United States. The percentage of output used as intermediate inputs in other industries was even lower, only 1.9 per cent, compared to Chenery and Watanabe's 72 per cent. Enclaves of this kind are therefore much more integrally part of the economies they supply abroad than of the countries whose territories they happen to occupy—even where very little foreign capital is involved!

3. The policy chapters are anticipated at the beginning of the book in a brief but useful survey of the prospects for African agricultural exports by J. A. C. Brown and by a discussion of the possible role of international organizations in the development of African primary products by H. M. A. Onitiri. Ian S. Livingstone then concerns himself in the last part of the book with rival arguments for and against government intervention in the marketing of domestic crops in Uganda and Tanganyika; Eric Clayton describes a linear programming study of policy alternatives to increase the efficiency of peasant production in Kenya; A. A. Suliman analyzes stabilization policies for cotton in the Sudan; and J. J. MacGregor explores the possibility of export diversification by developing timber resources.

Probably the most significant contribution to the book is Dudley Seer's paper on the special interests of Africa in international trade. He argues that

the oversupply of several primary commodities in world markets reflects the inability of Latin American countries to break into world manufacturing markets. If they were permitted entry there, "commodity markets would be vacated and Africa's share in these markets would grow." Even then, however, the projected increase in African export earnings would be insufficient for even modest development targets. Thus "trade not aid" is not in his judgment a slogan applicable to Africa. The need is rather for more trade and aid together, as the editors also point out in the introduction.

In sum, this book provides a balanced sampling of the kind of economic work being done on Africa. The pioneering character of the enterprise no doubt accounts for most of the deficiencies in its achievements to date.

HANS O. SCHMITT

The University of Wisconsin

The Balance of Payments: History—Methodology—Theory. By M. J. WASSERMAN AND R. M. WARE. New York: Simons-Boardman Publishing Corp., 1965. Pp. xi, 481. $10.00.

The first five chapters of this predominantly descriptive treatment of the balance of payments are historical and bring the reader from the bullionist period (dated as early as 1303) to the present. Developments within the United States and the United Kingdom receive extensive attention; several other countries receive lesser mention. The remainder of the book, seven additional chapters, is devoted to major facets of the balance of payments: nature, methodology, accuracy, measurement, reserve and exchange market policies, automatic disequilibria adjustments, and the international payments system.

The historical section focuses on the evolution of the concepts originally involved in the balance of trade, and later in the more inclusive balance of payments. Advances in methodology came slowly and sporadically. For example, it was not until 1911 that a comprehensive study of British foreign investments was made, while 1922 saw the first *official* U.S. balance of payments. Concepts changed slowly. At first the balance of payments was considered a statistical tool to be used in analyzing the international financial position of a country; it was not until after World War II that emphasis was placed on the extent to which domestically produced goods and services were transferred to foreigners, and on how the difference was financed. The International Monetary Fund's *Balance of Payments Manual* (1948) incorporated the then-current basic concepts and methodology; progress since then has been characterized more by minor improvements than by major changes.

Chapter 6 describes the minor differences between the U.S. and the IMF presentation forms, and points out the influence that the balance of payments has had on the other components of the nation's national income and product accounts. Chapter 7 focuses on the relative merits of linked and nonlinked source systems; Ceylon and the United States, respectively, are used as examples. The various methods of categorizing entries are described; one comes to understand why the errors and omissions account is so large and erratic. Woven into Chapter 8 are many of the recommendations of the Review Com-

mittee for Balance of Payments Statistics. If one wishes to discover how and where entries are made, and where they should be made (according to the Review Committee), this is the place.

Chapter 9 is especially well done. The authors describe the division of the balance of payments into substantive (above the line) and balancing (below the line) transactions and discuss the bearing that such classification has on the surplus or deficit. The authors do not avoid the relationship of deficit determination and policy considerations: an example is their treatment of the commonly heard criticism of the Department of Commerce's inclusion, *as a balancing item*, of the change in U.S. liquid liabilities to foreign *private* banks and other holders and the exclusion of their counterpart, U.S. private liquid asset claims on foreigners. *Official* short-term assets, on the other hand, are netted against official short-term liabilities. The rationale for this asymmetrical handling of official and private short-term assets and liabilities is inherent in the Department's "liquidity concept," which emphasizes the speed with which internationally invested funds can be used to defend the dollar. Alternative concepts are suggested, which call for different interpretations concerning which entries might be considered "balancing."

Chapter 10 describes how disequilibria may be settled through drawings on IMF rights, swap arrangements, prepayment of military exports, and the sale of Roosa bonds. The special case of the reserve center country is discussed; a résumé of recent reserve policies follows.

In Chapter 11, which deals with the automatic adjustment of disequilibria, emphasis is placed on the elasticities of supply and demand in determining whether a flexible exchange rate will move within wide or narrow ranges. Some space is devoted to Keynes's interest-parities theory of forward exchange, and a reference is made to Paul Einzig's "dynamic" theory, but the reader is not treated to an explanation of the latter. The foreign trade multiplier is briefly discussed, especially in relation to the U.S. deficits of 1958-64. Multiplier effects on these deficits are said to explain, in part, the persistent unemployment and the relatively slow rate of growth in this country during that period.

In the final chapter, various exchange standards are discussed, and the modern era of international monetary cooperation comes in for brief scrutiny. It was disappointing to find so few of the many suggestions that have been made in recent years for the reform of the international monetary system, especially since U.S. balance-of-payments difficulties are probably largely responsible for much of this interest.

The bibliography is extensive and is divided into subject-matter groupings. The appendices contain reproductions of early English trade documents; selected U.S. balance-of-payments tables proposed by the Review Committee; consolidated balances of payments for several countries; and notes on interpretation. This reviewer can see no reason why this material should have not been placed in the main body of the book, where much similar material is found. There is an author index and a subject index, but no list of figures (only four in the entire book) and no list of tables (101 in all).

Readers looking for new contributions or sophisticated theoretical analyses will be disappointed in this book, but those interested in description and policy will find it a useful volume.

BYRON B. BROWN, JR.

Wisconsin State University-Eau Claire

Business Finance; Investment and Security Markets; Insurance

Business Finance—A Management Approach. By LOUIS K. BRANDT. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. vi, 633. $9.75.

Manuscripts that achieve the preface goals should probably be considered a technical success. Acceptance of content and style is another issue. Professor Brandt has written a book "directed at undergraduate students seeking careers as business managers." Coverage is limited to internal financial analysis described in great detail. Repetitious references to decision-making implore the reader to utilize management techniques for planning precisely, executing carefully, and coordinating tightly short- and long-term policies. After many years of evolution away from finance texts emphasizing external, institutional viewpoints, it is surprising to find the same emphasis on definitions, forms, and lists of comparative advantages and disadvantages in a new book with the locale of analysis shifted to the internal environment.

The first 15 chapters on short-term topics are very similar to the first 17 chapters of a text written by Howard and Upton in 1952. Following a promising chapter on the relevance of financial theory, the reader is instructed in the construction of balance sheets and operating statements. Subsequent chapters describe techniques of controlling expenses, break-even chart analysis, ratios, pro forma statements, and cash budgets along with frequent admonitions to manage cash, temporary investments, receivables, and inventories for both liquidity and profitability. The first section is completed by a review of numerous sources of short-term financing. The decision to devote 15 chapters and 300 pages to short-term subjects and 15 chapters and 323 pages to the long-term section is typical of the thorough planning and organization of the author. The long-term material discusses the traditional topics of forms of debt and equity issues, capital structures, dividend policies, and special problems of changing ownership, recapitalization, reorganization, and beginning a new business. There is even a chapter on managing investment securities. Unfortunately, the good discussion of planning capital expenditures is followed by a very abbreviated coverage of the problems of rationing capital, risk, determining the aggregate cost of capital, and selection of tentative "cut-off" points. Financial terms, forms, and institutions are well defined, but the excitement and variable aspects of problems are frequently missing in the discussion, and an illusion of mechanical simplicity is sometimes created.

Topics are discussed in a systematic manner following the same format in each chapter, and few students should have difficulty understanding the material presented. The descriptive chapters provide a good reference source, although explanatory and reference footnotes are lacking. The absence of tables and charts summarizing empirical information and company experiences also

simplifies the reading job. A vast array of problems and short "casettes" are included for student assignments. Negative style features include a surprising number of minor errors and a tendency to pulverize simple ideas. In the discussion of the use of sinking funds, the reader is instructed (p. 366), "Thus, an 80 per cent plan fully executed leaves only 20 per cent of the debt exposed to risk at maturity, while a 50 per cent plan leaves 50 per cent exposed." The author attempts to build concepts by covering the same topics in several different chapters; however, the level of discussion remains the same so the result is often simply repetition. There are many sections where identical ideas, even entire paragraphs, are repeated (for example, see pages 356-58).

Criticisms of style are insignificant relative to consideration of subject coverage. Professor Brandt has attempted to prune away the emphasis on the financing and crisis epics of many finance texts following the lead of Howard and Upton, as later expanded by Johnson and Weston. No attempt was made to create a new format similar to the efforts of Beranek or Lindsay and Sametz. His approach highlights the dilemma of finance courses in today's curriculum. From one side, the managerial accountants are pushing into the liquidity and solvency topics of finance, and from the other, the managerial economists continue to concentrate on problems affecting profits and the allocation of capital. Finance professors may ignore these contributions and continue to emphasize the familiar tools of financial analysis. At schools where accounting departments have moved toward a managerial approach, this duplication will lead to inefficient use of the early sessions of finance courses. The opposite extreme is to drop all attempts to be analytical and concentrate on descriptive reviews of various sources of funds. A suitable compromise would be to create an advanced course utilizing the analytical skills and knowledge acquired in basic accounting and economics courses to examine problems of financing, allocating funds among alternatives, capital structures, valuation, liquidity, and other finanical topics. The beginning course in finance need not use a "basic" or "elementary" approach if the student first studies economics and accounting. The internal approach advocated in this book may be justified if a different curriculum is adopted. Therefore, while complimenting the author on his thoroughness and organization in the informative coverage of the topics selected, this reviewer does not believe this text solves the basic problem of deciding what the subject content of a finance course should be and what level of sophistication and breadth should be sought, relative to the amount of details and definitions of basic topics that are also analyzed in related academic studies.

SIDNEY L. JONES

University of Michigan

Industrial Organization; Government and Business; Industry Studies

Transportation Economics. A Conference of the Universities-National Bureau Committee for Economic Research. (Special Conference 17.) New York: Columbia University Press for National Bureau of Economic Research, 1965. Pp. xviii, 464. $10.00.

The sixteen papers that make up this volume in the National Bureau's conference series provide a rather broad and representative sample of the wares currently being vended under the label of transportation economics. They range in the problems attacked from the very old to the very new, in research technique from legal case-dredging to subtle statistical methods, and in scholarly weight from routine occasional pieces to important original research. The following comments deal selectively with some of the more interesting themes and individual contributions.

Several writers are concerned with aspects of the interrelated problems of pricing and intermodal allocation in the carriage of freight. The essays by J. R. Nelson and by R. A. Nelson and W. R. Greiner together examine selected historical forces helping to explain the present dilemma of excess capacity, low returns, and inefficient pricing in railroad freight service. J. R. Nelson reviews some of the organizational and market factors that may explain the original creation and persistence of excess capacity. R. A. Nelson and Greiner explore the hypothesis that the preservation of value-of-servicing pricing "has been the dominant policy underlying transportation regulation in the United States for many years" (p. 373), due to a political interest in maintaining discrimination in favor of the long-distance movement of basic raw materials and agricultural products. The attempt to insure the conformance of motor truck pricing to a rate structure containing important elements of discrimination has had the effect (among others) of denying railroads the opportunity to compete on the basis of price for traffic that they can profitably carry and leaving the rates for raw materials and agricultural products unadjusted. Indeed, the paper by J. C. Nelson on "The Effects of Entry Control on Surface Transport" develops evidence on the costs of imposing regulation on truck transport for the protection of the railroad pricing structure, since he analyzes the real costs attributable to franchise restrictions on the operations of common-carrier trucking firms and not just to entry control per se. The paper by M. J. Roberts on "Transport Costs, Pricing, and Regulation" touches upon the substantive cost-based criteria for pricing that would most likely serve to straighten out these misallocations. Long-run marginal cost is an appropriate basis for the (minimum) pricing of rail service, as are the long-run marginal costs of other transport agencies (except pipelines) to their own minimum charges. In the case of agencies other than railroads, long-run marginal cost can largely be identified with short-run average cost. In railroads, however, the appropriate marginal-cost concept is a hybrid, representing incremental costs in a rationalized version of the present railway plant and not the costs that would prevail if the railways were optimally rebuilt from scratch.

Two papers by R. J. Gordon and G. Kraft are concerned with airline costs. Gordon attempts to develop a measure of the cost of managerial inefficiency in domestic commercial air transport, identifying it with positive deviations from cross-section least-squares regressions (or similar fits) of selected airline cost components upon their major economic or technical determinants. Valuable as this paper may be as an exploratory effort, its results (which impute most of the subnormality of the trunk airlines' 1960-61 profits to inefficiency) are a bit hard to interpret. Gordon's measure is not a maximum one, since it

rests on deviations from average performance, not from some technical optimum; nor, on the other hand, is it a minimum, since omitted cost-determining variables outside of managerial control may account for some of the positive deviations. Kraft's paper on advertising costs in the airline industry uses a Koyck-type distributed-lag technique to assess the revenue productivity of advertising expenditures. For reasons fully adduced by Kraft's discussants, his results must be rejected. They more likely reflect the effect of revenue on advertising expenditures than the other way around. And, as usual when this method is applied to autocorrelated time series, the apparent goodness of fit is quite deceptive.

The volume includes a paper by E. Mansfield from his excellent series on technical change, "Innovation and Technical Change in the Railroad Industry." His results, concerning the size of innovating firms and interfirm and intrafirm rates of diffusion, seem to raise one problem of interpretation. The Schumpeterian hypothesis invoked by Mansfield (p. 181) emphasizes the risk attached to innovation and the allegedly greater capacity of firms possessing positions of monopoly to incur it. The list of innovations investigated by Mansfield includes, quite naturally for a service industry, many that are in essence new types of capital goods, the developmental risks of which must have been borne largely by equipment manufacturers outside the railroad industry. Whatever the significance of Mansfield's findings about their adoption by the railroads, it may not be as a test of the Schumpeterian hypothesis. Was Schumpeter talking about who would build the better mousetrap, or about who would first beat a path to the door?

M. J. Peck and J. R. Meyer contribute a stimulating paper on "The Determination of a Fair Return on Investment for Regulated Industries," in which they compare to the present-value calculations of rates of return indicated by capital-budgeting procedures those measures of returns commonly used in regulatory proceedings. It is a bit dismaying to see so little made of the distinction between replacement cost, a theoretically sound rate base, and the reproduction-cost base in actual use, considering all the blood that has been spilled over the latter. Peck and Meyer evade the issue of the "normal" rate of return to be allowed once the rate base is established, except to note that, around the early 1960's, public utility industries were doing less well than selected manufacturing industries marked by moderate to high seller concentration. Do they feel that a fair return to the utilities should include a fair share of monopoly profits?

Finally, a group of papers deals with the interrelated issues of urban economics and highway passenger transport. R. W. Harbeson's review of the problem of allocating highway costs to different classes of users fails, as discussant P. O. Steiner points out, to distinguish adequately between the "equity problem" of allocating costs among users after the event and the "efficiency problem" of levying charges equal to the associated incremental costs as a condition for use. W. Vickrey provides a spirited advocacy of marginal-cost pricing, and T. Kuhn an imprecise discussion of "The Economics of Transportation Planning in Urban Areas" which mixes comments on the appropriate form of decision criteria with complaints about abuses in their actual em-

ployment. J. F. Kain develops some novel material on the trade-off between residential-space costs and travel costs for urban commuters and on the impact of racial discrimination on housing costs and locational patterns. Finally, K. T. Healy pleads for placing urban transport problems in a larger decision-making context that would encompass the possibility of discouraging the further growth of already large urban agglomerations. He could make his case even stronger by arguing that economic units adding themselves onto large metropolitan areas probably pay charges (taxes and user payments for public services) that fall well below the associated long-run marginal social costs, but as long as red blood flows in chamber-of-commerce veins he is likely to win few influential converts to his cause.

RICHARD E. CAVES

Harvard University

The Urban Transportation Problem. By J. R. MEYER, J. F. KAIN, AND M. WOHL. Cambridge: Harvard University Press, 1965. Pp. xix, 427. $11.95.

This RAND Corporation Research Study deserves the serious attention of every student of the urban scene. The implementation of the study is impeccable: "The ultimate goal is to improve urban living, not to eliminate congestion, or maintain investments in real estate or transportation facilities as such. Urban transportation investments use resources that alternatively could be used for educational, health, recreational or other purposes." In the best tradition of descriptive (as opposed to prescriptive) economics, the authors largely succeed in their attempt to define the urban transportation problem and to explore the validity of many of the contemporary clichés about our urban transportation condition.

Part I is a well-written nontechnical discussion of trends in urban population, employment, residential patterns, trip demands, transit service, and so forth. Since the absence of adequate data precluded an econometric investigation, this section may not completely satisfy the reader accustomed to thinking in terms of income and cross price elasticities of demand. However, the authors' informed and plausible interpretation of available data does provide a clear—and not infrequently surprising—insight into the changing character of spatial activities in urban areas. The authors display this same talent for synthesizing theory and fact throughout the book.

Part II on comparative costs is the heart of the study. The authors carefully build detailed cost models for rail, bus, and automobile systems that provide the three basic urban transportation functions: residential collection and distribution, line-haul, and downtown distribution. Several dimensions of the one-way trip are standardized in order that the estimated dollar costs of the trip on alternative modes can be compared with one another. The authors specify identical speed, frequency of service, population density, convenience and comfort for each mode and proceed to estimate the dollar trip cost on each mode at various passenger-volume levels and various trip lengths. Obviously, congestion problems are assumed away at this stage of the analysis. The cost

estimates that result are too numerous to summarize in detail here since transfers, surface and underground construction, and combinations of modes are also considered as alternatives. But several conclusions are worth specific note. the freeway flier bus has the lowest trip cost of all modes, lower than rail for even high population densities. The frequently proposed park-and-ride rail system is usually the highest-cost means of providing trips. Equally interesting is the conclusion that the private automobile with an occupancy rate of 1.6 persons has a lower trip cost than either bus or rail for passenger volumes of less than 10,000 persons per hour per corridor (the case of most U.S. cities).

Part III, "Solutions and Public Policy," is both a summarization and a discussion of what contributions can and cannot be realistically expected from our technology and our political institutions. In brief, the authors advise rationing expressway space during peak hours by taxes, direct access controls, and by a priority system for express buses. They are not optimistic about the prospects for a technological breakthrough that will dramatically solve the urban transportation problem. They find improvements in existing technology more promising for the future and in this context suggest the following modifications for bus systems: more express buses, centralized fare collection, the addition of a third door with double side loading and unloading, and diagonal, multislot, high-speed, high-volume bus terminals.

The authors are understandably reluctant to commit themselves to a general policy recommendation for urban transportation. The conclusions they do reach might be paraphrased as follows: since we know so little about the demand function for urban travel and since the capital costs of rail systems are considerable, the economic approach in most U.S. cities would be to experiment with express buses, priority express buses, and congestion-control systems on existing freeways before plunging into the construction of expensive, long-lived rail facilities. Unfortunately this approach probably lacks sufficient glamour to satisfy the political and propertied urban establishment that already seems bent on the construction of capital intensive, federally financed rail systems.

In a sense this is an untimely book; it appears too late to have a beneficial effect on those responsible for drafting the federal legislation that will shape urban development for generations to come. On the other hand, the book appears too soon to include a discussion of the detail and impact of the Urban Mass Transportation Act of 1964. The usefulness of the authors' cost estimates must surely be evaluated in the light of the provisions of that act. Although their estimates are methodologically unassailable in the context of the national allocation problem, those costs are now not relevant at the urban level where the transportation-investment decisions are made. Under the provisions of the act the federal government is prepared to pay up to two-thirds of the capital costs of a mass rapid transit system for a qualifying area. Since the capital or fixed-cost component of a rail system is much higher than that of an express bus system, the effective community costs of a new rail facility are now considerably lower. The practical urban politician realizes this, of

course, and will revise the cost equations of this study accordingly in order to find *his* cost of urban transportation.

M. BRUCE JOHNSON

University of Washington

Economics of Private Truck Transportation. By WALTER Y. OI AND ARTHUR P. HURTER, JR., WITH ERROL GLUSTOFF, SEIKO HIGA, AND DAVID R. MILLER. Dubuque: Wm. C. Brown Co., 1965. Pp. xvii, 365. $9.00.

The role of private, as contrasted with common carrier, truck transport in the United States has long been one of the least understood aspects of transportation economics. This fine volume supplies much of the missing information. In the process, a major contribution is also made to the emerging field of transportation logistics (e.g. Chapter 8: "An Empirical Analysis of the Private Carriage Option"). Furthermore, the book incorporates (Chapter 5) the best analysis of the behavior of truck costs, both private and common carrier, yet available. Finally, a highly suggestive and original statistical analysis is presented of "The Value of Service Component in Rail Freight Revenues" (Chapter 7).

The major conclusion, though, will be as unsurprising to many transportation economists as it will be irritating to some of the more hidebound elements in the common carrier industry. Specifically, on the assumption that certain income and income redistribution effects are not dominant considerations, Walter Oi and Arthur Hurter conclude that private carriers are socially desirable on the grounds that they achieve at least some improvement in the allocation of transportation resources. Accordingly, they recommend a reduction in transportation regulation in order to evoke a more workably competitive environment in highway transport. In particular, they favor removal of minimum rate regulation as it is now applied to common carriage (by all modes). With such a change, they envisage that common carriers could compete more effectively with private trucking for certain sectors of the market, mainly short-haul traffic, where present common carrier rates seem most notoriously out of line with costs and therefore provide a considerable incentive for private carriage.

This conclusion derives from their detailed analysis of the incidence of private motor transport. Specifically, they argue, quite cogently, that private motor transport can be broadly viewed as a form of vertical integration that will be practiced by firms whenever profitable. They predict and provide evidence that this profitability is a function of several variables such as firm size, industrial affiliation, the frequency of short-haul shipments, the importance of freight expenses in terms of total expenses of the firm, the prevalence of intrafirm shipments and the extent to which the shipper (as contrasted with the consignee) bears the freight cost. On an empirical basis, the distance profile of shipments proves, somewhat unexpectedly, the most important single factor while the size of firm is less important than expected. Though medium-size firms use more private carriage than small firms, very large firms tend to use relatively little private transport. This phenomenon, incidentally, might be explained, though the authors do not investigate the point, by the greater bar-

gaining power of the very large shippers; this could enable them to extract rate concessions from common carriers much more readily than their small or medium-size competitors. Whatever the explanation, the usual defense of public policies to protect common carriers, based on the grounds that common carriers are particularly needed by smaller businesses, is at least mitigated.

Another important factor determining the substitution of private for common carriers, introduced somewhat *ad hoc* by the authors, relates to temporal variations in transport demands. They identify what they call a "base load" phenomenon. Many shippers seemingly find it advantageous to develop sufficient private capacity to meet their base or temporally steady transport requirements and rely upon for-hire carriers to take care of spillover demands which occur seasonally or intermittently. The same argument could have been extended to infer that shippers who generate temporally steady or constant demands for transportation will also have incentives to enter into private truck transport as long as pricing schemes used by common carriers do not reflect the cost effects of demand variations.

Inflexibility in the common carrier rate structure also seems to be the root cause of much of the private motor transport of short-haul traffic. Common carrier disadvantages in short-haul traffic can be substantially attributed to an inability to tailor equipment to the special needs of short-haul traffic, mainly because the common carrier has no guarantee that he will have the traffic over a sufficiently long period to amortize properly any specialized equipment investment. The private carriers, of course, implicitly possess this guarantee. In short, a major advantage of the private truck on short hauls, or any traffic requiring special equipment, is a traffic guarantee.

This, however, suggests a policy alternative to minimum rate deregulation, namely institution of so-called contract or agreed charges. Basically, these are long-term contracts entered into between the shipper and the common carrier in which the carrier agrees to a lower rate in exchange for the shipper's guarantee that a certain percentage or absolute level of traffic will be given to the carrier over a specified time period. This arrangement would enhance the competitive position of the common carrier both on short hauls and in stable demand situations, which, as noted previously, seem to account for a very high percentage of total private transport. Contract charges, incidentally, are in effect in many parts of the world today. Following Oi and Hurter's suggestion that some form of regulation will have to be continued even with elimination of minimum rate regulation, contract rates might be more acceptable if subject to regulatory review and cost justification.

JOHN R. MEYER

Harvard University

Government and the Railways in Nineteenth-Century Britain. By HENRY PARRIS. Toronto: University of Toronto Press; London: Routledge & Kegan Paul, 1965. Pp. xii, 243. $5.75; 35*s*.

In many respects the economic organization of the British railways in the nineteenth century was the envy of American railroad men. Britain restricted entry to the extent of requiring an act of Parliament for construction of a line.

The country was highly developed, so that railways were typically built to serve existing population, rather than speculatively in unpopulated areas. The Railway Clearing House was a more effective force for cartelization than any of the U. S. rate associations, and the courts exempted railways from the usual Anglo-American prohibition of collusive pricing. Finally, Parliament, from a notable speech by James Morrison in 1836, was dominated by the idea that collusion among railways was inevitable. Accordingly, Britain had no counterpart of the long American effort to organize the industry competitively. Parliament from 1840 provided for public intervention in the industry through a succession of regulatory bodies. The traditional British literature on this experience is as uncritical and unrewarding as our pre-1940 literature on the ICC, and there is little question that a new study of British railway regulation in the nineteenth century is needed.

Unfortunately, it would be difficult to argue that this volume does the job. The author devotes himself in his early chapters mainly to a detailed history of public administration in the Board of Trade Railway Department and its successor organizations. He has unearthed a great deal of information on internal operation of the regulatory bodies from manuscript sources, but he has written a text such that the information is essentially its own end. The author's choice of quotations and, especially, his use of italics lead me to suspect he had some argument in mind, but I am unable to identify it. Parris' principal interest is in accident investigation and cases concerning installation of safety devices. The chapter on relations of the Railway Board with the companies is almost entirely devoted to safety problems.

Worse, Parris' treatment is most intensive on the period 1840-67, whereas the regulation was made more comprehensive by the establishment of the Railway Commission in 1873. Thereafter, rate regulation was carried on systematically in a fashion which was in certain respects a model for the ICC. One wonders, naturally, whether it was an effort at artificial depression of rates or a facilitation of the industry's cartelization, but Parris provides no answer. By analogy to the U.S. experience and a priori considerations, one presumes the latter. When a legislature establishes a body to regulate an industry, there really isn't much to do but cartelize it.

Finally, Parris is presented with a problem in intellectual history: why in a period of widespread philosophical commitment to *laissez faire* was there such early recourse to public intervention in railways? He addresses himself to this question for about two and a half pages at the outset of a chapter entitled "Railways in the Theory of Government," but his treatment is limited to a few statements of the views of major economists on the proper scope of public intervention in price determination and resource allocation. He then wanders off into a discussion of contemporary interventions: the Factory Acts, the Poor Laws, factory inspection, and the like. He attempts no serious enquiry into the views of the later classical economists or Ricardian members of Parliament on the technological characteristics of the railways which made them a special case.

Publication of this book may possibly be justified by the antiquarian information it presents on public administration, notably in connection with safety

regulation. The readers of this journal are presumably interested in it as a piece of economic history. So viewed, it is thoroughly inadequate. Pratt and Cleveland-Stevens did a better job of writing on the subject in the World War I era and, to judge by the wealth of Parris' citation of sources in the Public Record Office, with a smaller expenditure of resources in the process.

GEORGE W. HILTON

University of California, Los Angeles

Air Transport Policy and National Security—A Political, Economic, and Military Analysis. By FREDERICK C. THAYER, JR. Chapel Hill: University of North Carolina Press, 1965. Pp. xxiii, 352. $8.00.

Despite the subtitle, the "analysis" offered in this volume is very largely confined to the two concluding chapters, which deal with present policy problems connected with military airlift and international air transport regulation, respectively. The other nine chapters, making up over two-thirds of the text, might better be described as "annals."

According to the dust jacket, the author holds the rank of lieutenant colonel in the Air Force, in which he has served since 1945; moreover, "he has spent eleven of those years in operational and staff assignments involving airlift or air transport." It is therefore not surprising that the subject of what appears to be his most closely reasoned chapter should be military airlift—specifically, the relevant policy decisions of the Kennedy administration, which Colonel Thayer finds "contradictory to each other" (p. 263), and his own suggestions for their correction. However, even the military nonexpert cannot but be aware that this analysis, though seemingly cogent, would need supplementation from non-Air Force sources in order to provide sufficient basis for evaluating the proffered recommendations. Those whose primary interest is in civil air policy will find here little help in answering the principal question which must concern them in connection with military airlift: namely, whether there is any military airlift requirement which can be met most efficiently by some modification of governmental policy towards commercial air transportation—e.g., by some form of subsidy.

In the absence of a clear answer to this question, attempts to relate commercial air policy to military airlift needs must tend to be confused and indecisive. This conclusion is borne out by Thayer's detailed narration of major and minor events connected with the pre-Kennedy airlift controversy, which accounts for some three-fifths of the historical section of the book. Political and military historians may find this narration, as well as Thayer's treatment of World War II airlift, to be of assistance in piecing together a fairly complex story.

The remainder of the historical section, which takes up national and international airline policy, goes over ground that has been more thoroughly and objectively covered in other works—for example, in Richard E. Caves's *Air Transport and its Regulators* (Harvard, 1962) and the sources cited therein.

In his analysis of international air transport policy, Thayer is handicapped by unfamiliarity with the facts and lack of sophistication in dealing with economic theory. He believes, for example, that the rate-making machinery of the

International Air Transport Association (IATA) constitutes "*de facto* regulation" (p. 68); that the United States has refused to "consider the possibility that the IATA apparatus may be the best middle ground available" (p. 83); and that this country's promotion of export sales of aircraft to foreign airlines is resulting in higher postage rates through increases in subsidization of our carriers by means of mail payments (p. 282). In fact, according to the calculations of the Civil Aeronautics Board, mail payments to U.S.-flag international air carriers have contained no element of subsidy since the late 1950's; furthermore, past subsidies had little or no effect on postage rates. The IATA and its rate-making procedures exemplify the general proposition that where enterprises are operated by governments—as most of the foreign air carriers are—the regulatory function is absent, neglected, or weak. In the Statement on International Air Transport Policy approved by the President in April, 1963, IATA rate-making is characterized as follows: "This multilateral mechanism, though it has some drawbacks, seems to be the most practical one we can achieve, and it should be maintained."

Thayer's contact with economic theory has persuaded him that the existence of oligopoly, with its departure from pure competitive pricing and its presumed promotion of "wasteful" competition in selling, justifies the institution of "government-industry partnership" covering both domestic and international air transportation (p. 297). Domestically, the "general rule for restructuring the industry would have to be that only one carrier operates across any particular route except where it is utterly impossible to avoid direct competition" (p. 296), and the industry should undergo a drastic rationalization and paring down of profits. International service by U.S.-flag airlines should be carried on by three companies, each serving one region of the world (pp. 304-6). Economists must shoulder some part of the blame for this sort of immediate translation of partial and tentative economic analyses into policy recommendations, not only with respect to air transportation, but in the large number of other fields exhibiting similar market characteristics.

Thayer's recommendations on international air policy are indeed partly defended by an appeal to special considerations: first, what seems to this reviewer a thoroughly obscure analogy with a doctrine of Admiral Mahan regarding sea power ("National power, he argued, depends upon military and economic dimensions of naval strength—the Navy and the Merchant Marine" (p. 300)); and, second, the need for overseas air bases in time of war. It should be obvious, however, that peacetime use by U.S. airlines is neither necessary nor sufficient for the wartime security or availability of such bases.

A recurring note in Thayer's work is the assertion that the U.S. government harbors an unreasoning hostility towards U.S. airlines. In connection with route negotiations, he believes, the interests of these carriers are inadequately represented: "the government still holds that it cannot assist U.S. airlines because they are private enterprises, but it must hurt them in the national interest" (p. 79). A glance at the international route map should suffice to dispel any impression of general ineffectiveness on the part of U.S. negotiators. There seems nevertheless to be one area—not mentioned by the author—where officials have fallen short of what might be reasonably expected in pur-

suit not only of equal opportunity for our carriers, but of maximum economy and efficiency in international air transportation. The power to suspend service by foreign air carriers, which is within this country's prerogative under the Bermuda-type bilaterals, was not available for use in the celebrated North Atlantic fare controversy in the spring of 1963 and, as far as this writer knows, is unavailable still, apparently because of official failure to provide the necessary legal machinery. Speedy repair of this oversight would help to silence allegations such as those made by Thayer.

LUCILE SHEPPARD KEYES

Washington, D.C.

Edinyi energeticheskii balans narodnogo choziaistva (problemy optimizacii). (Over-all Balance of Energy in the National Economy—Problems of Optimization.) By S. D. FEL'D. Moscow: Ekonomika, 1964. Pp. 311, Appendices.

Atommaja energiia v narodnom choziaistve (Ekonomika i primeneniia). (Atomic Energy in the National Economy—Economics and Utilization.) By W. I. POSTNIKOV AND I. M. RAZUMOV. Moscow: Ekonomika, 1964. Pp. 175.

Energia jadrowa w Polsce w latach 1955-1960. (Nuclear Energy in Poland, 1955-1960.) Edited by JOZEF HURWIC. Warsaw: Wydawnictvo Naukowe, 1963. Pp. 394. English Summary.

During the past few years, energy problems have attracted a great deal of academic attention in the Soviet Union. This has been reflected not only in a growing number of publications dealing with one or the other aspect of their own energy economy,[1] but also in a pronounced interest in similar inquiries elsewhere, especially in the United States.[2] Nevertheless, up to now, the Soviet energy studies lagged behind such undertakings in the West, notably in methodological refinement and analyses of the fundamental intersector energy flows in the national economy.

S. D. Fel'd attempts to fill in the gap in both directions. On the one hand, he offers his own methodology for constructing a uniform over-all energy balance sheet for the USSR and, on the other, he presents the very first Soviet intersector energy flows. These are restricted to four selected years (1910, 1928-29, 1958, and 1965—as planned) with greatly unequal periods and many socioeconomic upheavals in between, so that they do not permit a definite conclusion about the effects of Soviet energy balances on economic de-

[1] See, e.g., *Toplivno-energeticheskii balans SSSR* (The USSR Fuel Balance), by A. A. Riznik and C. E. Litvak, Moscow 1965; *Ekonomika Energetiky* (Economics of Energy), by L. A. Melentev and E. O. Shteingauz, Moscow 1963; *Toplivnaia promyshlennost SSSR i ekonomicheskaia effektivnost kapitalovlozhenii v ee razvitie* (The USSR Fuel Industries and Economic Effects of Capital Investment in Their Development), by G. D. Bakulev, Moscow 1961; *Toplivnyi balans SSSR* (The USSR Fuel Balance), by D. I. Maslakov, Moscow 1960.

[2] They recently translated *Energy in the American Economy, 1850-1975,* by Sam H Schurr, Bruce Netschert, *et al.*, Baltimore: Johns Hopkins Press, 1960.

velopment. In spite of this, however, these flows offer useful information on changing energy patterns in the USSR.

The book is divided into five parts, the first of which is devoted to definitional and theoretical issues generally encountered in constructing energy balance sheets. The subsequent parts are more oriented toward specific Soviet energy problems. The second and third chapters deal with efficiency in Soviet production and utilization of energy, respectively, while in the fourth chapter, Fel'd examines the complex link between economic growth and the structure of the energy economy. This is, in effect, a pivotal issue in Fel'd's work because, as he points out, the present Soviet economic plans, in regard to their energy base, are predicated on rather simple and mechanistic formulas of a technically attainable maximum output of the respective energy resource. Little, if any, attention is paid to considerations of an optimal solution, mutual substitution, and relative costs of individual fuels and hydroenergy. As may be expected, it is in the concluding chapter that the author presents his own model, based on linear programming, which should permit construction of more economics-oriented energy plans within the framework of a Marxian state.

Conceptually the author's approach rests on a cost accounting in real terms expressed in kwh (or kcal) of useful energy as needed in productive as well as nonproductive sectors of the Soviet economy. His main analytical tools include a complex concept of the "useful energy cost," that is, a sum of all useful energy inputs needed per unit of specific output. As a second step, the concept is extended to cover output of the entire industry. To that is related the industry demand for a certain quantity of potential energy in all raw inputs while the link between the two is provided by a coefficient which, generally speaking, corresponds to "thermal efficiency" as defined by Schurr.[3] The cost of producing energy is calculated in man/days (years) per unit of potential energy. An optimization criterion is the minimum total cost of all the useful energy which is determined in the given national plan as needed by the economy.

There are a number of inconsistencies in Fel'd's concept of an integral "energy cost" in the entire economy. To name only one, he omits fodder for animals from his computations although he does include therein the contribution of animal power. Also, the difficulties in computing and expressing the intra- and interindustry input-output flows in corresponding real terms of direct and indirect "energy cost" justifiably raise a question about the model's practicability. Nevertheless, in many respects, the study by Fel'd makes interesting reading.

Fel'd pays only perfunctory attention to the economic impact of atomic energy. One would expect that this question will be more adequately treated in the book of W. I. Postnikov and I. M. Razumov, whose title suggests that it is a specialized study. It is not, however; the authors merely repeat generally known assumptions about nuclear economics and in the rest discuss the uses of radioisotopes. Similarly, the Polish book edited by Jozef Hurwic is mainly

[3] Schurr, *ibid.*, pp. 171-72.

oriented toward a review of institutional developments rather than to economics of nuclear energy application in Poland.

JAROSLAV G. POLACH

Resources for the Future

Land Economics; Agricultural Economics; Economic Geography; Housing

Planning in Chinese Agriculture—Socialisation and the Private Sector, 1956-1962. By KENNETH R. WALKER. Chicago: Aldine Publishing Co., 1965. Pp. xviii, 109. $5.00.

The subtitle "Socialisation and the Private Sector, 1956-1962" more accurately describes the contents of this compact and well-documented book. This is one of the first Western books to explore a Communist Chinese enigma: how to increase real per capita agricultural output above the pre-Communist level within the Marx-Lenin-Mao framework of communal ownership of all productive resources.

Professor Walker records and analyzes the Communists' plans and programs in three parts. In Part I, he traces the Communists' step-by-step socialization of agriculture with land reform ending in 1952, peasants' mutual aid teams in 1954, agricultural producers' cooperatives in 1955, collectives in 1956, and communes in 1958 (p. 14). In Part II, the author analyzes the economic significance of the private plots to the individual peasants and to the Government. In Part III, about half the book, he chronicles the Communists' vacillating policy toward private plots from harassment in 1956-57, to abolition in 1958, and to restoration in 1961. Private plots, accounting for 5 to 7 per cent of China's total cultivated land, continued to prosper in 1965 (see "China's Agriculture" in *Foreign Agriculture,* FAS, USDA, Nov. 22, 1965, p. 2).

The quantities of food, fiber, and fertilizers produced on these private plots are a matter of conjecture. The author estimates that the modal amounts of food available to peasants from private plots in 1956, based on samples from many areas, were 72 grams of pork, 56 grams of vegetables, and 284 calories per capita per day (p. 30). Average amounts were substantially higher especially of vegetables. Grain from the collective sector was estimated to provide 1,792 calories per day. Roughly 14 per cent of the peasants' total calories came from private plots which raised their caloric intake above the subsistence level. Workers fared better because about 40 per cent of the population was either under 16 years of age or over 60.

The percentage of peasants' real disposable income from private plots varied from 13.7 per cent in Northwest China (1956) to 33.6 per cent in South China (1957) (p. 34). Total net disposable income per head was higher in the wheat-North than in the rice-South. These data suggest greater productivity on the private plots than on the collectives in the South where labor requirements per hectare are high. In the agriculturally diversified Fukien province, net income from private plots, as a percentage of total income, ran as high as 67.3 per cent (p. 39).

The more profitable private sector drained labor, managerial skills, and animal fertilizer from the collectives. In an effort to contain peasant capitalism, private plots were restricted to 5 per cent of a collective's land as a national average (p. 43). To further control private incentives the farmers' free market was closed in August 1957 (p. 73). Finally, private plots were abolished with the advent of the communes in 1958 (p. 75). Their abolition eliminated an embarrassing contradiction but Communist planners believed that private plots were no longer needed. Food was in ample supply and the efficiency of communal messhalls, producing their own poultry and hogs and serving free food, was to more than offset the loss of food from private plots (p. 76). The freed labor was expected to increase the output of the agricultural collectives which provided the bulk of the food calories and the savings for industrial investments.

The Great Leap Forward, statistically successful, lowered real agricultural output drastically. The shift of skilled farm labor to mines and factories, and natural disasters, decreased the food supply while the demand for food increased due to the free-food policies of messhalls and the swelling of the urban population. The huge Chinese population was reduced to a near-starvation level in 1960-61 (p. 83).

If there is a central character in this excellent treatise it is the pig. But unlike his dictatorial role in George Orwell's allegorical novel *Animal Farm*, Professor Walker's pig is more of the hero-type. The pig plays a vital role in not only supplying the population with essential animal proteins but also in producing a scarce and valuable resource: manure. In 1955 pig manure was produced at 24 kilograms per hectare of arable land as contrasted to only 14 kilograms of chemical fertilizer (p. 56). The pig population declined by more than 17 million during the early years of collectivization which resulted in less pork for urban markets and less pig manure that the collectives sorely needed.

Apparently, pigs do not prosper under collectivized management. Half of the collectivized young pigs died in 1960 because of diseases (pp. 67 and 94). The Communist Party, anticipating disaster, returned pig rearing, accompanied by appropriate incentives, to private hands in 1959 (p. 81). According to *Foreign Agriculture*, cited above, about 80 per cent of the hogs and 95 per cent of all poultry are presently raised on private plots in Communist China.

Gerald Winfield in *China: The Land and the People* (New York: William Sloane Associates, 1948) makes much of China's traditional "organic union of man with the land" (p. 46). The Chinese Communists set out to alter this symbiotic relationship. Yet, we are mindful that Russia after nearly 50 years of trial and error is still largely dependent on the private sector for her supplementary foods, and is a large importer of grains. China, too, continues to accommodate contradictions by tolerating private plots, supposedly as long as peasants do not exploit each other, and by importing wheat while exporting rice obtained by the Government's exploitation of agriculture and the collectivized peasants.

Though Professor Walker's numerous calculations may be suspect due to the paucity and credibility of available statistics, yet he clearly depicts the tribulations encountered by the planners, and the peasants, in Communist

China's attempt to industrialize rapidly. Many of their grandiose schemes have been shelved. Messhalls have been abolished and small production teams have been delegated the decision-making, income-sharing responsibilities subject, of course, to Party directives. After 15 years of experimentation it appears that the more Communist China's agriculture changes, the more it returns to earlier forms.

EDWARD L. RADA

University of California, Los Angeles

Labor Economics

Forced Labour and Economic Development—An Enquiry into the Experience of Soviet Industrialization. By. S. SWIANIEWICZ. Issued under the auspices of the Royal Institute of International Affairs. London: Oxford University Press, 1965. Pp. xi, 321. $7.20.

The author of this study of forced labor in the Soviet Union sees a certain dilemma confronting underdeveloped countries with agricultural overpopulation; the choice is to grow at a "snail's pace" or to face the harsh problems that a program of forced economic growth entails. The success of forced economic growth in the USSR makes it an attractive model for many countries, but its attractiveness is marred by the use of forced labor. The objective of this study is to examine the role of forced labor in Soviet development in the hope that ways may be devised for other countries to pursue development programs without having to resort to that technique.

The principal function of forced labor in the prewar period (in which the study concentrates) was to carry out the redistribution of labor required by the development plan. During the early 'thirties the task was to move the surplus agricultural labor off the farms into industry and construction. Because of the "wage-goods gap," normal economic incentives would not have accomplished the shift. The policy of "dekulakization" directly removed several million peasants to forced-labor camps and to forced settlements, and indirectly contributed to further mobility by encouraging others to leave their villages and by strengthening the official labor-recruitment organs. The author raises, but does not directly answer, the question of whether the policy was a "historical necessity," though his data suggest that on economic grounds the answer would be negative. The "wage-goods gap," for example, which motivated the policy, was in fact aggravated by the losses sustained in agriculture and by the opportunity cost of forced labor.

In the late 'thirties, the purpose of forced labor changed from the elimination of agricultural unemployment to the distribution of scarce labor "in accordance with governmental priorities," particularly to the investment ventures of the NKVD. The author stresses the importance of these projects, but in his calculation of the cost of forced-labor operations, he finds that the wage savings from the use of forced rather than free labor is almost offset by the high cost of the surveillance activities of the NKVD. The calculation does not take into account the lower productivity of forced labor, which would presumably tip the balance well toward free labor. He finds in summary that "it is

gard. (2) A model of organizational conflict is conditional on a model of an organization. A model of an organization should require that a position be taken on the group-decision problem. Walton and McKersie are not very specific about such matters. (3) Three-party conflicts often give birth to coalition formation, destruction, and the essentially intransitive behavior that disrupts many labor(and other) negotiations. These considerations, which would seem to be of crucial importance in the steel situation, for example, receive little more than passing attention from Walton and McKersie.

But, then, the offer of such additional teeth might serve only to emphasize a mandibular deficiency of the discipline. The enamel offered by Walton and McKersie is of good quality, and fills some unattractive gaps.

LAWRENCE E. FOURAKER

Harvard University

The Economics of Vocational Rehabilitation. By RONALD W. CONLEY. Baltimore: Johns Hopkins Press, 1965. Pp. xii, 177. $6.00.

This book is a worthwhile though somewhat tedious addition to the growing literature on human capital. Ronald Conley's topic—the retraining of disabled workers—should interest those who are concerned either with the economics of medical care or of education.

Over three million people are currently excluded from the labor force because of physical or mental disorders, a loss of about $10 billion annually in national income. Numerous others can work only part-time or at low-productivity jobs. Assistance to the disabled is provided by many private charities and government agencies. Conley concentrates on the federal-state program of vocational rehabilitation, which currently restores more than 100,000 people yearly to gainful employment. His main contribution is an analysis of the costs and benefits of this program to the individuals directly involved and to society as a whole.

Ideally, in calculating the social value of rehabilitation one should include the resulting increase in national income, housekeeping, and other self-sufficient services, and the decrease in medical, custodial, and nursing care required by the disabled. These should be summed over the entire lifetime of the rehabilitee and discounted to the present. An imputation should be made for nonmaterial gains. These benefits should then be compared with the social costs of rehabilitation, including counseling services, training, special medical attention, and foregone earnings.

In practice, as is often the case, it is very difficult to carry through this ideal calculation. As a rough approximation of social benefits, Conley uses increased earnings. He realizes that this covers only the first component mentioned above—increased output—and is an imperfect measure even of that. Annual program expenditures are used as a rough approximation of social costs, although this too is an understatement. Between 1959 and 1963, the present value of future increased earnings for each year's rehabilitees ranged between $.7 and $2 billion, the exact amount depending on year, estimation method, and discount rate. Dividing by annual program costs, which grew from

$75 million to $117 million during this period, Conley derives a return of $10-$17 for every dollar invested.

Unlike some comparable studies, Conley recognizes that these average returns are not necessarily indicative of marginal returns, and it is the latter concept which is relevant to the question of optimum expenditures. According to Conley's admittedly crude estimates, during the past decade marginal increased earnings for the first year after rehabilitation have exceeded marginal program costs. But marginals seem even more troublesome to measure than averages, so one is relieved when Conley points out that, in theory, at least, an a priori determination of the optimum expenditure level is not needed. Given an efficient referral system and rehabilitation counselors qualified to weed out unsuitable cases—two prerequisites which are currently lacking—we can simply allow the system to grow to its optimum size on the basis of decentralized decision-making.

Conley develops another interesting policy conclusion, concerning the allocation of rehabilitation funds among different subgroups within the population. As one might expect, earnings after rehabilitation tend to be higher for those who are younger, more educated, white, and suffering from physical disability rather than mental illness or retardation. However, these characteristics seem much less reliable in predicting the *increase* in earnings due to rehabilitation, and even less in predicting increased earnings per dollar of rehabilitation costs. Apparently, these groups were doing better than the other disabled even before rehabilitation; also, their rehabilitation costs tend to be relatively high. Therefore, and somewhat surprisingly, it may be just as rational for society to help the low-productivity disabled as the high-productivity disabled.

In reading this book, one is inevitably led to wonder about the rationale for government intervention. If the benefits of rehabilitation so greatly exceed the costs, why are the disabled themselves unwilling or unable to bear the temporary expense? One might interpret the entire rehabilitation program as a compulsory insurance scheme which society has undertaken to fill a void left by the private sector. But what, if any, is the relevance of the more traditional theories of public finance? Conley provides information and ideas bearing on this issue, although he does not discuss it directly.

Certainly, rehabilitation is not a public good; it is parcelled out to individuals who could, if necessary, be compelled to pay. Economies of scale may be significant, but this has not prevented private production of other goods and even of some services used within the rehabilitation program itself, in return for compensation by the government. If the private sector has not functioned adequately in providing broader rehabilitation services, is this primarily because of externalities, imperfect capital markets, or some other form of failure? This is not a mere academic question, for different answers have different policy implications. If the difficulty lies in the inability of individuals to finance investment in themselves, even when profitable, a system of government loans or government-insured loans might be preferable to our current system of direct public payments, unless income redistribution is an explicit

goal. On the other hand, subsidies are the logical remedy for externalities.

Conley does not discuss capital market imperfections, undoubtedly an important aspect of the problem. Nor does he demonstrate the existence of the usual type of externality. However, he indirectly suggests some reasons for a divergence between private and social benefits. Although average returns are high, the variability of success among individuals is also high, and if these individuals are risk-averters, investment in rehabilitation may not be rational for them although it is for society as a whole. Also, society gains from the total (before-tax) productivity of the rehabilitee, while the individual gains only from his after-tax income.

Apparently, the chief source of disparity arises from other government payments to the disabled, such as social security, public assistance, and workmen's compensation. These payments will often diminish or disappear once the individual becomes self-supporting; hence, his net increases in income stemming from rehabilitation may be virtually nil. In contrast, the elimination of these subsidies upon rehabilitation constitutes a substantial gain to the taxpayer. Thus, we are led to the rather disturbing conclusion that a major rationale for future government activity is past government activity, and that this intervention has reduced the incentive of the individual to help himself.

On the whole, Conley's is a careful and straightforward presentation. He goes to great efforts to detail the various interpretations and limitations of his data. While this should be and generally is a strength of the book, unfortunately it also becomes a weakness. Because of the organization of his material, he is forced to repeat many of these points, each time in a slightly different version. Consequently, his book suffers from a redundant and cumbersome style, which partially blurs its major arguments and obscures its potentially provocative economic implications.

ESTELLE JAMES

Stanford University

Population; Welfare Programs; Consumer Economics

The Residual Factor and Economic Growth. Paris: Organisation for Economic Co-operation and Development, 1964. Pp. 279. Paper, $5.00.

This, another volume emanating from the OECD Study Group in the Economics of Education, contains the papers and formal comments presented at a 1963 Paris Conference devoted broadly to the subject matter denoted by the title. In fact, and not surprisingly, the almost universal concern is with education—both its contribution to past growth and its projection for future development. The principal contributions are by Denison, Svennilson, Tinbergen and Bos, and Vaizey, with observations thereon by more than a dozen discussants. There is relatively little here that is novel. Denison's attempt to measure the role of education in modern U.S. growth is well known, although here he also seeks to extend the method, unconvincingly, to cross-section international comparisons. Tinbergen and Bos report upon a variety of educational planning models rooted in the notion of a fixed manpower requirement for final output; a fuller account is to be found in another recent OECD monograph, *Econometric Mod-*

els of Education. Vaizey's interests span both worlds, and his skeptical reception of the premises of each will be familiar to readers of his earlier book. Finally, there is Svennilson's vintage capital approach to technical progress—hardly unique at this juncture—and perhaps less adequately explained here than elsewhere; indeed, since the relevance of all this to education appears only as a postscript, and then without conviction, it is not entirely clear why the material is included.

The contribution of the volume is twofold. Casual readers of the literature relating to education are permitted to savor of the principal conventional approaches to the subject as advanced by their leading expositors (the rate of return calculation favored by Schultz, Becker, *et al.*, is very closely related to Denison's method, and involves only an explicit allowance for cost). More well-informed students of the subject, on the other hand, can benefit from the direct confrontation of these different views as inspired by the conference.

The central point of contention, it soon becomes clear, is the way in which skilled labor enters the aggregate production function. Implicit in the Denison calculations, or more rightly, the American school, is a neoclassical view: smooth, continuous substitution among different types of labor of varying quality; the differential market wages associated with given educational levels then become the arbiter of the productivity of education. For Tinbergen, i.e., the Continental school, the central characteristic of the production relationship is complementarity: different skills are necessary to final output, in fixed proportions, whence it follows that marginal productivities are undefined and wages determined by other forces. Each group proceeds to criticize the other for its central premise, with the English almost equally disdainful of both. Kaldor and Balogh are particularly vigorous in their simultaneous denial of marginal productivity and reluctance at anything so formal as manpower planning, in each instance without specifying constructive alternatives. Vaizey, while hardly devoid of criticism, has the redeeming virtue of directing attention to the various shortcomings of all the current approaches—the failure to differentiate quality in the educational variable, for one, and the lack of concern with the interrelationship of education with diffusion of technological change, for another—in a positive vein.

Relatively little of the discussion, unfortunately, is devoted either to empirical analysis of the divergent views or to potential improvements in the application of each set of assumptions. Sen, however, did produce data showing widely divergent ratios of secondary and university students to output, which directly calls into question the assumption of inflexibility over large ranges. Fixity within the context of a planning model can be justified, but those fixed coefficients in turn must be related to the structures of wages and extent of capital in individual countries—a variation Tinbergen largely ignores. Likewise, one might wonder at the relevance of another favorite Tinbergen device—a solution in terms of balanced paths wherein all types of educational capital retain a constant ratio to output. No historical time series exhibits such behavior, and I would have thought the problem was precisely the initial insufficient ratio of human capital to output which required rectification.

The Denison method, too, is not without its problems. The failure within

the context of a neoclassical approach to allow for substitution between formal education and other types of investment in humans—health outlays, say, or job training—as well as with physical capital is certainly notable. Individuals with greater education may earn more in part because more of physical capital and of other forms of human capital are characteristic of their selected occupations. The Denison demonstration that a 60 per cent adjustment of actual wage differentials seems to reflect correctly inherent differences in intelligence levels still leaves this further point unresolved. Similarly, since more than half the contribution of formal education derives from an increasing length of the individual school year rather than curriculum years, this adjustment is of the greatest importance. Malinvaud shows that if elementary school pupils alone benefited from the change—as Denison himself seems to suggest—the appropriate adjustment is considerably smaller than Denison's own. This is apart from the question of whether added days of attendance are on a par with completion of added years—a question subject to empirical test (but not easily) by calculating the differential income of those groups receiving fewer days of education but equivalent years.

Given the divergent major objectives of the American and Continental schools—measurement and planning—it is not surprising that different approaches to the role of education should have evolved. Yet, each also has something to bring to the other. One of the most useful grafts to the planning strain would be the rate of return emphasis so characteristic of work in this country. Even when manpower requirements are absolute, such a calculation makes sense: does it cost more, in terms of discounted output, to generate the skills requisite for additional production than the production itself is worth? The assumption is that it does not, and hence that one should simply go ahead to the extent required, but a careful analysis of the structure of wages, and hence opportunity costs, is required to justify it—something the Continental approach does nothing to encourage. Once we relax the assumption of fixity to allow for multiple activities both in final production and within the educational sectors, we naturally approach more closely to a neoclassical world, and the rate of return becomes all the more central. Planners precisely should be the ones to concern themselves with maximizing the benefits of producing more or fewer graduates of a certain type by different techniques. (And they should also pay more attention to quantification of such indirect gains as complete literacy are supposed to provide to assure that the calculus bears some semblance to reality!)

On the other side, the manpower-requirements emphasis does afford a means of translating the exact calculations of the past influence of education into a more valuable policy tool. Denison's future projections carry less force because they assume the same pattern of educational productivity differentials to prevail in the future as in the past. What the manpower forecasts permit, imperfectly to be sure, is some assessment of changing demands and hence potential changes in the future rate of return. Governments, and individuals, influenced by such expectations will presumably make better decisions than by relying upon the calculated historical rates. Whether we are currently under-

investing in college education requires some estimate of 1970 and 1980 returns, not those of 1940, 1950, and 1960.

As this little volume shows, the progress in the economics of education over the last decade has been substantial, to the point of controversy. What it also demonstrates is the considerable research potential the field still possesses.

ALBERT FISHLOW

University of California, Berkeley

Education and Economic Development. Edited by C. ARNOLD ANDERSON AND MARY JEAN BOWMAN. Chicago: Aldine Publishing Co., 1965. Pp. x, 436. $10.75.

C. Arnold Anderson and Mary Jean Bowman have edited the papers presented to a conference on the role of education in the early stages of development held in Chicago in 1963. It was sponsored jointly by the Comparative Education Center of the University of Chicago and the Committee on Economic Growth of the Social Science Research Council.

The papers fall into four main groups. The first deals with "Human Resources" from the point of view of investment and includes a paper by A. Kahan on traditional Russian views of education as investments. These views are a surprising series of anticipations of work later undertaken at Chicago by Professor Schultz and his colleagues. A. Harberger then analyzes the relative rate of return on investments in men and in machines in India, and finds that the rate of return on a very conservative estimate is no less on men than it is on machines. This is a careful, analytical piece of work. H. Leibenstein and L. Hansen complete this section with two papers on "Shortages and Surpluses in Underdeveloped Countries" and a study of the needs for teachers in educational expansion. This last paper by Hansen is a particularly incisive and worthwhile contribution to a complex area of educational planning and represents a useful piece of analysis of a labor market having certain specialized features.

The second major section deals with "The Formation of Human Competencies" and is a survey of the way in which education has developed at home and in school and on-the-job, particularly in the United States. It is a collection of essays, including two by Mary Jean Bowman, dealing with a wide range of topics, from training on-the-job, to technical education in India at the beginning of the century. Perhaps the most important article in this section is one by Foster, "The Vocational School Fallacy in Development Planning." It is concerned with a special study of Ghana in which the author contends that the emphasis upon vocational school training in developing nations is mistaken and, in particular, he launches an impressive attack upon various ideas for the use of agricultural education as a basis for planning educational development. This article is not necessarily to be accepted as universally applicable without further investigation, but it represents a welcome piece of argument pointing in a different direction from what has come to be commonly accepted.

The last two sections of this book are concerned with "The Diffusion of

Schooling, Technologies, and Educational Opportunities" and represent an analysis of the way in which knowledge is diffused throughout the world and the way in which the labor force and the education system have developed in the course of economic development. This ends up with some studies of Czarist Russia, seventeenth- and eighteenth-century England, and the Meiji Period in Japan. C. Arnold Anderson develops an argument in his article (which is concerned with the level of literacy and schooling in some historical cases of societies on the point of takeoff) that a relatively high degree of literacy prevailed in England and New England, and that a system of elementary education "complete in itself," which was surprisingly extensive, emerged in these countries. But as development took place, education lagged. Partly, according to Anderson, this was because the urbanization of the workers led to greater social isolation and a fall in the provision of community facilities, partly because the sense of community identity atrophied, so that the pressures for all children from a village or market town to attend the local school died away, and partly because the middle classes came to provide themselves with their own schools. This is an interesting thesis that is not really confirmed by the other articles; though it is fair to say that they do not attack the same question directly. It has, of course, some relevance to the place that publicly-provided education—especially of a material kind—must play in economic development.

This is a worthwhile collection of articles which will make a useful introduction to this subject for those who are interested in the place of education in economic development.

John Vaizey

University of California at San Diego

TITLES OF NEW BOOKS

General Economics; Methodology

AMMER, D. S. Readings and cases in economics. Boston: Ginn and Co., 1966. Pp. 330. $3.04; paper, $2.40.

ARCHIBALD, K., ed. Strategic interaction and conflict—original papers and discussion. Berkeley: Inst. Internat. Stud., Univ. of California, 1966. Pp. viii, 227. Paper, $2.25.

Original papers by M. Deutsch, J. C. Harsanyi, H. H. Kelley, A. Rapoport, and T. C. Schelling.

BÖHLER, E. Der Mythus in Wirtschaft und Wissenschaft. Freiburg im Breisgau: Rombach, 1965. Pp. 580. DM 32.

BOULDING, K. E. Economic analysis. Vol. 1, Microeconomics. Vol. 2, Macroeconomics. 4th ed. New York: Harper & Row, 1966. Pp. xxiv, 720; xviii, 280.

CAZES, B La vie économique. Paris: Lib. A. Colin, 1965. Pp. 444. F 32.

HARLAN, H. C., ed. Readings in economics and politics. 2nd ed. New York: Oxford Univ. Press, 1966. Pp. xviii, 775. Paper, $2.75.

LANGE, O. Wholes and parts—a general theory of systems behaviour. Transl. from Polish by E. Lepa. New York: Pergamon Press, 1965. Pp. v, 74. $5.

First published as *Całość i rozwój w świetle cybernetyki by* PWN—Polish Scientific Publishers, 1962.

LIPSEY, R. G. AND STEINER, P. O. Economics. New York: Harper & Row, 1966, Pp. xxxii, 760.

MARTIN, R. S. AND MILLER, R. G., WITH BOULDING, K. E., consultant. Economics and its significance. With a concluding chapter for elementary and secondary teachers by R. H. Muessig and V. R. Rogers. Columbus, Ohio: Merrill Books, 1965. Pp. x, 165. $3.95; paper, $1.75.

MCCONNELL, C. R. Economic issues—readings and cases. 2nd ed. New York: McGraw-Hill, 1966. Pp. xii, 426. $3.95.

OHM, H Allgemeine Volkswirtschaftspolitik. Vol. 1, Systematisch-theoretische Grundlegung. 2nd ed. rev. Berlin: de Gruyter, 1965. Pp. 137. DM 3.60.

PARSONS, T. AND SMELSER, N. J. Economy and society—a study in the integration of economic and social theory. New York: Free Press, 1965. Pp. xxi, 322. Paper, $2.45.

PREISER, E. Nationalökonomie heute. Eine Einführung in die Volkswirtschaftslehre. 5th ed. Munich: Beck, 1965. Pp. 139. DM 6.80.

THALHEIM, K. C. Beiträge zur Wirtschaftspolitik und Wirtschaftsordnung. Gesammelte Aufsätze und Vorträge. Aus Anlass seines 65. Geburtstages hrsg. von der Abt. für Osteuropäische Wirtschaft des Osteuropa-Instituts und dem Institut für Weltwirtschaft der Freien Universität Berlin. Berlin: Duncker & Humblot, 1965. Pp. x, 440. DM 56.60.

WIRZ, L. Wirtschaftsphilosophie. Rekonstruktion der Wirtschaftstheorie. Heidelberg: Kerle; Löwe: Nauwelaerts, 1965. Pp. 264. DM 27.60.

A directory of information resources in the United States: social sciences. Washington: Supt. Docs., 1965. Pp. 224. Paper, $1.50.

Economic report of the President, transmitted to the Congress, January 1966, together with the annual report of the Council of Economic Advisers. Washington: Supt. Docs., 1966. Pp. 306.

Mélanges—publiés par la Faculté des Sciences économiques et sociales de l'Université de Genève à l'occasion de son cinquantenaire. Vol. 18. Geneva: Georg & Cie, Librairie de l'Université, 196. Pp. 410.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

ABRAMOVITZ, M. AND OTHERS. The allocation of economic resources. Stanford: Stanford Univ. Press, 1966. Pp. 244. Paper, $2.65.
Originally published in 1959.

BAUMOL, W. J. Welfare economics and the theory of the state. 2nd ed. with a new intro.: Welfare and the state revisited. Cambridge: Harvard Univ. Press, 1965. Pp. x, 212. $5.50.

BEHRENS, K. C. Der Standort der Handelsbetriebe. Cologne: Westdeutscher Verlag, 1965. Pp. xvi, 239. DM 24.80.

BERGSON, A. Essays in normative economics. Cambridge: Belknap Press of Harvard Univ Press, 1966. Pp. ix, 246. $5.95.

BODKIN, R. G. The wage-price-productivity nexus. Philadelphia: Univ. of Pennsylvania Press, 1965. Pp. 302. $8.

BOS, H. C. Spatial dispersion of economic activity. Rotterdam: Rotterdam Univ. Press, 1965. Pp. ix, 99. *f* 21.50.

BOUNIATIAN, M. Mes théories économiques et Albert Aftalion. Paris: Lib. Gen. de Droit et de Jurisprudence, 1966. Pp. 38. F 5.80.

DENIS, H. Histoire de la pensée économique. Paris: Presses Univ. de France, 1966. Pp 756. F 30.

FEIWELL, G. R. The economics of a socialist enterprise—a case study of the Polish firm New York: Praeger, 1965. Pp. xvi, 398. $17.50.

FELS, R. The law of supply and demand—a programmed approach. Boston: Allyn and Bacon, 1966. Pp. vi, 116. Paper, $1.85.
Originally part 1 of *Student workbook to accompany challenge to the American economy.*

FRISCH, R. Theory of production. Chicago: Rand McNally; Dordrecht: D. Reidel, 1965. Pp. xiv, 370. $11.
First published as *Memorandum fra Universitetets Sosialøkonomiske Institutt,* Oslo, Norway. Translated from Norwegian by R. I. Christophersen.

GEORGESCU-ROEGEN, N. Analytical economics—issues and problems. Cambridge: Harvard Univ. Press, 1966. Pp. xvi, 434. $12.95.

HAAS, C. Unsicherheit und Risiko in der Preisbildung. Cologne: Heymanns, 1965. Pp. 147 DM 18.50.

HOSELITZ, B. F. AND OTHERS. Theories of economic growth. New York: Free Press; London: Collier-Macmillan, 1965. Pp. 344. Paper, $2.45.
Contributions by J. J. Spengler, J. M. Letiche, E. McKinley, J. Buttrick, and H. J. Bruton.

JAFFÉ, W., ed. Correspondence of Léon Walras and related papers. Vol. 3, 1898-1909 and indexes. Amsterdam: North-Holland Pub. Co., 1965. Pp. xxiii, 538. $40 per set of 3 vols.

LAMBERTON, D. M. The theory of profit. Oxford: Basil Blackwell, 1965. Pp. vii, 211. 37s 6d.

LEFTWICH, R. H. The price system and resource allocation. 3rd ed. New York: Holt, Rinehart and Winston, 1966. Pp. xii, 369. $6.75.

MEISSNER, W. Oligopolanalyse im Rahmen eines allgemeinen Handlungsmodells. Berlin: Duncker & Humblot, 1965. Pp. 155. DM 26.60.

MOCKERS, J. -P. Dynamique et structures—La méthode structurale comme fondement de l'analyse dynamique en économie. Recherches écon. et fin., no. 12. Paris: Sirey, 1966. Pp. iii, 411. F 39.

MOLITER, B. Vermögensverteilung als wirtschaftspolitisches Problem. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965, Pp. xiv, 204. DM 27.50.

MÜLLER-GROELING, H. Maximierung des sozialen Gesamtnutzens und Einkommensgleichheit. Cologne: Heymanns, 1965. Pp. 151.

OTT, A. E., ed. Preistheorie. Cologne: Kiepenheuer, Witsch and Co., 1965. Pp. 554. DM 19.80.

PAPANDREOU, A G. Introduction to macroeconomic models—an analytical approach. Training seminar ser., no. 5. Athens, Greece: Center of Planning and Econ. Research, 1965. Pp. 200. Paper, $3.

PERROUX, F. La Pensée économique de Joseph Schumpeter. Les Dynamiques du capitalisme Geneva: Lib. Droz, 1965. Pp. 263. F 28.

PILISI, D. AND OTHERS. Une contribution à la théorie du revenu permanent. Paris: Presses Univ. de France, 1965. Pp. xii, 223. F 16.

ROBINSON, J. The accumulation of capital. 2nd ed. New York: St. Martin's Press; London: Macmillan, 1965. Pp. xvi, 440. Paper, $2.95.

———. Collected economic papers. Vol. 3. Oxford: Basil Blackwell, 1965. Pp. vii, 215. 30s.

———. The new mercantilism—an inaugural lecture. New York and Cambridge: Cambridge Univ. Press, 1966. Pp. 26. Paper, 75c.

ROSE, K. Theorie der Einkommensverteilung. Wiesbaden: Gabler, 1965. Pp. 88. DM 6.50.

SCHNEIDER, E. Einführung in die Wirtschaftstheorie. Vol. 1, Theorie des Wirtschaftskreislaufs. 12th ed. Vol. 3, Geld, Kredit, Volkseinkommen und Beschäftigung. 9th ed. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp viii, 165; x, 367. DM 14; 18.

———, ed. Probleme der Einkommenspolitik. Vorträge auf der Round-Table-Konferenz des Institute für Weltwirtschaft an der Universität Kiel vom 17.-19. Mai 1965. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. 215. DM 25.

SILVA LEMA, R. A. da. Contribuições à teoria da localizaçao industrial. (With English summary.) Bull. no. 39. São Paulo: Univ. de São Paulo, Faculdade de Ciencias Econ. e Adminis, 1965. Pp. x, 365. Mimeo.

WEINTRAUB, S. A Keynesian theory of employment growth and income distribution. Philadelphia: Chilton Books, 1966. Pp. x, 147. $7.95.

WINCH, D. Classical political economy and colonies. Cambridge: Harvard Univ. Press, 1965 Pp. vii, 184. $5.50.

Surveys of economic theory—prepared for the American Economic Association and the Royal Economic Society. Vol. 1, Money, interest, and welfare (Surveys I-IV). Vol. 2, Growth and development (Surveys V-VIII). New York: St. Martin's Press; London: Macmillan, 1965. Pp. xi, 222; xi, 271. $4.25 each vol.

Economic History; Economic Development; National Economies

ALTON, T. P. AND OTHERS. Polish national income and product in 1954, 1955, and 1956. New York: Columbia Univ. Press, 1965. Pp. viii, 252. $6.

AMUZEGAR, J. Technical assistance in theory and practice—the case of Iran. New York and London: Praeger, 1966. Pp. ix, 275. $15.

ANDREANO, R. L., ed. and comp. New views on American economic development: a selective anthology of recent work. Cambridge, Mass.: Schenkman, 1965. Pp. x, 434. $8.95.

ANDRUS, J. R. AND MOHAMMED, A. F. Trade, finance and development in Pakistan. Stanford: Stanford Univ. Press, 1966. Pp. xii, 289. $8.75.

ANTONY, H. Economic et prospective. Paris: Aubier, 1965. Pp. 206. F 15.

ASHLEY, C. A. AND SMAILS, R. G. H. Canadian Crown corporations—some aspects of their administration and control. Toronto: Macmillan; New York: St. Martin's Press, distrib., 1965. Pp. x, 360. $5.50.

BADEAU, J. S. AND STEVENS, G. G., eds. Bread from stones: fifty years of technical assistance. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. 133.

BAILEY, N. A., ed. Latin America—politics, economics, and hemispheric security. New York: Praeger, for Center for Strategic Studies, 1965. Pp. xviii 291. $9.

BAKLANOFF, E. N., ed. New perspectives of Brazil. Nashville: Vanderbilt Univ. Press, 1966. Pp. xvi, 328. $7.50.

BASU, S. K., GHOSH, A., AND RAY, S. Problems and possibilities of ancillary industries in a developing economy—a study based on the survey of ancillary units in West Bengal. Calcutta: World Press Private Ltd., 1965. Pp. viii, 161. Rs. 12.50.

BEHARI, B. Imports in a developing economy. Bombay: Vora, 1965. Pp. xii, 194. $5.

BLANC, A. L'Economie des Balkans. Paris: Presses Univ. de France, *s.d.* Pp. 128. F 2.50.

BOSMAN, H. W. J. Monetary and financial aspects of the problems of developing countries. The Hague: Mouton, 1965. Pp. 45.

BREWIS, T. N., ENGLISH, H. E., SCOTT, A., AND JEWETT, P. with stat. append. by GANDER, J. E. Canadian economic policy. Rev. ed. Toronto: Macmillan; New York: St. Martin's Press, distrib., 1965. Pp. xv, 463. Paper, $3.75.

CIPOLLA, C. M. Guns and sails in the early phase of European expansion, 1400-1700. London: Collins, 1965. Pp. 192. 30s.

COURT, W. H. B. British economic history 1870-1914—commentary and documents. New York: Cambridge Univ. Press, 1965. Pp. xviii, 495. $12; paper, $4.95.

DATT, R. AND SUNDHARAM, K. P. M. Indian economy. New Delhi: Niraj Prakashan, 1966. Pp. xiii, 785. Rs 14.50.

DEANE, P. The first Industrial Revolution. New York: Cambridge Univ. Press, 1965. Pp 295. $8; paper, $2.95.

DEMAS, W. G. The economics of development in small countries with special reference to the Caribbean. Centre for Developing-Area Stud., McGill Univ., Keith Callard Lectures, ser. 1. Montreal: McGill Univ. Press, for The Centre, 1965. Pp. xv, 150. $4.50.

DE MEO, G. Produttività e distribuzions de reddito in Italia nel periodo 1951-63. Annali di Stat., ser. 8, vol. 15. Rome: Instituto Centrale di Stat., 1965. Pp. x, 297.

EPSTEIN, F. T., TODD, A. C., AND VIEDERMAN, S., eds., WITH WHITTAKER, C. H. The American bibliography of Russian and East European studies for 1963. Russian and East European ser., vol. 32. Bloomington: Indiana Univ. Press, 1966. Pp. xvii, 116.

FISHLOW, A. American railroads and the transformation of the ante-bellum economy. Harvard Econ. Stud., vol. 127. Cambridge: Harvard Univ. Press, 1965. Pp. xv, 452. $10.

FURTADO, C. Diagnosis of the Brazilian crisis. Transl. by S. Macedo. Berkeley: Univ. of California Press, 1965. Pp. xxiv, 168. $5.

GABOVITCH, B. Economics of development. Tel Aviv: Afro-Asian Inst. for Labour Stud. and Coop., 1962. Pp. iv, 67.

GILLOT, F. Eléments de logique économique. Paris: A. Michel, 1965. Pp. 160. F 18.

GLOTZ, G. Ancient Greece at work—an economic history of Greece from the Homeric period to the Roman conquest. Transl. by M. R. Dobie. New York: Barnes & Noble, 1965. Pp. xii, 402. $7.50.
First published in 1926.

GOODWIN, C. D. W. Economic enquiry in Australia. Duke Univ. Commonwealth-Stud Center pub. no. 24. Durham: Duke Univ. Press, 1966. Pp. xv, 659. $12.50.

GUTH, W., ed. Die Stellung von Landwirtschaft und Industrie im Wachstumsprozess der Entwicklungsländer. Berlin: Duncker & Humblot, 1965. Pp. 69. DM 11.60.

HILDEBRAND, G. H. Growth and structure in the economy of modern Italy. Cambridge: Harvard Univ. Press, 1965. Pp. xx, 475. $11.95.

HOCEVAR, T. The structure of the Slovenian economy, 1848-1963. New York: Studia Slovenica, 1965. Pp. 277. $7; paper, $5.

HUGHES, J. The vital few—American economic progress and its protagonists. Boston: Houghton Mifflin, 1966. Pp. 504. $6.95.

HUMLOVA, O., ed. The Czechoslovak co-operative achievement. L. Sieber, trans. Prague: Central Co-op. Council, 1965. Pp. 303.

KETTELL, T. P. with intro. bibliog. and index by GREEN, F. M. Southern wealth and Northern profits as exhibited in statistical facts and official figures: showing the necessity of union to the future prosperity and welfare of the republic. University: Univ. of Alabama Press, 1966. Pp. xxv, 181. $5.95.
First published in 1860.

KROOSS, H. E. American economic development—the progress of a business civilization. 2nd ed. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xi, 498. $8.95.

LANE, F. C. Navires et constructeurs à Venise pendant la Renaissance. Paris: S.E.V.P.E.N., 1965. Pp. 309.

LAUTERBACH, A. Enterprise in Latin America—business attitudes in a developing economy. Ithaca: Cornell Univ. Press, 1966. Pp. xviii, 207. $6.75.

LÉVY-LEBOYER, M. Les Banques européenes et l'industrialisation internationale dans la première moitié du XIX^e siècle. Paris: Presses Univ. de France, 1964. Pp. 814. F 65.

LITTLE, I. M. D. AND CLIFFORD, J. M. International aid—a discussion of the flow of public resources from rich to poor countries with particular reference to British policy. London. Allen & Unwin, 1965. Pp. 360. 45s.; paper 25s.

LOUIS, P. *Ancient Rome at work—an economic history of Rome from the origins to the* empire. Transl. by E. B. F. Wareing. New York: Barnes & Noble, 1965. Pp. xiv, 347. $7.50.
First published in 1927.

MAMALAKIS, M. AND REYNOLDS, C. W. Essays on the Chilean economy. Homewood, Ill.: Irwin, for Econ. Growth Center, Yale Univ., 1965. Pp. xxii, 409. $8.50.

MELOTTI, U. Fame e sottosviluppo nel mondo. Milan: Ed. La Culturale, 1966. Pp. 222. L. 1.700.

MÖLLER, B. U-länder i omvälvning. (Underdeveloped countries in transition.) Stockholm: Bokförlaget Aldus/Bonniers, 1965. Pp. 265. Paper, SKR. 13.75.

MOLNOS, A. Die socialwissenschaftliche Erforschung Ostafrikas 1954-1963 (Kenya, Tanganyika/Sansibar, Uganda). Berlin: Springer-Verlag., 1965. Pp. xv, 304. DM 43.

NORTH, D. C. The economic growth of the United States 1790-1860. New York: Norton, 1966 Pp. xv, 304. Paper, $1.95.
First published by Prentice-Hall, 1961.

PRASAD, B. Planned capital formation in India—a critique of Keynesian fiscal and monetary policies Bombay: Vora, 1965. Pp. xiv, 224. $5.

RIGAUDIERE, A. ZYLBERMAN, E., AND MANTEL, R. Etudes d'histoire économique rurale au XVIII^e siècle. Paris: Presses Univ. de France, 1965. Pp. 208. F 15.

SINGH, V. B., ed. Economic history of India: 1857-1956. Bombay: Allied Pub., 1965. Pp. xxxiv, 795. Rs 30.

STOBBE, H. AND CROUSAZ, V. v. Methodisch-theoretisches Schrifttum zur wirtschaftlichen Entwicklung in Entwicklungsländern und Schrifttum zur Entwicklungspolitik. Kieler Schrifttumskunden zu Wirtschaft und Gesellschaft no. 9. Kiel: Inst. f. Weltwirtschaft, Univ. Kiel, 1965. Pp. vi, 474, DM 30.

STONEHILL, A. Foreign ownership in Norwegian enterprises. (Utenlandske eierinteresser i Norske bedrifter.) Stud. in Nat. Econ., no. 14. Oslo: Central Bur. Stat. of Norway, 1965. Pp. 213. NKr 12.

TOUSSAINT, J. R. De Labour Party en de Britse volkshuishouding—Programmapunten en beleidsdaden 1918, 1945, 1951. (The Labour Party and the British economy—points of programmes and acts of policy 1918, 1945, 1951.) (With English summary.) Schiedam: N.V. Drukkerij De Eendracht, 1965. Pp. 208.

TSUNOYAMA, S. A concise economic history of modern Japan. Bombay: Vora, 1965. Pp ix, 132. $3.

VAN MEERHAEGHE, M. A. G. Aktuelle Probleme der belgischen Wirtschaftspolitik. Kieler Vorträge, n.s. no. 43. Kiel: Inst. f. Weltwirtschaft, Univ. Kiel, 1966. Pp. 21. DM 3.

WILCOX, C., WEATHERFORD, W. D., JR., HUNTER, H., AND BARATZ, M. S. Economies of the world today—their organization, development, and performance. 2nd ed. New York: Harcourt, Brace & World, 1966. Pp. x, 171. Paper, $1.95.

WILSON, T., ed. Papers on regional development. Suppl. to *Jour. Indus. Econ.* Oxford: Basil Blackwell, 1965. Pp. xiv, 130 35s.

WORMS, G. Les Méthodes modernes de l'économie appliquée. Paris: Paris: Dunod, 1965. Pp. 244. F 34.

All India rural household survey. Vol. 2, Income, investment and saving. New Delhi: Nat. Coun. of Applied Econ. Research, 1965. Pp. xvii, 139. $4.

L'Année politique, économique, sociale et diplomatique en France, 1964. Paris: Presses Univ. de France, 1965. Pp. 463. F 60.

Annual report, July 1, 1964-June 30, 1965. Karachi: Pakistan Inst. Develop. Econ., *s.d.* Pp. 20.

Basic statistics for formulating and implementing plans of economic and social development in countries of Asia and the Far East. U.N. Econ. Comm. for Asia and the Far East, sales no. 65.II. F. 8. New York: Sales Sec., U.N., 1965. Pp. 50.

Energy development in the countries of the East African sub-region. U.N. Econ. Comm. for Africa. New York: Sales Sec., U.N., 1965. Pp. 133.

Industrial co-ordination in East Africa: a quantified approach to first approximations. U.N. Econ. Comm. for Africa. New York: Sales Sec., U.N., 1965, Pp. 132.

Información estadística del Ministerio de Hacienda. Precedida de los discursos pronunciados ante las Cortes Española por el Excmo. Señor Ministro de Hacienda D. Mariano Navarro Rubio en los dias 6 de Junio y 22 de Diciembre de 1964. [Madrid]: Secretaría Gen. Técnica del Ministerio de Hacienda, 1965. Pp. 198.

Panorama de la economia Asturiana. Consejo economico sindical de Asturias no. 2. Madrid: Cabinete Técnico del Consejo Econ. Sindical Nacional, 1965. Pp. 63.

Problems and prospects of the textile industry in Latin America. U.N. Econ. Comm. for Latin America. New York: Sales Sec., U.N., 1965. Pp. 42.

El Salvador en gráficas, 1964. San Salvador C. A.: Direccion Gen. de Estadistica y Censos, Ministerio de Econ., 1965. Pp. 96. Gratis.

Saving in India 1950-51 to 1961-62. New Delhi: Nat. Coun. of Applied Econ. Research, 1965. Pp. xii, 185. $4.

Small industry in East Africa. U.N. Econ. Comm. for Africa. New York: Sales Sec., U.N., 1965.

Third Five-Year Plan and other papers. Papers read at the 12th Pakistan Economic Conference held at Peshawar from March 1 to 3, 1965. Rawalpindi: Pakistan Econ. Assn., [1965]. Pp. iii, 325. Rs 20.

Yearbook of Nordic statistics 1965. Stockholm: The Nordic Council, 1966. Pp. 118.

Statistical Methods; Econometrics; Social Accounting

BRENNEN, M. J. Preface to econometrics—an introduction to quantitative methods in economics. 2nd ed. Cincinnati: South-Western, 1965. Pp. viii, 440. $7.25.

BRYANT, E. C. Statistical analysis. 2nd ed. New York: McGraw-Hill, 1966. Pp. ix, 321. $7.95.

CULMANN, H. Les Comptabilités nationales. Paris: Presses Univ. de France, 1965. Pp. 128 F 2.50.

DESABIE, J. Théorie et pratique des sondages. Paris: Dunod, 1965. Pp. 500. F 79.

DUESENBERRY, J. S., FROMM, G., KLEIN, L. R., AND KUH, E., eds. The Brookings quarterly econometric model of the United States. Chicago: Rand McNally; Amsterdam: North-Holland Pub. Co., 1965. Pp. xv, 776.

ESENWEIN-ROTHE, I. Statistik an deutschen Universitäten mit besonderer Berücksichtigung der Wirtschaftsstatistik. Stuttgart: Kohlhammer, 1965. Pp. 48. DM 6.80.

FEINSTEIN, C. H. Domestic capital formation in the United Kingdom 1920-1938. New York: Cambridge Univ. Press, 1965. Pp. xii, 270. $17.50.

HÉLINE, R. Etude méthodologique du capital national. Paris: Gauthier-Villars, 1965. Pp. 417. F 86.

HOROWITZ, C. M. Basic statistics as applied to the social sciences and business. Brooklyn: Faculty Press, 1966. Pp. viii, 132. Paper, $4.95.

HÜRLIMANN, W. Lineare Programmierung. Eine Einführung für Kaufleute. Düsseldorf: Schilling, 1965. Pp. 94. DM 9.60.

JOHNSTON, J. B., PRICE, G. B., AND VAN VLECK, F. S. Linear equations and matrices. Reading, Mass.: Addison-Wesley, 1966. Pp. viii, 308. $8.50.

KREYSZIG, E. Statistische Methoden und ihre Anwendungen. Göttingen: Vandenhoeck & Ruprecht, 1965. Pp. 422. DM 32.

MAINEI, P. Calcul économique en agriculture: Application des programmes linéaires et des joux. Paris: Dunod, 1965. Pp. 198. F 28.

NEMCHINOV, V. S., ed. of Russian ed.; NOVE, A., ed. of English ed. The use of mathematics in economics. Cambridge: M.I.T. Press, 1964. Pp. xxi, 377. $12.50.

Contributions by: O. Lange, A. L. Lur'e, L. V. Kantorovich, V. S. Nemchinov, and V. V. Novozhilov.

PARENTI, G., ed. Modelli econometrici per la programmazione—atti del Convegno di studi sui modelli di programmazione nei paesi della Communità Economica Europea. Florence: Scuola di Statistica dell'Univ. di Firenze, 1965.

PIATIER, A., CAHUZAC, P., AND CHAMBADAL, L. Economie et mathématiques—eléments et exercices. Vol. 1, Les Nombres: algèbre, analyse. Paris: Presses Univ. de France, 1965. Pp xii, 508. F 24.

RAO, C. R., ed, WITH LAHIRI, D. B., PANT, P., NAIR, K. R., AND SHRIKHANDE, S. S. Contributions to statistics. Presented to Professor P. C. Mahalanobis on the occasion of his 70th birthday. New York: Pergamon Press, *s.d.* Pp. 528. $18.

ROSENTIEHL, P. AND MOTHES, J. Mathématiques de l'action: Langage des ensembles, des statistiques et des aléas. Paris: Dunod, 1965. Pp. 500. F 62.

SMITH, W. L. AND WILKINSON, W. E., eds. Proceedings of the symposium on congestion theory held at the University of North Carolina at Chapel Hill August 24-26, 1964. Chapel Hill: Univ. of North Carolina Press, 1965. Pp. xv, 457. $14.

SONQUIST, J. A. AND MORGAN, J. N. The detection of interaction effects—a report on a computer program for the selection of optimal combinations of explanatory variables. Monogr. no. 35. Ann Arbor: Survey Research Center, Inst. for Sco. Research, Univ. of Michigan, 1964. Pp. xi, 292.

THOMPSON, R. G., COLYER, D., WILSON, R., AND BROCK, W. Computational techniques. Columbia: Research Center, School of Bus. and Public Admin., Univ. of Missouri, 1965. Pp. 112.

TILANUS, C. B. Input-output experiments—the Netherlands 1948-1961. Rotterdam: Rotterdam Univ. Press, 1966. Pp. 141. Paper, *f* 24.50.

ZUSMAN, P. AND HOCH, I. Resources and capital requirement matrices for the California economy. Giannini Foundation research rept. no. 284. Berkeley: College of Agric., Univ. of Calif., 1965. Pp. iv, 89.

On political economy and econometrics—essays in honour of Oskar Lange. New York: Pergamon; Warsaw: Polish Sci. Pub., 1965. Pp. viii, 661. $18.50.

Economic Systems; Planning and Reform; Cooperation

ANGERS, F. A., AND OTHERS. Problèmes de conversion économique. Paris: Ed. Génin, 1965. Pp. 400. F 36.

ASHFORD, D. E. Morocco-Tunisia: politics and planning. Nat. planning ser. no. 2. Syracuse: Syracuse Univ. Press, 1965. Pp. xx, 65. Paper, $2.75.

BARDET, P. Die Organisation der Planung. Stuttgart: Kohlhammer, 1965. Pp. 36. DM 4.80.

Bettelheim, C., Charriere, J., and Marchisio, H. La Construction du socialisme en Chine. Paris: F. Maspéro, 1965. Pp. 184. F 17.50.

Burke, F. G. Tanganyika: preplanning. Nat. planning ser. no. 3. Syracuse: Syracuse Univ. Press, 1965. Pp. xxix, 108. Paper, $3.25.

Cairncross, A. K. The short term and the long in economic planning—tenth anniversary lecture . . ., January 6, 1966. Washington: Econ. Develop. Inst., 1966. Pp. 30.

Fiechter, J. J. Le Socialisme français: de l'affaire Dreyfus à la grande guerre. Geneva: Lib. Droz, 1965. Pp. 293, F 44.

Friedmann, J. Venezuela: from doctrine to dialogue. Nat. planning ser. no. 1. Syracuse: Syracuse Univ. Press, 1965. Pp. xxiii, 87. Paper, $2.95.

Gruchy, A. G. Comparative economic systems—competing ways to stability and growth Boston: Houghton Mifflin, 1966. Pp. x, 908. $9.95.

Heide, H. Die langfristige Wirtschaftsplanung in Schweden. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. viii, 147. DM 28.

Hesse, K. Planungen in Entwicklungsländern. Eine Einführung in Wesen und Praxis des Entwicklungsplanens an Hand von 6 Beispielen. Berlin: Duncker & Humblot, 1965. Pp. xx, 618. DM 69.

Iyengar, S. K. Fifteen years of democratic planning. Vol. 2, Sectoral developments. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. xvi, 693. $18.

Masnata, A. Le système socialiste-soviétique—essai d'une étude générale de son économie Neuchatel: Ed. Baconnière; Paris: Société Française du Livre, 1965. Pp. 348. 27 Sw. fr

Nove, A. The Soviet economy—an introduction. Rev. ed. New York: Praeger, 1966. Pp xxvi, 354. $7; paper, $2.50.

Perkins, D. H. Market control and planning in Communist China. Cambridge: Harvard Univ. Press, 1966. Pp. viii, 291. $7.

Perroux, F. Les Techniques quantitatives de la planification. Paris: Presses Univ de France, 1965. Pp. 316. F 20.

Rao, C. R., ed., with Lahiri, D. B., Pant, P., Nair, K. R., and Shrikhande, S. S. Essays on econometrics and planning. Presented to Professor P. C. Mahalanobis on the occasion of his 70th birthday. New York: Pergamon Press, *s.d.* Pp. 354. $15.

Rosovsky, H., ed. Industrialization in two systems: essays in honor of Alexander Gerschenkron, by a group of his students. New York, London and Sydney: Wiley, 1966. Pp. ix 289, $6.95.

Szczepanik, E. F. National accounting as a tool of economic planning. The Hague: Mouton, 1965. Pp. 61.

Waterston, A. with Martin, C. J., Schumacher, A. T., and Steuber, F. A. Development planning: lessons of experience. Baltimore: Johns Hopkins Press, 1965. Pp. xix 706. $10.75.

Information supplement to *Soviet Studies*. Glasgow: Univ. of Glasgow, 1966. Pp. 36.

Problems of economic dynamics and planning—essays in honour of Michal Kalecki. New York: Pergamon Press; Warsaw: Polish Sci. Pub., 1966. Pp. viii, 494. $15.50.

Business Fluctuations

Bird, C. The invisible scar. New York: David McKay, 1966. Pp. xviii, 364. $5.95.

Burns, A. F. The management of prosperity. 1965 Benjamin F. Fairless Memorial Lectures. New York: Carnegie Inst. of Technology; Columbia Univ. Press, distrib., 1966 Pp. 70. $3.50.

Hultgren, T. with Pech, M. R. Cost, prices, and profits: their cyclical relations. NBER stud. in bus. cycles no. 14. New York: Nat. Bur. Econ. Research; Columbia Univ. Press distrib. 1965. Pp. xxvi, 229. $6.

Obradovitch, M. Les Récessions américaines d'après-guerre et leur impact sur le rest du monde. Observation écon., vol. 21. Paris: C.D.U. and SEDES, 1965. Pp. 372. F 32

SEPÚLVEDA, C. Los indicadores económicos. Inst. Econ. pub. no. 78. Santiago: Inst. Econ. Univ. Chile, 1965. Pp. xiii, 134.

La France économique en 1964. *Reveu d'Econ. Pol.,* July-Oct. 1965. Paris: Sirey, 1965. Pp iii, 442.

Stabilisierung ohne stagnation. Stuttgart: Kohlhammer, 1965. Pp. xv, 263.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

ARNDT, H W. AND HARRIS, C. P. The Australian trading banks. 3rd ed. Melbourne: F. W. Cheshire, 1965. Pp. vi, 229.

BEN-SHAHAR, H. Interest rates and the cost of capital in Israel 1950-1962. Basel: Kyklos-Verlag; Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. viii, 136. DM 22.50.

BRUNNER, G A Personal bankruptcies: trends and characteristics Bur. Bus. Research monogr. no. 124. Columbus: Bur. Bus. Research, Div. Research, College of Commerce and Admin., Ohio State Univ., 1965. Pp. xv, 122.

CARSON, D., ed Money and finance: readings in theory, policy and institutions. New York: Wiley, 1966. Pp. 372.

CRICK, W. F., ed. Commonwealth banking systems. New York: Oxford Univ. Press; Oxford: Clarendon Press, 1965. Pp. xi, 536. $8.

DOODHA, K. D. India: instalment credit, extent, stability, growth. Univ. of Bombay ser. in monetary and internat. econ. no. 9. Bombay: Manaktala, 1966. Pp. xx, 304. Rs 17.50.

FRAZER, W J., JR The liquidity structure of firms and monetary economics. Univ. of Florida soc. sci monogr. no. 27. Gainesville: Univ. of Florida Press, 1965. Pp. 91. $2.

———, AND YOHE, W P. Introduction to the analytics and institutions of money and banking. Princeton, N.J.: Van Nostrand, 1966. Pp. xi, 682. $10.75.

HANSON, W. C. AND WANG, F. W., eds. Capital sources in the United Kingdom—a directory. New York Simmons-Boardman, 1966. Pp. 164. $10.

LANGER, W. Sicherung der Geldwertstabilität—eine Aufgabe der Konjunktur-, Struktur- und Wachstumspolitik. Kieler Vorträge, n s. no. 44. Kiel: Inst. f. Weltwirtschaft, Univ. Kiel, 1966 Pp. 20.

MINSKY, H P, ed. California banking in a growing economy: 1946-1975. Berkeley, Calif.: Inst of Bus. and Econ. Research, 1965. Pp. xiv, 356.

MORS, W P Consumer credit finance charges: rate information and quotation. NBER stud in consumer instalment fin. no. 12. New York: Nat. Bur. Econ. Research; Columbia Univ Press, distrib., 1965. Pp. xvii, 133. $5.

ROWE, J Z The public-private character of United States central banking. New Brunswick: Rutgers Univ. Press, 1965. Pp. 210. $6.

SMITH, A. E. George Smith's money—a Scottish investor in America. Madison: State Hist. Soc Wisconsin, 1966. Pp. vii, 197. $4.50.

TANNAN, M. L. Banking law and practice in India. 11th ed. rev. Bombay: Thacker, 1965. Pp 614.

URI, P. WITH DE KALDOR, N., RUGGLES, R., AND TRIFFIN, R. Une politique monétaire pour l'Amérique latine. Paris: Plon, 1965. Pp. 114. F 9.72.

WITTGEN, R Die Geldpolitik der Geschäftsbanken. Frankfurt am Main: Knapp, 1965. Pp. 170. DM 18.

Aspectos financieros de las economías latinoamericanas 1964. Mexico: Centro Estud. Mon. Latinoamericanos (CEMLA), 1965. Pp. 263.

Consolidation of bank examining and supervisory functions. Hearings before the House Subcommittee on Bank Supervision and Insurance of the Committee on Banking and Currency and the Committee on Banking and Currency, 89th Cong., 1st sess. Washington: Supt. Docs, 1965. Pp. 602.

Los mercados de capitales en América Latina. Mexico: Centro Estud. Mon. Latinoamericanos (CEMLA), 1966. Pp. 213.

Supplement to *Banking and Monetary Statistics*. Sec. 9, Federal Reserve Banks. Sec. 12, Money rates and securities markets. Washington: Board of Governors, Federal Reserve System, *s.d.* Pp. iv, 36; vi, 182. 35c; 65c.

Public Finance; Fiscal Policy

ARDANT, G. La Théorie sociologique de l'impôt. 2 vols. Paris: S.E.V.P.E.N., 1965. Pp. 1,212. F 160.

BOWER, J. B., NISWONGER, C. R., AND LANGENDERFER, H. Q. Income tax procedure: 1966 edition—for returns of 1966 income and 1966 tax planning. Cincinnati: South-Western, 1966. Pp. iv, 292. $4.96.

CAMERON, H. A. AND HENDERSON, W. Public finance—selected readings. New York: Random House, 1966. Pp. vii, 310. Paper, $3.50.

COMIEZ, M. S. A capital budget statement for the U.S. Government. Washington: Brookings Inst., 1966. Pp. xv, 143. $6.

DÄHNE, E. Inflationsbekämpfung in unserer Zeit. Frankfurt am Main: Knapp, 1965. Pp. 136. DM 12.80.

DE BRUNHOFF, S. Capitalisme financier public—influence économique de l'Etat en France (1948-1958). Paris: SEDES, 1965. Pp. 212.

EINAUDI, L. Cronache economiche e politiche di un trentennio (1893-1925). Vol. 8, 1925 Turin: G. Einaudi, 1965. Pp. xi, 588.

HALLER, H. Finanzpolitik. Grundlagen und Hauptprobleme. 3rd ed. rev. Tübingen: J.C.B. Mohr (Paul Siebeck); Zurich: Polygraphischer Verlag, 1965. Pp. x, 354. DM 25.

JOHANSEN, L. Public economics. Chicago: Rand McNally; Amsterdam: North-Holland Pub. Co., 1965. Pp. viii, 367.

KEITH, J. H. Property tax assessment practices—a reference book for the assessor, appraiser, accountant, attorney and the student. Monterey Park, Calif.: Highland Pub Co., 1966. Pp. xxiv, 544. $15.

MARION, G. Répartition fonctionnelle des revenus—analyse de la part du travail au Canada. Montreal: Presses Univ. de Montréal, 1965. Pp. 125. $5.50.

MUSGRAVE, R. A., ed. Essays in fiscal federalism. Washington: Brookings Inst., 1965. Pp xvi, 301. $6.

NETZER, D. Economics of the property tax. Washington: Brookings Inst., 1966. Pp. xviii, 326. $6.75.

NEUMARK, F. Grundsätze der Besteuerung in Vergangenheit und Gegenwart. Wiesbaden: Steiner, 1965. Pp. 35. DM 4.50.

NOVICK, D., ed. Program budgeting—program analysis and the federal budget. Cambridge: Harvard Univ. Press, 1965. Pp. xvi, 382. $6.50.

RECKTENWALD, H. C. Die Finanzwissenschaft unserer Zeit. Stuttgart: Kohlhammer, 1965. Pp. 40. DM 6.20.

REVIGLIO, F. La teoria della curva di domanda e gli effetti delle imposte. Stud. del Laboratorio di Econ. Pol., Univ di Torino, ser. III^a, vol. 1. Milan: A. Giuffrè, 1965. Pp. 255. L. 2.000.

SCHMÖLDERS, G. Finanzpolitik. Berlin and New York: Springer-Verlag, 1965. Pp. viii, 544. DM 48.

VENKATARAMAN, K. Local finance in perspective. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. v, 63. $3.75.

Depreciation policy and the reserve ratio test—a symposium. Tax symposium pub. no. 2. New York: Nat. Assn. of Manufacturers, [1965]. Pp. 93. $3.

Problems of tax administration in Latin America—papers and proceedings of a conference held in Buenos Aires, Argentina, October, 1961. Baltimore: Johns Hopkins Press for Joint Tax Program OAS/IDB/ECLA, 1965. Pp. x, 574. $10.

State and local taxes on business. Symposium conducted by the Tax Inst. of America, October 28-30, 1964. Princeton: Tax Inst. of Am., 1965. Pp. xiv, 352. $8.

Statistics of income 1963: individual income tax returns. Treasury Dept., Internal Revenue Svce., pub. no. 79 (1-66). Washington: Supt. Docs., 1966. Pp. 208. $1.25.

International Economics

ALIBER, R. Z. The future of the dollar as an international currency. New York: Praeger, 1966. Pp. 169. $6.

ALLEN, W. R., ed. International trade theory: Hume to Ohlin. New York: Random House, 1965. Pp. 210.

AVRAMOVIC, D. AND GULHATI, R. Debt servicing problems of low-income countries 1956-1958. Baltimore: Johns Hopkins Press, 1960. Pp. xvii, 74. Paper, $2.50.

BACHMANN, H. Zollpolitik und Entwicklungsländer. Die Zollpolitik der Industriestaaten als Hindernis der witschaftlichen Entwicklung unterentwickelter Länder. Tübingen: J.C.B. Mohr (Paul Siebeck); Zurich: Polygraphischer Verlag, 1965. Pp. x, 121. DM 17.50.

BALDWIN, R. E., AND OTHERS. Trade, growth, and the balance of payments—essays in honor of Gottfried Haberler. Chicago: Rand McNally; Amsterdam: North-Holland Pub Co., 1965. Pp. viii, 267. $8.50.

CASSELL, F. Gold or credit? The economics and politics of international money. New York and London: Praeger for Federal Trust for Educ. and Research, London, 1965. Pp. ix, 216. $6.

DAS GUPTA, A. K., ed. Trade theory and commercial policy in relation to underdeveloped countries—proceedings of the seminar on international trade held under the auspices of the Indian School of International Studies in March 1963. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. viii, 108. $5.25.

EINZIG, P. The Euro-dollar system—practice and theory of international interest rates. 2nd ed. New York: St. Martin's Press; London: Macmillan, 1965. Pp. xiii, 183. $7.50.

FOLDI, T., ed. Studies in international economics. Budapest: Akadémiai Kiadó, 1966. Pp. 156. $4.20.

Contributions by G. Göncöl, G. Kovásznai, F. Kozma, F. Molnár, D. Sóki and B. Tálas.

GANGULI, B. N. Dadabhai Naoroji and the drain theory. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. ix, 152. $6.

HABERLER, G. Geld in der internationalen Wirtschaft. Eine Studie über Zahlungsbilanzausgleich, internationale Liquidität und Wechselkurse. Transl. from English by W. Michalski. Hamburg: Verlag Weltarchiv, 1965. Pp. 56. DM 9.80.

Originaly published as *Money in the International economy.* Hobart paper no. 31, 1965.

HALM, G. N. Die Vergrösserung der Bandbreite für Devisenkursschwankungen. Berlin: Duncker & Humblot, 1965. Pp. 68. DM 8.

HAWKINS, R. G., ed. Compendium of plans for international monetary reform. Bull. no. 37-38. New York: C. J. Devine Inst. of Fin., Grad. School of Bus. Admin., New York Univ., 1965. Pp. 131.

HAWKINS, R. G. AND ROLFE, S. E. A critical survey of plans for international monetary reform. Bull. no. 36. New York: C. J. Devine Inst. of Fin., Grad. School of Bus. Admin., New York Univ., 1965. Pp. 80.

JOHNSON, H. G. The world economy at the crossroads—a survey of current problems of money, trade and economic development. New York: Oxford Univ. Press; London: Clarendon Press, 1966. Pp. 106. $3.50.

KIDRON, M. Foreign investments in India. New York: Oxford Univ. Press, 1965. Pp. xv, 368. $8.

KINDLEBERGER, C. P. Los déficit de balanza de pagos y la demanda internacional de liquidez. Ensayos no. 15. Mexico: Centro Estud. Mon. Latinoamericanos (CEMLA), 1965. Pp. 50.

KÖHLER, H. Economic integration in the Soviet Bloc—with an East German case study. New York: Praeger, 1965. Pp. xxi, 402. $15.

McCOLL, G. D. The Australian balance of payments—a study of post-war developments. Melbourne: Melbourne Univ. Press; New York: Cambridge Univ. Press, 1965. Pp. xiii, 180. $9.

PERUMBULAVIL, V., comp. The European Economic Community and New Zealand—a checklist. Bibliog. ser. no. 8. Wellington, New Zealand: R. E. Owen, Govt. Printer, 1965. Pp. v, 44.

ROOSA, R. V. Monetary reform and the world economy. New York: Harper & Row, 1965. Pp. lx, 173. $1.95.

ROSE, K., ed. Theorie der internationalen Wirtschaftsbeziehungen. Cologne: Kiepenheuer, Witsch and Co., 1965. Pp. 455. DM 19.80.

ROWE, J. W. F. Primary commodities in international trade. New York: Cambridge Univ. Press, 1965. Pp. xi, 224. $6.50; paper, $2.45.

SCHNEIDER, E., ed. Weltwirtschaftliche Probleme der Gegenwart. Verhandlungen auf der Tagung des Vereins für Socialpolitik, Gesellschaft für Wirtschafts- und Socialwissenschaften im Ostseebad Travemünde, September 21-24, 1964. Berlin: Duncker & Humblot, 1965. Pp. vii, 690. DM 78.60.

SCITOVSKY, T. Requirements of an international reserve system. Essays in internat. fin. no. 49. Princeton: Internat. Fin. Section, Dept. of Econ., Princeton Univ., 1965. Pp. 18.

SHANNON, I. International liquidity—a study in the economic functions of gold. Chicago: Regnery, 1966. Pp. 143. $5.
Originally published in 1964 by F. W. Cheshire, Australia.

SINGH, R. K. Prosperity through export. Chicago and Calcutta: Engineering Export Promotion Council, [1965]. Pp. xi, 285. Rs 25.

TONTZ, R. L., ed. Foreign agricultural trade—selected readings. Ames: Iowa State Univ. Press, 1966. Pp. xx, 500. $7.50.

VAJDA, I. The role of foreign trade in a socialist economy—new essays in persuasion Budapest: Corvina Press, 1965. Pp. 336.

WILLIAMSON, J. H. The crawling peg. Essays in internat. fin. no. 50. Princeton: Internat. Fin. Section, Dept. of Econ., Princeton Univ., 1966. Pp. 25. Single copies free.

WONNACOTT, P. The Canadian dollar 1948-1962. Toronto: Univ. of Toronto Press, 1965. Pp. xviii, 339. $6.50.

YEAGER, L. B. International monetary relations—theory, history, and policy. New York: Harper & Row, 1966. Pp. viii, 504.

East-West commercial relations. Study conference, Brussels, May 14th and 15th, 1965. Pub. no. 40. Brussels: European League for Econ. Co-op., *s.d.* Pp. 70.

Summary proceedings—1965 annual meetings of the Boards of Governors, Washington, D.C.—September 27-October 1, 1965. Washington: Inter. Bank for Reconstruction and Develop., 1965. Pp. 124.
Copies in French and Spanish will be available soon. Requests should be sent to Secretary, International Bank for Reconstruction and Development, 1818 H. Street N.W., Washington, D.C. 20433.

Business Finance; Investment and Security Markets; Insurance

BAUMOL, W. J. The stock market and economic efficiency. The Millar Lectures, no. 6, 1965. New York: Fordham Univ. Press, 1965. Pp. xiii, 95. $3.

CAGAN, P. The effect of pension plans on aggregate saving: evidence from a sample survey. NBER occas. paper no. 95. New York: Nat. Bur. Econ. Research; Columbia Univ. Press, distrib., 1965. Pp. xii, 105. $3.

COHEN, J. B. AND ROBBINS, S. M. The financial manager—basic aspects of financial administration. New York: Harper & Row, 1966. Pp. xi, 1006. Text ed., $12.50.

DIETZ, P. O. Pension funds—measuring investment performance. New York: Grad. School of Bus., Columbia Univ., and Free Press of Glencoe, 1966. Pp. xvii, 166. $4.

EITEMAN, W. J., DICE, C. A. AND EITEMAN, D. K. The stock market. 4th ed. New York: McGraw-Hill, 1966. Pp. 570. $12.

FERRONATO, J. Why not get rich? New York: Vantage Press, 1966. Pp. 118. $3.50.

FOSTER, O. D. Ticker technique—the fascinating art of tape reading. Rev. and ed. by R. H. Persons, Jr. with D. Worden and H. Liesner. Palisades Park, N.J.: Investor's Press, 1965. Pp. 127.

FRIEDLAND, S. The economics of corporate finance. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xii, 418. $11.95.

ROBICHEK, A. A. AND MYERS, S. C. Optimal financing decisions, Englewood Cliffs, N.J.: Prentice-Hall, 1965. Pp. x, 166.

ROBINSON, R. I. Financing the dynamic small firm—problems of promotion, survival, and growth. Belmont, Calif.: Wadsworth, 1966. Pp. ix, 118.

ROBINSON, R. I. AND JOHNSON, R. W. Self-correcting problems in finance. Boston: Allyn and Bacon, 1965. Pp. 105. Paper, $2.95.

WEAVER, M. The technique of short selling: making money on declines in the stock market. Rev. ed. Palisades Park, N.J.: Investor's Press, 1963, Pp. xi, 81.

WILSON, S. J. The speculator and the stock market—a study of speculation, margin accounts, volume of transactions, and movements of the averages. Palisades Park, N.J.: Investor's Library, 1963. Pp. xi, 145. $5.95.

Business Organization; Managerial Economics; Marketing; Accounting

ANTON, H. R. AND FIRMAN, P. A., eds. Contemporary issues in cost accounting. Boston: Houghton Mifflin, 1965. Pp. xii, 530. Paper, $5.50.

BEDFORD, N. M. Income determination theory: an accounting framework. Reading, Mass.: Addison-Wesley, 1965. Pp. xiv, 241. $8.50.

CAREY, J. L. AND DOHERTY, W. O. Ethical standards of the accounting profession. New York: Am. Inst. of Certified Pub. Accountants, 1966. Pp. xv, 330. $5; $4, members.

DYER, F. C. AND DYER, J. M. Bureaucracy vs. creativity. Coral Gables: Univ. of Miami Press, 1965. Pp. 153. $5.75.

GARNER, P. AND BERG, K. B., eds. Readings in accounting theory. New York: Houghton Mifflin, 1966. Pp. xi, 520. Paper, $4.95.

GONOD, P., NANNINGA, H. G. C., AND VANNUTELLI, C. Problèmes et méthodes de mesure de la productivité dans les industries de la Communauté. Paris: Dalloz, 1965. Pp. 81. F 10.

HEUSS, E. Allgemeine Markttheorie. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. ix, 275. DM 35.

IJIRI, Y. Studies in mathematical and managerial economics. Vol. 3, Management goals and accounting for control. Chicago: Rand McNally; Amsterdam: North-Holland Pub. Co, 1965. Pp. xvii, 191.

JOHNSON, J. T. AND BRASSEAUX, J. H. Readings in auditing. 2nd ed. Cincinnati: South-Western, 1965. Pp. x, 710. $8.50.

KENDRICK, J. W. AND CREAMER, D. Measuring company productivity—handbook with case studies. Stud. in bus. econ. no. 89. New York: Nat. Indus. Conf. Board, 1965. Pp. 120. $2.50; $1 students and teachers; $12.50, non-associates.

LEVITT, T. Industrial purchasing behavior—a study of communications effects. Boston: Div. of Research, Grad. School of Bus. Admin., Harvard Univ., 1965. Pp. xv, 184. $4.

LORSCH, J. W. Product innovation and organization. New York: MacMillan; London: Collier-Macmillan, 1965. Pp. xiii, 184.

McFarland, D. E., ed. Current issues and emerging concepts in management—readings from the Academy of Management, vol. 2. Boston: Houghton Mifflin, 1966. Pp. xi, 513. $6.50; paper, $4.50.

Menrad, S. Der Kostenbegriff. Eine Untersuchung über den Gegenstand der Kostenrechnung. Berlin: Duncker & Humblot, 1965. Pp. 184. DM 24.

Mulvihill, D. F., ed. Guide to the quantitative age—readings from *Fortune*. New York: Holt, Rinehart and Winston, 1966. Pp. v, 233. Paper.

Pessemier, E. A. New-product decisions—an analytical approach. New York: McGraw-Hill, 1966. Pp. x, 214. $5.95.

Pollard, S. The genesis of modern management—a study of the Industrial Revolution in Great Britain. Cambridge: Harvard Univ. Press, 1965. Pp. 328. $7.95.

Rastogi, T. N. Indian manufactured goods at home and abroad. Calcutta: Orient Longmans, 1965. Pp. xxv, 500. Rs 25.

Ross, H. The elusive art of accounting—a brash commentary on financial statements. New York: Ronald, 1966. Pp. ix, 205. $5.

Sabel, H. Die Grundlagen der Wirtschaftlichkeitsrechnungen. Berlin: Duncker & Humblot, 1965. Pp. 231. DM 32.60.

Seuster, H. Der funktionelle Ablauf im landwirtschaftlichen Betrieb—umfang, vielfalt und steuerung. Hamburg: Parey, 1966. Pp. 160.

Still, R. R. and Cundiff, E. W. Essentials of marketing. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. 186.

Thompson, J. D., ed. Approaches to organizational design. [Pittsburgh]: Univ. of Pittsburgh Press, 1966. Pp. vi, 223. $3.95.

Vokuhl, P. Die Anwendung der linearen Programmierung in Industriebetrieben. Berlin: de Gruyter, 1965. Pp. 176. DM 28.

Developments in the structure of retail trade—likely effects of the abolition of resale price maintenance. Prepd. by Marketing Economics. London: Aims of Industry, [1965]. Pp. 18. 10s.

L'Entreprise ses techniques et son gouvernement. 3, La formation des décisions et l'entre prise. *Econ. Appliquée,* Jan.-June, 1965. Paris: Presses Univ. de France, 1965. Pp. 286.

Solving international pricing problems. New York: Bus. Internat., 1965. Pp. 50. Paper, $50.

Svenska handelshögskolan—arsberättelse 1964-1965. (Swedish School of Business annual report 1964-1965.) Helsinki: [Svenska Handelshögskolan], 1965. Pp. 68.

Industrial Organization; Government and Business; Industry Studies

Backe-Dietrich, B. Der Binnenhandel mit Konsumgütern in der Sowjetunion. Berlin: Duncker & Humblot, 1965. Pp. 220. DM 37.60.

Blaine, J.C.D., and Rushing, R. W. Transportation and spatial immobility in India. Research paper 16. Chapel Hill: Grad. School of Bus. Admin., Univ. of North Carolina, 1965. Pp. x, 59.

Bolton, R. E., ed. Defense and disarmament—the economics of transition. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. viii, 180. $4.95; paper, $1.95.

Burstein, M. L., and others. The cost of trucking: econometric analysis. Dubuque, Iowa: Wm. C. Brown, 1965. Pp. xix, 137. $4.50.

Bussman, L. Der Einfluss der Konzentration auf Preise und Gewinne. Der Einfluss des Konzentrationsgrades einer Branche auf die Preise und Gewinne im internationalen Vergleich. Cologne: Bund Verlag, 1965. Pp. 139. DM 27.

Chardonnet, J. Géographie industrielle. Vol. 2, L'Industrie. Paris: Ed. Sirey, 1965. Pp. 460. F 52.

Corbett, D. Politics and the airlines. Toronto: Univ. of Toronto Press, 1965. Pp. 350. $5.75.

FEL'D, S. D. Edinyi energeticheskii balans narodnogo choziaistva (problemy optimizacii). (Over-all balance of energy in national economy—problems of optimization.) Moscow: Ekonomika, 1964. Pp. 311.

FISCHLER, G. Energiewirtschaft in Israel. (With English summary.) Basel: Kyklos-Verlag; Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. vi, 110. Dm 17.80.

FUCHS, V. R. The growing importance of the service industries. NBER occas. paper 96. New York: Nat. Bur. Econ. Research; Columbia Univ. Press, distrib., 1965. Pp. 30.

GRUNEWALD, D. AND BASS, H. L. Public policy and the modern corporation—selected readings. New York: Appleton-Century-Crofts, 1966. Pp. ix, 380. Paper, $3.95.

HAEFELE, E. T. AND STEINBERG, E. B. Government controls on transport—an African case. Washington: Brookings Inst., 1965. Pp. x, 102.

HARDY, G. Monopoly and the individual. Carnegie, Australia: Reform Pub. Co., 1964. Pp. iv, 203. 25s.; paper, 9s. 6d.

HAWLEY, E. W. The New Deal and the problem of monopoly—a study in economic ambivalence. Princeton: Princeton Univ. Press, 1966. Pp. xv, 525. $10.

HUNTER, A. Competition and the law. Univ. of Glasgow soc. and econ. stud., n. s. no. 7. London: Allen & Unwin, 1966. Pp. 238. 45s.

KLATT, S. Die ökonomische Bedeutung der Qualität von Verkehrsleistungen. Berlin: Duncker & Humblot, 1965. Pp. 350. DM 44.

MACAVOY, P. W. The economic effects of regulation—the trunk line railroad cartels and the Interstate Commerce Commission before 1900. Cambridge: M.I.T. Press, 1965. Pp. ix, 275. $10.

POSTNIKOV, W. I. AND RAZUMOV, I. M. Atommaja energiia v narodnom choziaistve (Ekonomika i primeneniia). (Atomic energy in national economy—economics and utilization.) Moscow: Economika, 1964. Pp. 175.

RAPHAEL, J. S. Governmental regulation of business. New York: Free Press; London: Collier-Macmillan, 1966. Pp. x, 260. $6.95.

ROJAS, J. L. F. Tarifas, entradas y gastos de la Empresa de Ferrocarriles del Estado de Chile. Inst. Econ. pub. no. 76. Santiago: Inst. Econ., Univ. Chile, 1965. Pp. vii, 102.

SCHNEIDER, E.-D. Absatzpolitik pharmazeutischer Industrieunternehmungen. Grundfragen des Absatzes von Arzneispezialitäten auf den Inlandsmarkt. Berlin and New York: Springer, 1965. Pp. xvi, 169. DM 39.

SLITZ, T. Preisführerschaft im Oligopol. Cologne: Heymanns, 1965. DM 28.

SHERMAN, A. Price control by any other name. Stud. no. 7. London: Aims of Industry, [1965]. Pp. 15. 2s. 6d.

TEELING-SMITH, G., ed. Science, industry and the state. Office of Health Economics Symposium held at Royal Society of Health, Buckingham Palace Road, London. Oxford, Eng., and New York: Pergamon Press, 1965. Pp. xv, 100. $3.50.

Bulletin 1966-67. Washington: Patent, Trademark, and Copyright Research Inst., George Washington Univ., *s.d.* Pp. 93.

The Battle Act report, 1965. Mutual Defense Assistance Control Act of 1951. 18th report to Congress. Dept. of State pub. 8019. Washington: Supt. Docs., 1966. Pp. 124. 40c.

The enterprise and factors affecting its operation. Geneva: Internat. Lab. Off., 1965. Pp. x, 193. $2.50.

Identical bidding in public procurement. Fourth report of the Attorney General under Executive Order 10936. Washington: Supt. Docs., 1965. Pp. 237.

Land Economics; Agricultural Economics; Economic Geography; Housing

BOSERUP, E. The conditions of agricultural growth—the economics of agrarian change under population pressure. Chicago: Aldine, 1966. Pp. 124. $5.

BRAMLEY, M. Farming and food supplies—the case for expansion of British agriculture. London: Allen & Unwin, 1965. Pp. 131. 20s.

BURCKHARDT, K. U. AND ESCOBAR, R. L. Agricultura y tributacion—dos ensayos. Inst. Econ. pub. no. 75. Santiago: Inst. Econ. Univ. Chile, 1965. Pp. xiv, 188.

CHRISTY, F. T., JR. AND SCOTT, A. The common wealth in ocean fisheries—some problems of growth and economic allocation. Baltimore: Johns Hopkins Press, for Resources for the Future, 1965. Pp. xiii, 281. $6.

COURTENAY, P. P. Plantation agriculture. New York: Praeger, 1965. Pp. viii, 208. $6.

FLIEDNER, H. Die Bodenrechtsreform in Kenya—Studie über die Änderung der Bodenrechtsverhältnisse im Zuge der Agrarreform unter besonderer Berücksichtigung des Kikuyu-Stammesgebietes. Afrika-Stud. no. 7. New York and Berlin: Springer-Verlag, 1965. Pp. xiv, 114. DM 18.

GRIGG, D. B. The agricultural revolution in South Lincolnshire. Cambridge: Cambridge Univ. Press, 1966. Pp. 218.

HAMMER, M. Probleme der sizilianischen Agrarstruktur. Basel: Kyklos-Verlag; Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. vii, 63. DM 6.

JACOLIN, H. Les transports ferroviaires dans l'Europe orientale et danubienne. *Cahiers de L'I.S.E.A.*, no. 161, ser. G., no. 21. Paris: Inst. Sci. Econ. Appliquée, 1965. Pp. 174.

JAIN, S. C. Agricultural development of African nations. Vol. 1. Bombay: Vora, 1965. Pp. vii, 256. $6.

JÜRGENSEN, H. Produktivitätsorientierte Regionalpolitik als Wachstumsstrategie Hamburgs. Gutachten erstattet der Behörde für Wirtschaft und Verkehr d. Freien und Hansestadt Hamburg. Göttingen: Vandenhoeck & Ruprecht, 1965. Pp. 220. DM 35.

KUMAR, D. Land and caste in South India—agricultural labour in the Madras Presidency during the nineteenth century. New York: Cambridge Univ. Press, 1965. Pp. xii, 211. $8.50.

LEON, C. E. Leyes de reforma agraria en America Central. Bogotá: Inst. Interamericano de Ciencias Agric. de la OEA, Centro Interamericano de Reforma Agraria, 1965. Pp. 65.

LOEWENSTEIN, L. K. The location of residences and work places in urban areas. New York: Scarecrow Press, 1966. Pp. 331. $7.50.

LOVEJOY, W. F. AND HOMAN, P. T. Methods of estimating reserves of crude oil, natural gas, and natural gas liquids. Baltimore: Resources for the Future; Johns Hopkins Press, distrib., 1965. Pp. xvi, 163. Paper, $3.

MARGOLIS, J., ed. The public economy of urban communities. Papers presented at the second Conference on Urban Public Expenditures, February, 1964. . . Washington: Resources for the Future; Johns Hopkins, distrib., 1965. Pp. xvii, 264. Paper, $5.

MCCARTY, H. H. AND LINDBERG, J. B. A preface to economic geography. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. x, 261. Paper, $3.95.

MOOMAW, I. W. The challenge of hunger—a program for more effective foreign aid. New York and London: Praeger, 1966. Pp. viii, 222. $5.95.

MORGAN, R. J. Governing soil conservation: thirty years of the new decentralization. Baltimore: Johns Hopkins Press, for Resources for the Future, [1966]. Pp. xiv, 399. $8.

NANAVATI, M. B. AND ANJARIA, J. J. The Indian rural problem. 6th rev. ed. Bombay: Indian Soc. Agric. Econ., 1965. Pp. x, 595. $5.

O'CONNOR, A. M. Railways and development in Uganda: a study in economic geography. East African stud., 15. Nairobi: Oxford Univ. Press, for East African Inst. of Soc. Research, 1965. Pp. 176.

PATVARDHAN, V. S. British agricultural marketing—a study in government policy. New York: Asia Pub. House; Taplinger, distrib., 1966. Pp. xi, 158. $7.50.

PRESCOTT, J. R. V. The geography of frontiers and boundaries Chicago: Aldine, 1965. Pp. 190. $5.

PROST, M. A. La Hiérarchie des villes en fonction de leurs activités de commerce et de service Paris: Gauthier-Villar, 1965. Pp. 335. F 68.

RICHARDSON, S. D. Forestry in Communist China. Baltimore: Johns Hopkins Press, 1966. Pp. xvi, 237. $6.95.

ROY, H. Tea price stabilisation—the Indian case. Calcutta: World Press, 1965. Pp. viiii, 204. Rs 15.

SHIBER, S. G The Kuwait urbanization—documentation, analysis, critique. Kuwait: Kuwait Govt. Printing Press, 1964. Pp. lxxii, 643. $25.

SHOVER, J. L. Cornbelt rebellion—the Farmers' Holiday Association. Urbana: Univ. of Illinois Press, 1965. Pp. vi, 239. $5.95.

SHUKLA, T. Capital formation in Indian agriculture. Bombay: Vora, 1965. Pp. xiv, 261. $6.

STAMER, H. AND WOLFFRAM. R. Die Nachfrage nach Agrarprodukten. Elastizitäten und Entwicklungstendenzen. Hamburg: Parey, 1965. Pp. 140. DM 22.

TREGEAR, T. R. A geography of China. Chicago: Aldine, 1965. Pp. xvii, 342. $7.95.

TUMA, E. H. Twenty-six centuries of agrarian reform—a comparative analysis. Berkeley: Univ. of California Press, 1965. Pp. xi, 309. $6.95.

VENKATARAMANAN, L. S. The theory of futures trading—hedging, speculation and storage in organised commodity markets. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. xi, 115. $7.

WEBER, M. The city. Transl. and ed. by D. Martindale and G. Neuwirth. New York: Free Press, London: Collier-Macmillan, 1966. Pp. 242. Paper, $2.45.

WILKINSON, T. O. The urbanization of Japanese labor, 1868-1955. Amherst: Univ. of Massachusetts Press, 1965. Pp. xiii, 243. $6.

Annual report for the year ending September 30, 1965. Washington: Resources for the Future, 1965. Pp. 116.

Economic development of agriculture—the modernization of farming. Ames: Iowa State Univ. Press, 1965. Pp. viii, 314. $4.95.

Market towns and spatial development in India. New Delhi: Nat. Council Applied Econ. Research, 1965. Pp. xi, 162. $5.

1964 farm economy analysis report—from the results of 62 self-accounting farm records, in the Kinki District. Kyoto, Japan: Farm Accounting Inst., Faculty of Agric., Kyoto Univ., 1965. Pp. 49.

1964 farm economy analysis report—from the results of 225 simplified farm records, in the Kinki District. Kyoto, Japan: Farm Accounting Inst., Faculty of Agric., Kyoto Univ., 1965. Pp. 29.

Oxford economic atlas of the world. 3rd ed. New York: Oxford Univ. Press, 1965. Pp. viii, 286. $15.

Primer seminario nacional sobre reforma agraria para curas parrocos, Bogotá, Colombia 19-24 Octubre 1964. Bogotá: Inst. Interamericano de Ciencias Agric. de la OEA, Centro Interamericano de Reforma Agraria, 1964. Pp. 75.

World housing conditions and estimated housing requirements. U.N. Dept. of Econ. and Soc. Affairs, sales no. 65. IV. 8. New York: Sales Sec., U.N., 1965. Pp. 58.

Labor Economics

BELING, W. A. Modernization and African labor—a Tunisian case study. New York: Praeger, 1965. Pp. xv, 259. $12.50.

BERG, H. Ökonomische Grundlagen der Bildungsplanung. Berlin: Duncker & Humblot, 1965. Pp. 170. DM 24.80.

BOARMAN, P. M. Facts and fancies about automation. New York: Nat. Assn. of Manufacturers, *s.d.* 50c.

CHENG, C. Y. Scientific and engineering manpower in Communist China, 1949-1963. Nat. Sci. Foundation, 65-14. Washington: Supt. Docs., 1965. Pp. xx, 588. Paper, $2.

CHRISTENSON, C. L. AND MYREN, R. A. Wage policy under the Walsh-Healey Public Contracts Act—a critical review. Indiana Univ. soc. sci. ser. no. 24. Bloomington and London: Indiana Univ. Press, 1966. Pp. xiii, 239.

COLE, G. D. H. AND FILSON, A. W. British working class movements—select documents 1789-1875. New York: St. Martin's Press; London: Macmillan, 1965. Pp. xxii, 629. Paper, $5.
First published in 1951.

DUNLOP, J. T., ed. The theory of wage determination—proceedings of a conference held by the International Economic Association. New York: St. Martin's Press; London: Macmillan, 1964. Pp. xv, 437. $8.
First published in 1957. Reviewed in the March 1958 *Review* by G. H. Hildebrand.

FLEMING, R. W. The labor arbitration process. Urbana: Univ. of Illinois Press, 1965. Pp. 233. $5.

GADGIL, D. R. Women in the working force in India. London: Asia Pub. House; New York: Taplinger, distrib., 1965. Pp. 33. $1.75.

GAMBS, J. S. The decline of the IW.W. New York: Russell & Russell, 1966. [© 1932]. Pp. 268.

GOETZ-GIREY, R. Le mouvement des grèves en France. Paris: Ed. Sirey, 1965. Pp. 220. F 30.

HAYS, P. R. Labor arbitration—a dissenting view. New Haven: Yale Univ. Press, 1966. Pp. vii, 125. $4.50.

HENDERSON, J. P. Labor market institutions and wages in the lodging industry. East Lansing: Div. of Research, Bur. of Bus. and Econ. Research, Grad. School of Bus. Admin., Michigan State Univ., 1965. Pp. xv, 227. $6.50.

KOLAJA, J. Workers' councils—the Yugoslav experience. New York: Praeger, 1965. Pp. xii, 84. $4.50.

LANGER, H. C., JR. The fight for work. New York: Pageant Press, 1965. Pp. xi, 121. $3.

LESTER, R. A. Manpower planning in a free society. Princeton: Princeton Univ. Press, 1966. Pp. xiv, 227. $5.

MARSH, A. Industrial relations in engineering. New York: Pergamon Press, 1965. Pp. xxiii, 362. Paper, $5.

MCKEAN, E. C. The taxable wage base in unemployment insurance financing. Kalamazoo: Upjohn Inst. for Employment Research, 1965. Pp. ix, 96. Single copies free.

MORTON, J. E. Analytical potential of the current population survey for manpower and employment research. Kalamazoo: Upjohn Inst. for Employment Research, 1965. Pp. 71. Single copies free.

MURRAY, M. G. Proposed federal unemployment insurance amendments. Kalamazoo: Upjohn Inst. for Employment Research, 1966. Pp. v, 49. Single copies free.

PHELPS BROWN, E. H. The growth of British industrial relations—a study from the standpoint of 1906-14. New York: St. Martin's Press; London: Macmillan, 1965. Pp. xxxvii, 414. Paper, $4.

RANDLE, C. W. AND WORTMAN, M. S., JR. Collective bargaining—principles and practices. 2nd ed. Boston: Houghton Mifflin, 1966. Pp. x, 564. $8.75.

RAYBACK, J. G. A history of American labor—expanded and updated. New York: Free Press; London: Collier-Macmillan, 1966. Pp. x, 491. Paper, $3.50.

ROUTH, G. Occupation and pay in Great Britain, 1906-60. Nat. Inst. Econ. and Soc. Research, econ. and soc. stud. no. 24. New York and Cambridge: Cambridge Univ. Press, 1965. Pp. xi, 182. $6.50.

SINHA, M. R., ed. The economics of manpower planning. Bombay: Asian Stud. Press, for Indian Inst. of Asian Stud., 1965. Pp. xv, 194. $4.
Seven essays by M. Debeauvais, F. Paukert, J. Vaizey, R. G. Hollister, J. Timar, A. Hovne, and S. Okita.

TOURAINE, A. WITH DURAND, C., PECAUT, D. AND WILLENER, A. Workers' attitudes to technical change—an integrated survey of research. Paris: OECD; New York: McGraw-Hill, distrib., 1965. Pp. 177. $6.50.

VAN DER HORST, S. T. African workers in town—a study of labour in Cape Town. New York, Cape Town, and London: Oxford Univ. Press, 1964. Pp. ix, 140. $4.80.

WITNEY, F. Labor policy and practices in Spain—a study of employer-employee relations under the Franco regime. New York: Praeger, 1965. Pp. xii, 103. $12.50.

Impact of federal research and development policies on scientific and technical manpower. Hearings before the Senate Committee on Labor and Public Welfare, Subcommittee on Employment and Manpower, 89th Cong., 1st sess., June 2-10 and July 22, 1965. Washington: Supt. Docs., 1965. Pp. 954.

Labour faces the new age—purposes, structure and work of the I.L.O.: a workers' education manual. Washington and Geneva: Internat. Lab. Off., 1965. Pp. v, 227. 75c.

Ocupacion y desocupacion—Gran Santiago, Septiembre de 1965. Inst. Econ. pub. no. 77. Santiago: Inst. Econ. Univ. Chile, 1965. Pp. ix, 35.

Recursos humanos de la Administración Pública Chilena. Informe complementario: clasificación ocupacional. Pub. no. 24. Santiago: Inst. de Administración, 1965. Pp. ix, 79.

The requirements of automated jobs. North American Joint Conference, Washington, D.C., 8th-10th December 1964. Paris: OECD, 1965. Pp. 451. $5.50.

A twenty-year review. Kalamazoo: Upjohn Inst. for Employment Research, 1965. Pp. vii, 30

Population; Welfare Programs; Consumer Economics

ALTMEYER, A. J. The formative years of social security. Madison and London: Univ. of Wisconsin Press, 1966. Pp. xi, 314. $6.50.

ANKERL, G. L'epanouissement de l'homme dans les perspectives de la politique economique. Paris: Sirey; Cologne: Westdeutscher Verlag, 1965. Pp. xxvii, 238.

CARLTON, F. T. Economic influences upon educational progress in the United States, 1820-1850. Classics in educ. no. 27. New York: Teachers College Press, Columbia Univ., 1965. Pp. xvii, 165. $3.75; paper, $1.95.

CLARKE, J. I Population geography. Oxford and New York: Pergamon Press, 1965. Pp. x, 164. Paper, $4.50.

FAIDUTTI-RUDOLPH, A. M. L'Immigration italienne dans le Sud-Est de la France. 2 vols. Gap: Ed. Ophrys, 1964. Pp. 402; 227. F 65.

FOGEL, W. Education and income of Mexican-Americans in the Southwest. Advance rept. 1 Los Angeles: Mexican-American Stud. Project, Grad. School of Bus. Admin., Univ. of California, 1965. Pp. ix, 30. Paper, $1.

FRIEDLANDER, S L. Labor migration and economic growth—a case study of Puerto Rico. Cambridge: M.I.T. Press, 1965. Pp. ix, 181. $6.

GREBLER, L. with contributions by NEWMAN, P. M. AND WYSE, R. Mexican immigration to the United States. Mexican-American study project advance rept. 2. Los Angeles: Div. of Research, Grad. School of Bus. Admin., Univ. of California, 1965. Var. pp. Paper, $3.

LEVITAN, S. A. Programs in aid of the poor. Kalamazoo: Upjohn Inst. for Employment Research, 1965. Pp. vii, 59. 25c.

MEYER-DOHM, P. Sozialökonomische Aspekte der Konsumfreiheit. Untersuchungen zur Stellung des Konsumenten in der Marktwirtschaftlichen Ordnung. Freiburg im Breisgau: Rombach, 1965. Pp. 419. DM 29.

ROTHERMUND, I. Die politische und wirtschaftliche Rolle der asiatischen Minderheit in Ostafrika (Kenya, Tanganyika/Sansibar, Uganda). Afrika-Stud. no. 6. New York and Berlin: Springer-Verlag, 1965. Pp. 75. DM 11.20.

ROUCEK, J. S., ed. Programmed teaching: a symposium on automation in education. New York: Philos. Lib., 1965. Pp. viii, 194. $10.

SIMON, K. A. AND FULLAM, M. G. Projections of educational statistics to 1974-75 (1965 edition). Dept. of Health, Educ., and Welfare, Off. of Educ., circ. 790. Washington: Supt. Docs., 1965. Pp. v, 68. 45c.

STASSART, J. Les avantages et les inconvénients économiques d'une population stationnaire. Liège: Faculté de Droit; The Hague: Martinus Nijhoff, 1965. Pp. 256. Bfr. 250.

TURNBULL, J. G. WITH COHEN, M. S. AND PEPPLE, M. The changing faces of economic insecurity. Minneapolis: Univ. of Minnesota Press, 1966. Pp. ix, 157. $5.

WEISBROD, B. A., ed. The economics of poverty—an American paradox. Englewood Cliffs, N.J.: Prentice-Hall, 1965. Pp. x, 180. $4.95; paper, $1.95.

WRIGHT, P. Earned degrees conferred 1962-1693—bachelor's and higher degrees. Dept. of Health, Educ., and Welfare, circ. no. 777. Washington: Supt. Docs., 1965. Pp. viii, 280 $1.75.

Changes in mortality trends: England and Wales 1931-1961. A study of trends in the death rates in England and Wales analyzed by sex, age, and cause of death as part of a survey of trends in the United States and other countries. Pub. Health Svce. pub. no. 1000, ser. 3, no. 3. Washington: Supt. Docs., 1965. Pp. 49. 40c.

Demanda de bienes durables, Gran Santiago—junio 1964. Inst. Econ. pub. no. 79. Santiago: Inst. Econ. Univ. Chile, 1965. Var. pp.

Disability days, United States, July 1963-June 1964. Nat. Center for Health Stat., ser. 10, no. 24. Washington: Supt. Docs., 1965. Pp. 53. Paper, 40c.

Divorce statistics analysis—United States, 1962. Nat. Center for Health Stat., ser. 21, no 7. Washington: Supt. Docs., 1965. Pp. 56. Paper, 40c.

Economic policies and practices. Paper no. 7, European social security systems. A comparative analysis of programs in England, Sweden and the Common Market countries, together with a description of the U.S. system. Materials prepared for the Joint Economic Committee, 89th Cong., 1st sess. Washington: Supt. Docs., 1965. Pp. 129.

Health insurance and related provisions of Public Law 89-97, the Social Security amendments of 1965. Senate Special Committee on Aging, 89th Cong., 1st sess., Committee print. Washington: Supt. Docs., 1965. Pp. 68.

Report of the Asian Population Conference and selected papers, New Delhi, India, December 10-20, 1963. New York: Sales Sec., U.N. 1964. Pp. 207.

Report of the Commission on the Cost of Medical Care. Vol. 1. Chicago: American Medical Assn., 1964. Pp. 182. Single copies free.

Statistical summary of state school systems, 1963-64. Dept. of Health, Educ., and Welfare, Office of Educ., circ. no. 789. Washington: Supt. Docs., 1965. Pp. 25. 20c.

Related Disciplines

ADLER, S. The uncertain giant: 1921-1941—American foreign policy between the wars New York: Macmillan; London: Collier-Macmillan, 1966. Pp. xi, 340. $6.95.

AUBERT, V. The hidden society. Totowa, N.J.: Bedminster Press, 1965. Pp. 351. $7.50.

BEATON, L. Must the bomb spread? Baltimore Md. and Middlesex, Eng.: Penguin Books, 1966. Pp. 147. Paper, 95c.

BENTON, W. WITH SILVERT, K. The voice of Latin America. Rev. ed. New York: Harper & Row, 1965. Pp. xx, 202. Paper, $1.60.

BORNET, V. D. Labor politics in a democratic republic—moderation, division, and disruption in the Presidential Election of 1928. Washington: Spartan Books; London: Cleaver-Hume Press, 1964. Pp. xiii, 376. $6.95.

BRAND, W. Impressions of Haiti. Inst. of Soc. Stud., ser. minor vol. 8. The Hague: Mouton, 1965. Pp. 77. Paper, ƒ 8.

BUCK, P., ed. Social sciences at Harvard 1860-1920—from inculcation to the open mind. Cambridge: Harvard Univ. Press, 1965. Pp. xiv, 320. $6.95.
Contributions by R. L. Church, D. B. Potts, C. F. Baird, S. M. Stern, and A. G. Powell.

DAVINSON, D. Commercial information—a source handbook. New York: Pergamon Press, 1965. Pp. vii, 164. $3.95.

FREMONT-SMITH, M. R. Foundations and government—state and federal law and supervision. New York: Russel Sage Foundation, 1965. Pp. 564. $8.50.

HANSER, C. J. Guide to decision: the Royal Commission. Totowa, N.J.: Bedminster Press, 1965. Pp. xiii, 274. $6.50.

HERSKOWITZ, H. AND MARLIN, B. A guide to reading in American history: the unit approach. New York: New Am. Library, Signet Book, 1966. Pp. 236. Paper, 60c.

HUNT, E. F. Social science: an introduction to the study of society. 3rd ed. New York: Macmillan; London: Collier-Macmillan, 1966. Pp. xvii, 876. $8.96.

KERSTIENS, T. The new elite in Asia and Africa—a comparative study of Indonesia and Ghana. New York: Praeger, 1966. Pp. vi, 281. $12.50.

KÜLP, B. Theorie der Drohung. Cologne: Kohlhammer, 1965. Pp. 106. DM 12.60.

LUBELL, S. The future of American politics. 3rd ed. rev. New York: Harper & Row, 1965. Pp. xiii, 270. Paper, $1.75.

MATALON, B. *L'Analyse hiérarchique*. Paris: Gauthier-Villars, *s.d.* Pp. 150. F 25.

MAZLISH, B., ed. The railroad and the space program—an exploration in historical analogy. Cambridge: M.I.T. Press, 1965. Pp. xix, 223. $7.50.

NEEDLER, M. C. Understanding foreign policy New York: Holt, Rinehart and Winston, 1966. Pp. viii, 280. $4.95.

RANNEY, A. The governing of men. Rev. ed. New York: Holt, Rinehart and Winston, 1966. Pp. x, 678. $8.95.

ROSS, A. The United Nations: peace and progress: Totowa, N.J.: Bedminster Press, 1966. Pp. xi, 443. $8.

RUTLAND, R. A. The ordeal of the Constitution—the Antifederalists and the ratification struggle of 1787-1788. Norman: Univ. of Oklahoma Press, 1966. Pp. xiii, 329. $6.95.

SAMPSON, A. Anatomy of Britain today. New York: Harper & Row, 1965. Pp. xiv, 720. Paper, $2.95.

SIMON, H. A. Administrative behavior—a study of decision-making process in administrative organization. 2nd ed. New York: Free Press; London: Collier-Macmillan, 1965. Pp. xlviii, 259. Paper, $2.45.

SIMON, R., ed. Perspectives in public relations. Norman: Univ. of Oklahoma Press, 1966. Pp. xi, 353. $6.95.

SINGLETON, F. B. Background to Eastern Europe. New York: Pergamon Press, 1965. Pp. vii, 226 Paper, $3.75.

SNOW, P. G. Argentine radicalism—the history and doctrine of the Radical Civil Union. Iowa City: Univ. of Iowa Press, 1965. Pp. ix, 137. $3.75; paper, $2.75.

TAVENEAUX, R. Jansénisme et politique. Paris: Lib. A. Colin, 1965. Pp. 258. Paper, F 11.50.

VEBLEN, T. with intro. by DORFMAN, J. Imperial Germany and the Industrial Revolution. Ann Arbor: Univ. of Michigan Press, 1966. Pp. xxi, 341. Paper, $2.25.

WANG, Y. C. Chinese intellectuals and the West, 1872-1949. Chapel Hill: Univ. of North Carolina Press, 1966. Pp. xiv, 557. $10.

WHALEN, R. J. The founding father—the story of Joseph P. Kennedy. New York: New Am. Library, Signet Book, 1966. Pp. xii, 532. Paper, 95c.

Printed catalog of current journals, January 1966. [Cambridge]: Baker Library, Grad. School of Bus. Admin., Harvard Univ., 1966. p. 64. $5.

Social Science Research Council annual report 1964-1965. New York: [Soc. Sci. Research Coun., 1965]. Pp. 93.

Subject list of current journals, January 1966. [Cambridge]: Baker Library, Grad. School of Bus. Admin., Harvard Univ., 1966. Pp. 72. $5.

NOTES

The Nominating Committee of the Association announces the following list of candidates for office:

President Elect:

Kenneth E. Boulding, University of Michigan

Vice-Presidents:

William J. Baumol, Princeton University
Gerhard Colm, National Planning Association
John T. Dunlop, Harvard University
Robert Triffin, Yale University

Executive Committee:

Kenneth J. Arrow, Stanford University
Richard A. Easterlin, University of Pennsylvania
Franco Modigliani, Massachusetts Institute of Technology
Joseph A. Pechman, The Brookings Institution

Ralph L. Andreano, University of Wisconsin, was named as the Association's representative to the ACLS. Albert Rees, University of Chicago, was named as a representative of the Association on the Board of Directors of the Social Science Research Council.

The membership of the Nominating Committee was: Marvin Frankel (University of Illinois); Kermit Gordon (The Brookings Institution); Bernard F. Haley (University of California, Santa Cruz), chairman; Juanita M. Kreps (Duke University); Stanley Lebergott (Wesleyan University); and David W. Slater (Queen's University).

Visiting Scientist Program in Economics

The American Economic Association has received a grant of $5,750 from the National Science Foundation to establish a "Visiting Scientist Program in Economics." The purpose of this program, which parallels those previously established in mathematics and the physical sciences, is to stimulate improved teaching and interest in modern economics at colleges and universities whose major focus is at the undergraduate level.

The NSF grant provides financing for visits to such campuses by distinguished economists, who normally will spend a day or a day and a half on such a visit. The program for each visit is planned jointly between the visitor and the host institution; customarily it includes a talk to undergraduate major students, perhaps an informal seminar with faculty and students, discussion of current course and research interests of the host faculty, and other activities as may be desirable. While the ultimate concern is to encourage greater interest in, and better teaching of, economics at the undergraduate level, visitors' talks may be on research, current developments in economics, policy issues, or other topics that may seem appropriate to the needs of the particular institution.

This program will be under the general direction of the Association's Committee on Economic Education. Professor Phillip Saunders, department of economics, Carnegie Institute of Technology, will be administrator of the program, under the policies established by the Committee. Professor Saunders will have an informal roster of economists around the country who may be available for such visits, depending on their own schedules and the degree of common interest between them and the inviting institutions. Institutions are free to suggest visitors they would especially like to have, so long as these are within a radius of 100-200 miles of the campus, in order to minimize traveling costs. The host institution is expected to provide lodging and meals for the visitor when he is on the campus; other costs will be covered by the NSF grant.

The program is a small, experimental one for 1966-67, so it will be possible to accept only a limited number of requests during the coming year. If the program proves to be valuable, the Committee on Economic Education hopes to expand it in the following years.

Any institution which wants further information on this program, or which wishes to submit a request for a "visiting scientist" during 1966-67, is invited to write directly to Professor Saunders at Carnegie Institute of Technology, Pittsburgh, Pennsylvania 15213.

National Institute of Social and Behavioral Science

The National Institute of Social and Behavioral Science, in collaboration with the Section on Social and Economic Sciences of the American Association for the Advancement of Science, will hold its regular sessions for contributed papers at the annual meeting of the AAAS in Washington, D.C., December 26-31, 1966.

Association members interested in presenting a paper at these sessions are invited to forward titles and abstracts of some 300 words not later than September 1st to Donald P. Ray, Director, National Institute of Social and Behavioral Science, 863 Benjamin Franklin Station, Washington, D.C. 20044.

Papers should be based upon the current research of the author. Subjects may relate to any aspect of national or international economics; multidisciplinary studies are also of interest. Selected materials of the sessions of the National Institute will be published in its *Symposia Studies Series*. The annual meeting of the AAAS is the largest professional meeting held, embracing all fields of science. The American Economic Association regularly holds symposia sessions at AAAS meetings as an affiliate member of the Section on Social and Economic Sciences. The contributed papers program of the National Institute is supplementary to these sessions.

AER Manuscripts

The following manuscripts, exclusive of comments and replies and in addition to those listed in the June 1965 and December 1965 issues, have been accepted for publication in subsequent issues of the *American Economic Review:*

Robert Baldwin, "The Role of Capital-Goods Trade in the Theory of International Trade"

J. O. Blackburn, "Progressive Taxes, Growth, and Proportional Revenue"

J. S. Coleman, "The Possibility of a Social Welfare Function"

E. D. Domar, "On Collective Farms and Producer Co-operatives"

R. A. Easterlin, "Economic-Demographic Interactions and Long Swings in Economic Growth"

H. G. Grubel and A. D. Scott, "The Role of Foreigners in the Economic Profession"

Edwin Kuh, "The Measurement of Potential Output"

R. J. Larner, "Ownership and Control in the 200 Largest Nonfinancial Corporations, 1929 and 1963"

M. F. Long, "Foreign Graduate Students in Economics"

M. C. Lovell and William Brainard, "Some Simple Propositions Concerning Cost-Push Inflation"

Thomas Mayer, "The Propensity to Consume Permanent Income"

Nobuo Minabe, "The Heckscher-Ohlin Theorem, the Leontief Paradox, and a Neoclassical Model of Economic Growth"

P. A. Neher, "An Implication of the Labor-Surplus Assumption"

R. R. Nelson, "Full-Employment Policy and Economic Growth"

Tibor Scitovsky, "A New Approach to International Liquidity"

Jaroslav Vanek, "A Theory of Growth with Technological Change"

F. H. Weymar, "The Supply of Storage Revisited"

O. E. Williamson, "Peak-Load Pricing and Optimal Capacity under Indivisibility Constraints"

Announcements

The *Development Digest*, a quarterly journal prepared for the Agency of International Development by the National Planning Association, is now available to the public. The journal consists primarily of summaries and reprints of and excerpts from articles, books, monographs, and other materials dealing with social and economic development.

Those interested in subscribing should write to Superintendent of Documents, Government Printing Office, Washington, D.C. 20402. U.S. subscription is $2.50 per year; foreign subscription, $3.00 per year; and single copies are available at $.65 each.

Commencing with this issue of the *AER*, the *Periodicals* listings will appear in *The Journal of Economic Abstracts*. This *Journal* will be distributed to all members of the American Economic Association.

Deaths

Thomas C. Adams, November 7, 1965.

William H. Brown, Jr., associate professor of economics, Swarthmore College, January 16, 1966.

Stephen Cook, Detroit, Michigan.

John L. Dillinger, January 14, 1966.

H. LaRue Frain, emeritus professor of economics, Wharton School, University of Pennsylvania, October 4, 1965.

Harry G. Friedman, New York, November 21, 1965.

Branko Majstorovich, government statistician and economist, Illinois Bureau of Employment Security, March 9, 1966.

Constatine E. McGuire, Washington, D.C., October 22, 1965.

Herbert von Beckerath, emeritus professor, Duke University, in Washington, D.C., March 11, 1966.

Albert Conser Whitaker, professor emeritus of economics, Stanford University, October 14, 1965.

Retirements

James W. Angell, professor of economics, Columbia University, June 1966.

Ralph Conwell, professor emeritus, University of Wyoming, July 1966.

Walter L. Foster, assistant professor, department of economics, The City University of New York, August 1966.

W. A. Guinn, professor of insurance, College of Business Administration, University of Arkansas.

Louis M. Hacker, professor of economics, Columbia University, terminal leave, June 1966.

William M. Hench, professor of economics, The Pennsylvania State University.

Eastin Nelson, professor, department of economics, University of Texas, January, 1966.

Visiting Foreign Scholars

Jagdish Bhagwati, Delhi School of Economics, Delhi University: visiting professor of economics, Columbia University, 1966-67.

Kurt Martin, University of Manchester: visiting professor of economics, Princeton University, academic year 1966-67.

Promotions

Gerald O. Bierwag: associate professor of economics, University of Oregon.

David Burger: associate professor, department of political science and economics, State University College, Oswego, New York.

John F. Chant: associate professor of economics, Queen's University, Ontario, September 1966.

Edward L. Claiborn: associate professor of economics, U.S. Air Force Academy.

Paul A. David: associate professor of economics, Stanford University, fall 1966.

Donald J. Dewey: professor of economics, Columbia University.

Alexander Erlich: professor of economics, Columbia University.

Irwin Feller: assistant professor of economics, The Pennsylvania State University.

Virginia Galbraith: professor of economics, Mount Holyoke College, July 1966.

Francis W. Gathof: associate professor of economics and business, Beloit College.

Bruce Glassburner: professor, department of economics, University of California, Davis, July 1966.

Myron A. Grove: associate professor of economics, University of Oregon.

Raymond W. Hooker: associate professor of economics, University of Wyoming.

John J. Klein: professor, department of economics, Fordham University.

Howard D. Marshall: professor of economics, Vassar College, July 1966.

Thomas R. Masterson: professor of business administration, School of Business Administration, Emory University.

Ronald I. McKinnon: associate professor of economics, Stanford University, fall 1966.

Constantine Michalopoulos: assistant professor of economics, Trinity College.

Sarah S. Montgomery: associate professor of economics, Mount Holyoke College, July 1966.

Harold J. Noah: associate professor of economics of education, Teachers College, Columbia, University.

Edward B. Oppermann: associate professor of economics, U.S. Air Force Academy.

Attiat F. Ott: associate professor, department of economics, Southern Methodist University

Robert Piron: assistant professor of economics, Oberlin College.

Jan S. Prybyla: professor of economics, The Pennsylvania State University.

Stephen E. Schoderbek: associate professor of economics, U.S. Air Force Academy.

William O. Shropshire: associate professor, department of economics, Emory University.

Barry N. Siegel: professor of economics, University of Oregon.

David C. Smith: professor of economics, Queen's University, Ontario, September 1966.

Robert G. Taylor: permanent professor, U.S. Air Force Academy.

Jaroslav Vanek: professor of economics, Cornell University.

Roger C. Van Tassel: professor, department of economics, Clark University.

Administrative Appointments

Mario S. Brodersohn: director, Instituto Torcuato Di Tella, Centro de Investigaciones Económicas, Buenos Aires.

Lester V. Chandler: chairman, department of economics, Princeton University.

Barend A. de Vries: deputy-director, economics department, World Bank Group, International Bank for Reconstruction and Development.

Melvin L. Greenhut: professor and head, department of economics, Texas A. and M University.

James H. Hugon: associate director, 1966-67, director, 1967-68, International Business Studies Program, Italian Studies Center, Oregon State System of Higher Education, University of Pavia, Italy.

Andrew M. Kamarck; director, economics department, World Bank Group, International Bank for Reconstruction and Development.

Harold H. Kastner, Jr., Florida State Department of Education: dean, Polk Junior College, Winter Haven, Florida.

Charles A. Lininger: head, Michigan Sample Survey Project for socio-economic data in Peru, with USAID and the Government of Peru, until June 1968.

Thomas D. Mara: director of management programs, Emory University, fall 1965.

Albert G. Matamoros: chief economist, Armstrong Cork Company, Lancaster, Pennsylvania.

Herman L. Myers, Department of State, Mexico: chief, Regional Integration and Trade Division, Department of State, Washington.

Robert Ortner: vice president, Bank of New York.

Leonard S. Silk: vice chairman, Board of Editors, *Business Week*.

William P. Snavely: head, department of economics, University of Connecticut.

Harold K. Strom: director, Transport and Logistics Research Center, University of Oregon, January 1966.

Robert E. Strotz: dean, College of Arts and Sciences, Northwestern University, September 1966.

Guy W. Trump, Emory University: educational director, American Institute of CPAs, New York, September 1965.

Roger C. Van Tassel: acting chairman, department of economics, Clark University.

C. Edward Weber, University of Pittsburgh: professor and dean, School of Business Administration, University of Wisconsin, fall 1966.

Murray L. Weidenbaum: chairman, department of economics, Washington University.

W. Tate Whitman: acting chairman, department of economics, School of Business Administration, Emory University, fall 1965.

Appointments

A. L. Keith Acheson: assistant professor of economics, Queen's University, Ontario, September 1966.

Roger E. Alcaly: instructor in economics, Columbia University, fall 1967.

William M. Bailey: instructor, The American University, Washington.

Leo I. Bakony, Southern Methodist University: Economic Council of Canada, Ottawa, Ontario.

Pranab K. Bardhan: assistant professor of economics, Massachusetts Institute of Technology.

David Barkin: assistant professor of economics and education, Washington University.

Ernest J. Bartell: assistant professor, department of economics, University of Notre Dame.

Ralph Beals: associate professor of economics, Amherst College.

Theodore C. Bergstrom: assistant professor, department of economics, Washington University.

Stanley W. Black, III, Council of Economic Advisers: assistant professor of economics and Bicentennial Preceptor, Princeton University.

Anthony J. Blackburn, Massachusetts Institute of Technology: Mathematica staff.

David F. Bradford: assistant professor of economics, Princeton University.

David A. Bowers, Southern Methodist University: assistant professor, Western Reserve University, September 1965.

William Branson: assistant professor of economics, Princeton University.

Keith Brown, Southern Methodist University: assistant professor of economics and industrial management, Purdue University, September 1966.

Philip Cagan, National Bureau of Economic Research: professor, department of economics, Columbia University, fall 1967.

William F. Campbell, University of Virginia: assistant professor of economics, Louisiana State University, September 1966.

Robert Carlsson, Rutgers University: assistant professor of economics, College of Business Administration, University of South Carolina, September 1966.

James A. Chalmers: assistant professor of economics, Amherst College.

Kang Chao, University of California, Berkeley: assistant professor, department of economics, University of Wisconsin.

Nai-Ruenn Chen, University of California, Berkeley: assistant professor of economics, Cornell University.

Alpha C. Chiang, University of Connecticut: visiting professor of economics, Cornell University.

S. Y. Cho: associate professor of economics, Butler University, September 1966.

Robert M. Coen, Brookings Institution: assistant professor of economics, Stanford University, fall 1965.

John Conlisk, Rice University: assistant professor, department of economics, University of Wisconsin, fall 1966.

William J Craddock: research associate, department of economics, Iowa State University.

Robert W. Crandall: assistant professor of economics, Massachusetts Institute of Technology

James R. Crotty, Carnegie Institute of Technology: assistant professor, department of economics, State University of New York, Buffalo.

William I. Davisson: associate professor, department of economics, University of Notre Dame.

Floyd R. Dill: assistant professor of economics, Cornell University.

Dennis J. Dugan: assistant professor, department of economics, University of Notre Dame.

A. Thomas Eapen, Federal Reserve Bank of New York: assistant professor of economics, State University of New York, Binghamton and Harpur College.

P Gail Ellis: assistant professor: The American University, Washington.

Robert Evans: associate professor, department of economics, Brandeis University.

Heywood Fleisig, Yale University: assistant professor of economics, Cornell University.

Duncan K. Foley: assistant professor of economics, Massachusetts Institute of Technology.

Hugh W. Folk: associate professor, department of economics, and associate professor, Institute of Labor and Industrial Relations, University of Illinois, fall 1966.

James A. Gentry, Jr.: assistant professor of business administration, School of Business Administration, Emory University, fall 1965.

Moheb A. Ghali, University of Washington: assistant professor of economics, Rice University, September 1966.

Alan L. Ginsburg: assistant professor, department of economics, The City University of New York.

Howard M. Gitelman, Brandeis University: associate professor, department of economics, College of William and Mary, fall 1966.

Robert Gronau: instructor in economics, Columbia University, fall 1967.

Earl J. Hamilton, University of Chicago: distinguished professor of economic history, department of economics, State University of New York, Binghamton and Harpur College.

Edward H. Hanis: assistant professor of economics, Bowdoin College.

J. William Hanlon: assistant professor of economics, DePauw University.

John R. Harris: assistant professor of economics, Massachusetts Institute of Technology.

Paul T. Hartman: assistant professor, department of economics, University of Illinois, fall 1966.

Paul Hayashi, Southern Methodist University: assistant professor, Arlington State University, September 1965.

John D. Heinberg: assistant professor of economics and public affairs, Princeton University.

A. James Heins: associate professor, department of economics, University of Illinois, fall 1966.

Carl F. Hertz: visiting lecturer, department of economics, Iowa State University, March-May, 1966.

Walter P. Hettich: assistant professor of economics, Queen's University, Ontario, September 1966.

Paul Heyne: associate professor, department of economics, Southern Methodist University, July 1966.

Bert G. Hickman, Brookings Institution: professor of economics, Stanford University, fall 1966.

Toussaint Hočevar, Keuka College: assistant professor of economics, Florida State University.

Dorothy J. Hodges: assistant professor, department of economics, University of Wisconsin.

Richard T. Holmes, University of British Columbia: associate professor, department of economics and commerce, Simon Fraser University, July 1966.

John W. Hooper, Yale University: professor of economics, University of California, San Diego, July 1966.

Melvin E. Horton, University of Washington: instructor in economics, Occidental College.

Joseph C. Horvath, Department of the Interior, Denver: senior economist, Midwest Research Institute, Kansas City, Missouri.

Sheng-tieh Hsiao: visiting assistant professor of economics, University of Colorado, academic year 1966-67.

David S. Huang: associate professor, department of economics, Southern Methodist University, August 1965.

William R. Hughes, Wesleyan University: associate professor of economics, Boston College.

Warren S. Hunsberger: professor of economics, The American University, Washington.

Shane J. Hunt, Yale: associate professor of economics and international affairs and associate director, Graduate Program, Woodrow Wilson School, Princeton University.

Philip Jagolinzer: assistant professor of accounting, University of Maine, Portland.

Estelle D. James, University of California, Berkeley: assistant professor of economics, Stanford University, fall 1965.

David R. Kamerschen, Washington University: associate professor of economics, University of Missouri, September 1966.

K. William Kapp, Brooklyn College, The City University of New York: University of Basel, Switzerland.

David E. Kaun, University of Pittsburgh: associate professor of economics, University of California, Santa Cruz.

Harry H. Kelegian: assistant professor of economics, Princeton University.

Mahmood H. Khan, State Agricultural University, Wageningen, Netherlands: assistant professor, department of economics and commerce, Simon Fraser University, April 1966.

Chulsoon Khang: assistant professor of economics, University of Oregon.

S. D. Khosla: assistant professor of economics, Queen's University, Ontario, September 1966.

Sungwoo Kim: assistant professor, department of economics, State University of New York, Buffalo.

Glenn Kirby, Jr., Southern Methodist University: instructor, University of Omaha, September 1966.

Alvin K. Klevorick: assistant professor of economics, Princeton University.

Ramon Knauerhase: assistant professor of economics, University of Connecticut.

Serge-Christophe Kolm, Harvard University: associate professor of economics, Stanford University, fall 1966.

Walter Krause, University of Iowa: team leader, Survey Mission, Inter-American Development Bank.

Robert O. Krueger, Illinois Teachers College: assistant professor of business administration, University of Pittsburgh, fall 1966.

Mordecai Kurz, Hebrew University: visiting associate professor of economics, Stanford University, fall 1966.

Kelvin Lancaster, Johns Hopkins University: professor, department of economics, Columbia University, fall 1967.

William M. Landes, Columbia University: assistant professor of economics, Stanford University, fall 1965.

Anthony M. Lanyi: assistant professor of economics, Princeton University.

Laurence Lau: acting assistant professor of economics, Stanford University, fall 1966.

Roger C. Lawrence, Federal Reserve Bank of New York: instructor in economics, Columbia University, fall 1967.

William H. Leahy: assistant professor, department of economics, University of Notre Dame.

Louis Lefeber: professor of economics, Brandeis University, fall 1966.

John B. Legler: assistant professor, department of economics, Washington University.

Walter J. Lesiuk: data processing representative, Douglas Aircraft Company, Cape Kennedy, Florida.

James M. Litvack, Massachusetts Institute of Technology: assistant professor of economics, Princeton University.

C. A. Knox Lovell: assistant professor of economics, State University of New York, Binghamton and Harpur College.

David Major: assistant professor, department of economics, The City University of New York.

Michael McCracken, Southern Methodist University: Economic Council of Canada, Ottawa, Ontario, September 1965.

William F. McFarland: assistant professor, department of economics, Texas A. and M. University.

George W. McKenzie: assistant professor, department of economics, Washington University.

John N. McKinney, California State College, Los Angeles: assistant professor department of economics and commerce, Simon Fraser University, summer 1966.

Donald McLeod: assistant professor of economics, Temple University, February 1966.

Joseph Mire: professor of research in economics, The American University, Washington.

John J. Moroney, Florida State University: associate professor of economics, Michigan State University, September 1966.

M. Eugene Moyer: assistant professor, department of economics, University of Illinois, fall 1966.

Thomas Muench, Office of Defense Analysis: assistant professor of economics, University of Minnesota, September 1966.

Richard F. Muth: professor, department of economics, Washington University.

Peter K. Newman: professor of political economy, Johns Hopkins University, July 1966.

Donald A. Nichols: assistant professor, department of economics, University of Wisconsin, fall 1966.

David H. Nissen, University of California: assistant professor of economics, Rice University, September 1966.

Hugh S. Norton, University of Tennessee: professor of economics, College of Business Administration, University of South Carolina, September 1966.

Kenji Okuda, Government of Nepal: associate professor, department of economics and commerce, Simon Fraser University, fall 1966.

J. O. Parr, III, Vanderbilt University: operations research consultant, Ernst & Ernst, Cleveland.

Eric Pedersen, University of Stockholm: assistant professor of economics, Florida State University.

Richard L. Peterson, University of Michigan: assistant professor, department of economics, Southern Methodist University, September 1966.

James L. Pierce, Yale University: banking section, Division of Research and Statistics, Board of Governors of the Federal Reserve System.

Michael J. Piore: assistant professor of economics, Massachusetts Institute of Technology.

Jack Pounders: assistant professor, University of Southern California, September 1965.

John P. Powelson: professor of economics, University of Colorado, September 1966.

David Ramsey: assistant professor of economics, University of Missouri, September 1966.

James B. Ramsey, University of Wisconsin: assistant professor of economics, Michigan State University, September 1966.

William S. Rawson, Duke University: assistant professor of economics, College of Business Administration, University of South Carolina, September 1966.

David H. Richardson: assistant professor, department of economics, University of Kansas, fall 1966.

Alejandro Rofman: professor of social accounting, School of Economics, University of the Litoral, Argentina.

Robert Rogow, Michigan State University: assistant professor, department of economics and commerce, Simon Fraser University, summer 1966.

Mark L. Rose: Mathematica staff.

James N. Rosse, University of Minnesota: assistant professor of economics, Stanford University, fall 1965.

Beverly K. Schaffer: assistant professor of economics, School of Business Administration, Emory University, fall 1966.

Lawrence H. Seltzer: visiting distinguished professor of economics, Swarthmore College, first semester, 1966-67.

Stephen L. Shapiro: instructor, department of economics, Old Dominion College, fall 1966.

Mordechai Shechter: research associate, department of economics, Iowa State University.

Miguel Sidranski: assistant professor of economics, Massachusetts Institute of Technology.

Som Liem: assistant professor of economics, Amherst College.

William F. Staats, Rice University: Federal Reserve Bank of Philadelphia.

Richard E. Stanley, Jacksonville University: associate professor of marketing, College of Business Administration, University of South Carolina, July 1966.

James L. Stern: professor, department of economics, University of Wisconsin.

Joseph E. Stiglitz: assistant professor of economics, Massachusetts Institute of Technology.

Koji Taira, International Labor Organization, Geneva: associate professor of economics, Stanford University, winter 1965-66.

Samuel H. Talley: assistant professor of business and economics, University of Maine.

Morris Teubal: assistant professor of economics, University of Minnesota, September 1966

Adrian W. Throop: assistant professor of economics, Amherst College.

Harry M. Trebing: professor of economics and director, Institute of Public Utilities, Michigan State University, July 1966.

George M. von Furstenberg, Brookings Institution: assistant professor of economics, Cornell University.

Roger W. Walker: visiting lecturer, department of economics, Iowa State University, March-May, 1966.

A. Peter Walshe: assistant professor, department of economics, University of Notre Dame.

Stanley L. Warner, Jr.: assistant professor of economics, University of California, Santa Cruz

R. L. Weissman: visiting professor of economics, Niagara University, New York.

Finis Welch, University of Chicago: associate professor, department of economics, Southern Methodist University, September 1966.

Terry Wilford: A.I.D. program economist, Latin America, November 1965.

James Willis: assistant professor, Arlington State University, September 1965.

Alan R. Winger: assistant professor, department of economics, College of Business and Economics, University of Kentucky.

Melvyn L. Woodward, Kent State University: associate professor of management, College of Business Administration, University of South Carolina, September 1966.

Patrick Yeung: instructor, department of economics, University of Illinois, fall 1966.

Arnold Zellner, University of Wisconsin: professor, department of economics, University of Chicago.

Robert Zevin, University of California, Berkeley: instructor in economics, Columbia University, fall 1967.

Leaves for Special Appointments

Patrick M. Boarman, Bucknell University: visiting professor, Graduate Institute of International Studies, Geneva, Switzerland, 1965-66.

Alfred Carlip, State University of New York, Binghamton and Harpur College: Fulbright lecturer, National Taiwan University, 1966-67.

Carl F. Christ, Johns Hopkins University: Keynes visiting professor of economics, University of Essex, 1966-67.

Arthur T. Dietz, Emory University: Institut pour l'étude des Methodes de Direction de l'enterprise (IMEDE), University of Lausanne, Switzerland, academic year 1965-66.

Douglas F. Dowd, Cornell University: Fulbright professor of economics, Johns Hopkins University Center for International Studies, Bologna, Italy.

Edgar O. Edwards, Rice University: program specialist, Ford Foundation, Ministry of Economic Planning and Development, Government of Kenya, academic years 1966-68.

John Hein, Federal Reserve Bank of New York: staff, Economic Research Institute, Dublin, Ireland, May-October 1966.

Harley Hinrichs, University of Maryland: visiting assistant professor, department of economics, University of Wisconsin, first semester 1966-67.

Joseph S. La Cascia, University of Omaha: visiting Fulbright lecturer, University of the Northeast, Argentina, July-November, 1966.

John La Tourette, State University of New York, Binghamton and Harpur College: Brookings Research Professorship, 1966-67.

Darrell R. Lewis, Luther College: visiting assistant professor of economics, Louisiana State University, academic year 1966-67.

Ta-Chung Liu, Cornell University: Ziskind visiting professor of economics, Brandeis University.

Michael McCarthy, Brookings Institution: visiting associate professor, department of economics, Southern Methodist University, spring 1966.

Howard W. Nicholson, Clark University: program director for economics, National Science Foundation, Washington.

Eli Schwartz, Lehigh University: visiting lecturer, London School of Economics, fall term, 1966.

William O. Thweatt, Vanderbilt University: visiting professor of economics, Institute of Economic Research, São Paulo University, Brazil, February 1966-August 1967.

Melvin I. White, Brooklyn College: Assistant Deputy Treasurer of the United States, Washington.

Resignations

Takeshi Amemiya, Stanford University.

Jon Cunnyngham, Columbia University.

A. Deutsch, Queen's University, Ontario.

Jack E. Gelfand, Temple University, February 1966.

Arnold Katz, Columbia University.

Donald B. Keesing, Columbia University, June 1967.

David Martin, State University of New York, Binghamton and Harpur College.

Glen Wing, College of Business Administration, University of Arkansas.

Harold Wolozin, American University, Washington.

Miscellaneous

Barbara R. Berman has married Fred F. Bergmann and will be known professionally as Barbara R. Bergmann.

HAROLD HOTELLING

Distinguished Fellow

1965

To have pioneered in the development of American mathematical statistics would be fame enough for any scholar. But to have pioneered as well in applying mathematical tools to the study of economics makes Harold Hotelling indeed a prophet with honor in his own country. The welfare economics of marginal cost pricing and taxation, the properties of demand and supply functions, and the behavior of prices over space and time—all have yielded to the prepared genius of this inspired teacher.



Harold Hotelling

The American Economic Review

Volume LVI SEPTEMBER 1966 Number 4, Part 1

FOREIGN ASSISTANCE AND ECONOMIC DEVELOPMENT

By HOLLIS B. CHENERY AND ALAN M. STROUT*

Most underdeveloped countries depend heavily on external resources to increase their per capita income. A crude measure of this dependence is the net flow of some $9 billion per year from advanced to less developed countries, which is equal to a quarter of their gross investment and nearly a third of their imports.[1] Equally important is the provision of skilled manpower and transfer of technical skills through foreign assistance.

The institutional framework for this resource transfer has changed profoundly over the past ten years. Programs of foreign assistance have replaced colonial relations, and donors and recipients now agree that economic and social development is their primary objective. Private investment, which now comprises only a quarter of the total resource flow, is increasingly screened for its contribution to the recipient country's development. Thus the inflow of external resources—which can loosely be called "foreign assistance"[2]—has become virtually a separate factor of production, whose productivity and allocation provide one of the central problems for a modern theory of development.

The possibilities of securing rapid and sustained development by effective use of foreign assistance have been strikingly demonstrated in the past decade by such countries as Greece, Israel, Taiwan, and the Philippines. In each case, a substantial increase in investment financed

* The research on which this article is based was carried out by the Office of Program Coordination of the U.S. Agency for International Development. The analysis and judgments expressed are the sole responsibility of the authors. The authors are, respectively, professor of economics at Harvard University, and acting chief, Policy Planning Division, AID. They are indebted to Jaroslav Vanek, Joel Bergsman, Lorene Yap, Paula Tosini, and Carmel Ullman of AID in carrying out the analysis, and to Irma Adelman, Samuel Bowles, Francis Bator, David Cole, Robert Dorfman, and Stephen Lewis for helpful comments. An early version of the paper was presented by Chenery to the Boston meeting of the Econometric Society in December 1963. A more complete statistical analysis is given in [8].

[1] The OECD countries' component of this flow in 1963 and 1964 averages $8.3 billion, of which 70 per cent was from public sources [16].

[2] The Development Assistance Committee of the OECD defines "assistance" to include public grants and loans of more than five years' duration; it also uses a broader definition which includes private investment. The latter is more convenient for our purposes, although obviously only part of the total is "assistance" in the sense of an unrequited transfer of resources. The significance of the term as used here is that it represents a governmental decision by lenders and borrowers to secure a given transfer of resources.

largely by foreign loans and grants has led to rapid growth of GNP followed by a steady decline in the dependence on external financing. Not only was growth accelerated by foreign assistance, but the ability of each economy to sustain further development from its own resources was very substantially increased.[3]

The present study first outlines a theoretical framework designed to analyze the process of development with external assistance in quantitative terms. This framework is then used to evaluate the current performance of the developing countries and to assess their future needs for assistance under various assumptions. The evaluation suggests a range of practical possibilities for accelerating growth through external aid as well as some of the conditions which may frustrate this objective. The comparative analysis also suggests some international standards of performance which might facilitate the planning and execution of programs of foreign assistance.

I. *Aid and the Transition to Sustained Growth*

Modern theories of economic development[4] investigate the process by which a poor, stagnant economy can be transformed into one whose normal condition is sustained growth. There is general agreement on the principal changes that characterize this transformation: an increase in human skills, a rise in the level of investment and saving, the adoption of more productive technology, a substantial change in the composition of output and employment, the development of new institutions, etc. There has been relatively little analysis, however, of the possibility of accelerating these changes through the use of significant amounts of external resources over a limited period of time.

A country setting out to transform its economy without external assistance must provide for all of the requirements of accelerated growth from its own resources or from imports paid by exports. Success thus requires a simultaneous increase in skills, domestic saving, and export earnings as well as an allocation of these increased resources in such a way as to satisfy the changing demands resulting from rising levels of income. The attempt to increase output can be frustrated by failure in any one of these attempts, even when the others have been quite successful. When growth is limited in this way by a few bottlenecks, there is likely to be underutilization of other factors such as labor, natural resources, and specific types of productive capacity.

By relieving these constraints, foreign assistance can make possible fuller use of domestic resources and hence accelerate growth. Some of the

[3] This conclusion is documented in more detailed studies of Israel [6], Greece [1], and Taiwan [12].

[4] For example, Lewis [13], Rostow [20], Gerschenkron [10], and Ranis and Fei [17].

potential bottlenecks—of skills, savings, or foreign exchange—can be temporarily relaxed by adding external resources for which current payment is not required. More efficient use can then be made of other resources, so that the growth of total output may be substantially higher than would be permitted by the rate of increase of the most restrictive domestic factor.

While this alternative sequence recognizes the existence of a given set of requisites for continued growth, it makes the timing of their appearance much more flexible. The full set of requirements need only become available from domestic sources as the inflow of foreign resources is reduced. To achieve this result, the additional resources produced through more rapid growth must be used to make good the deficiencies which are temporarily being supplied from outside assistance.[5]

Two basic questions may be raised as to the feasibility of such a sequence. The first is the extent to which foreign resources can actually substitute for missing local factors and permit an increase in total output. The second is whether countries which have achieved some initial success through external assistance will take the further steps needed to reduce their dependence on it in the future. These issues will be discussed in Section II.

A. *External Resources and the Limits to Growth*

The impact of external resources on the growth of an economy can be judged by their contribution to the mobilization and allocation of all productive resources. Three types of resources should be distinguished: (1) the supply of skills and organizational ability; (2) the supply of domestic saving; and (3) the supply of imported commodities and services. At any moment in time these factor supplies represent separate limits to economic growth. While investment can be devoted to increasing the supplies of skills or of imported commodities (through import substitution or raising exports), changes in these factor supplies can only be brought about gradually. They are also substitutes in the production process to only a limited degree in the short run.

Aggregate growth models usually focus on the saving limit, which in a closed economy also sets the investment limit. When external financing is available, however, we need to examine other limits to the ability to increase investment. These may result either from limited supplies of skilled labor, entrepreneurs and other inputs complementary to the investment process or from the limited market expected for the output. The evidence cited below shows that underdeveloped countries have

[5] In criticizing the notion of a fixed set of "prerequisites" to industrialization, Gerschenkron [10] suggests other possibilities of substitution for the missing requirements which stimulate their subsequent development.

demonstrated an ability to raise the level of investment much mo
rapidly than the level of saving. Sustained rates of increase in inve
ment of 12–15 per cent per year are common, while typical figures f
saving growth are 6–8 per cent.[6]

A third factor which may limit the possibilities for accelerated grow
is a country's inability to change its productive structure to meet t
changing patterns of internal and external demand. Although this pro
lem is not likely to be serious in a slowly developing economy, rap
growth requires a large increase in the supplies of machinery and equi
ment, raw materials, and other manufactured goods that are typical
imported in a poor country. The more rapid the rate of growth, t
larger the reallocation of labor and capital away from traditional pa
terns that will be needed to prevent bottlenecks developing. If this r
allocation is not sufficiently rapid, shortages of imported goods w
provide a limit to further growth quite apart from the investment lin
tation. This import limit reflects the inability of the economy to provi
the composition of output—from domestic sources plus imports—that
required by its level of income, rate of investment, and pattern of co
sumer demand. In cases of acute shortages of imported goods, the eco
omy will be unable to transform potential saving into investment b
cause of insufficient supplies of investment goods.

The foregoing description of underdeveloped countries as characte
ized by persistent resource bottlenecks may be summed up as a hypot
esis of limited structural flexibility. In the short run—for periods of fi
to ten years—we will describe such an economy by a set of linear rel
tionships in the Harrod-Domar tradition which determine the pattern
growth under given assumptions as to government policy. This bas
model will be used to evaluate current performance as well as to mal
5–10-year projections. For longer periods, we will use a model based o
the neoclassical view that domestic resources can be substituted f
imports to the extent required by changing demands, although wi
diminishing productivity. This second model has the effect of reducir
the aid requirements for any given pattern of growth. Since most unde
developed countries fall somewhere between the two extreme cases, th
use of both models for projections indicates the probable limits to th
range of aid requirements.

1. *The Basic Model.*[7] To simplify our analysis, we shall develop
basic model of the role of aid in the transition in two steps. We conside

[6] These and other characteristics of a sample of 31 underdeveloped countries are summariz
in Table 1.

[7] This model is taken from Chenery and Bruno [6], which utilizes a detailed analysis of th
economic structure of Israel. The derivation of the three aggregate limits from an interindust
programming model is given there.

first the case in which only the first two resource limits—on skills and saving—are relevant; this situation will be described as *investment limited growth*. It includes the Harrod-Domar model as the limiting case of no external assistance. We then take up the possibilities for achieving self-sustaining growth when the balance of payments limit is effective. This situation will be identified as *trade limited growth*. The complete model includes all three potential limits.

The principal endogenous variables and parameters to be used in the basic model are the following:

Endogenous Variables (subscript indicates year)

- V_t Gross National Product
- I_t Gross investment
- S_t Gross domestic savings
- $\bar{S}_t$ Potential gross domestic savings
- M_t Imports of goods and services
- $\bar{M}_t$ Required imports of goods and services
- E_t Exports of goods and services
- F_t Net inflow of foreign capital
- C_t Consumption

Parameters

- $\bar{r}$ Target rate of growth of GNP
- r_t Rate of growth of GNP in year t
- α' Marginal savings rate $(\Delta\bar{S}/\Delta V)$
- α_t Average savings rate in year $t(S_t/V_t)$
- β Maximum rate of growth of investment
- k Incremental gross capital-output ratio $(I/\Delta V)$
- μ' Marginal import rate $(\Delta\bar{M}/\Delta V)$
- μ_t Average import rate in year t (M_t/V_t)
- Φ_t Ratio of foreign capital inflow to GNP in year t (F_t/V_t)
- ϵ Rate of growth of exports

Since the basic model is designed to explain the functions of aid and to evaluate current performance of developing countries, it is useful to have in mind the typical values of the principal parameters. Table 1 gives the upper quartile, median, and lower quartile values of each parameter for a sample of 31 countries during the period 1957–62. The sample covers most of the underdeveloped world, and the median values are quite close to the aggregate U.N. estimates for all underdeveloped countries.[8] The median capital-output ratio (3.5) and saving rate (.12)

[8] The U.N. estimates investment at 16 per cent of GNP in 1960 and a growth of GNP of 4.4 per cent for the previous decade [23, pp. 19, 37].

TABLE 1—DISTRIBUTION OF PARAMETER VALUES, 31-COUNTRY SAMPLE

Parameter	Symbol	Upper Quartile	Median	Lower Quartile
Highest 5 years in recent past				
Compound growth rate of gross investment	β	.19	.14	.10
Relationships during 1957–62				
Compound growth rate of gross investment	i	.12	.07	.01
Incremental capital-output ratio (assuming 1-year lag)	k	2.78	3.52	4.72
Compound growth rate of GNP	r	.062	.046	.034
Ratio of gross investment to GNP in 1962 (after time-trend fitting)	I_0/V_0	.20	.17	.14
Ratio of net foreign capital inflow to GNP in 1962 (after time-trend fitting)	Φ_0	.07	.04	.01
Ratio of gross national saving to GNP in 1962 (after time-trend fitting)	α_0	.16	.12	.09
Marginal national saving ratio (change in saving ÷ change in GNP)	α'	.26	.19	.02
Ratio of gross imports of goods and services to GNP in 1962 (after time-trend fitting)	μ_0	.16	.20	.39
Marginal import ratio (change in gross imports of goods and services ÷ change in GNP)	μ'	.01	.20	.46
Compound growth rate of exports of goods and services	ϵ	.080	.051	.021
Change in gold and convertible foreign currency reserves, December 1956 to December 1962 ÷ change in GNP 1957–62, GNP first converted to 1962 U. S. dollars[a] (after time-trend fitting)	ρ'	.101	−.001	−.065

[a] Excludes Trinidad-Tobago and Mauritius because of lack of data.
Source: Table A-1, Appendix.

ggest that without external assistance the typical growth rate of ıderdeveloped countries would be about 3.4 per cent or less than 1 per nt per capita.

2. *Investment-Limited Growth.* Our hypothesis of an economy with nited flexibility suggests the use of a programming model[9] in which owth proceeds at the highest rate permitted by the most limiting ctor. We assume to start with that the balance of payments does not come the limiting factor. A process by which self-sustaining growth[10] n be attained by using aid to fill the temporary gap between investent ability and saving ability can be derived from the following deription of the economic structure:

finitions:

$$V_t = S_t + C_t, \quad)$$

$$S_t = I_t - F_t, \quad)$$

pacity Limit

$$V_t \leq V_0 + \frac{1}{k} \sum_{T=0}^{T=t-1} I_T \quad \text{where} \quad k = \frac{I_{t-1}}{V_t - V_{t-1}}, \quad)$$

ility to Invest

$$I_t \leq (1 + \beta) I_{t-1}, \quad)$$

ving Limit

$$S_t \leq \bar{S} = S_0 + \alpha'(V_t - V_0), \quad)$$

rget Growth Rate

$$V_t \leq (1 + \bar{r}) V_{t-1}.$$

The *capacity limit* (3) is based on the Harrod-Domar assumption that specified amount of investment is needed to increase output. The sumption of a linear capital-output function is a matter of convenice. A similar formulation can be derived from more general production ıctions of the Cobb-Douglas type if there are not significant changes the relative costs of labor and capital. Since in most cases the period of nsition is one in which the total supply of labor is not a significant ıitation, it is plausible to approximate the aggregate production funcn in this way.[11]

A more complete statement of this model in linear programming form, given in Chenery MacEwan [7], considers the implications of the present analysis for the optimal planning development.

This concept will be defined as growth at a given rate with capital inflow limited to a cified ratio to GNP which can be sustained without concessional financing.

The introduction of a nonlinear relation between capital and output would not materially ct the conclusions of our analysis. Intercountry regression analyses suggest that there is a

The *limit on the ability to invest* (4) is introduced to reflect the widely held view that absorptive capacity for additional investment in any period is limited by the supply of complementary inputs, which can only be increased as a result of the development process. We refer to the parameter β as the "skill limit," reflecting the skill formation required of managers, skilled labor, and civil servants in order to increase productive investment.[12] The highest observed value for the skill limit over any recent five-year period is about 20 per cent per year, but few countries have sustained a growth of investment of over 10 per cent for as long as ten years.

The *saving limit* (5) is designed to include not only the marginal propensity to save but the government's ability to increase total saving by changes in the tax structure and by other policies. For this reason, we make the saving limit a function of total GNP (and hence of time) rather than of per capita income.[13]

The *target growth rate* (6) reflects the almost universal practice in developing countries of summing up the principal goal of development in a given rate of increase in GNP. In the present context, it also reflects the fact that foreign assistance is limited and is unlikely to be available to finance growth rates much above 6–7 per cent even if they were attainable. Since the average terms on external loans are based largely on the country's future economic prospects, this puts a limit on the total amount which it can afford to borrow. For all these reasons, either a target growth rate or some other reflection of the fact that investment cannot indefinitely exceed saving must be included in the model.[14]

To complete the system, we need some minimal assumptions as to the objectives of the recipient country and the conditions under which aid is provided. We assume that aid is sufficiently limited—or expensive—to make the recipient unwilling or unable to increase aid merely to increase consumption without also securing some rise in GNP. Second, we assume that the country tries to maximize consumption until the target

---

reduction in the capital-output ratio at higher growth rates but little relation to per capita income. Efforts to estimate more general production functions from time series in underdeveloped countries have been quite inconclusive because of the limited data available.

[12] In the original model for Israel [6], the skill limit was associated with labor only, but in the more typical underdeveloped country the managerial aspect is at least as important.

[13] Fei and Paauw [9] have recently utilized a similar model to analyze aid requirements for the case in which investment resources provide the limit to growth (our Phase II). They have adapted the Rosenstein-Rodan model [18] by assuming that per capita saving is a constant fraction of the increment in per capita income. There has been no empirical test of the relative merits of this alternative specification of the savings function compared to ours, but they yield similar results when the rates of growth of per capita income and population do not vary greatly.

[14] Alternative formulations are discussed in Section I.C.

growth rate is attained. These assumptions lead to a determinate pattern of growth whose welfare implications will be examined below.

The model of investment-limited growth contains six restrictions and five variables. Under the assumptions made, there is no incentive to build excess capacity or to increase aid by reducing saving. Inequalities (3) and (5) therefore become equalities. The increase in GNP will be limited first by the ability to invest and then by the target growth rate if the investment rate reaches the level ($k\bar{r}$) required to sustain it. We shall denote the first period as Phase I, which is described by equations (1) to (5). In Phase II, inequality (6) becomes effective and replaces inequality (4) as a restriction on the system.[15]

The growth path and aid requirements over time can be described by solving for V_t and F_t in each phase and determining the point at which the economy passes from Phase I to Phase II.

Phase I is characterized by a constant growth in investment at the annual rate of β and by an accelerating growth rate of GNP. From equations (3) and (4) it can be determined that the increment in investment in each period is a constant ratio (βk) to the increment in GNP. Solving the system for the level of capital inflow, gives:

$$F_t = F_0 + (\beta k - \alpha')(V_t - V_0), \tag{7}$$

where $F_0 = I_0 - S_0$. This equation shows that the increment in external capital ($F_t - F_0$) finances the difference between the increment in investment and the increment in saving. Without increased capital inflow a country having the median values of k and α' (Table 1) would have a growth of investment of about 5 per cent per year. To achieve a growth of investment of 10 per cent would require that nearly half of the increased investment during Phase I be financed by external capital.[16]

This formulation can be interpreted in terms of Harrod's original suggestion of different growth rates corresponding to the growth of the labor force (the "natural" rate) and the potential saving limit (the "warranted" rate). We have replaced the natural rate with a skill-determined rate based on the ability to invest. External assistance fills the gap between investment and saving, permitting the higher rate to be reached.

Phase I ends in year m when investment reaches a level adequate to sustain the target rate of growth:

[15] As shown in [7], this result can be derived in more formal terms by maximizing a welfare function having the characteristics indicated subject to the given restrictions. Each phase is then defined by the restrictions which are binding, which have positive shadow prices. This linear programming formulation is quite useful if we replace the assumption of a target growth rate by a more complicated set of limits, but it is unnecessary with the simplified assumptions made here.

[16] With $k = 3.5$ and $\alpha' = .19$, growth of investment at 10 per cent would require $\Delta I = .35\ \Delta V$ of which $.19\ \Delta V$ (54 per cent) would be financed by increased savings and $.16\ \Delta V$ (46 per cent) by increased capital inflow.

$$I_m = k\bar{r}V_m. \tag{8}$$

Substituting this value for investment in the equations for Phase I gives the value of GNP in the terminal year:

$$V_m = V_0 \frac{(\beta - r_0)}{(\beta - r_m)} . \tag{9}$$

If, for example, investment grows at 10 per cent per year, the hypothetical median country could increase its investment rate from 12 per cent of GNP with no aid to the 21 per cent needed to support a 6 per cent growth target in a period of eleven years.[17]

Phase II in our model corresponds to the process of aid and growth discussed by Rosenstein-Rodan [18] [19]. GNP and investment rise at a constant rate with external assistance determined by the difference between $k\bar{r}$ and α_t. Solving the system for the rate of growth yields a modified form of the Harrod-Domar equation:

$$r_t = \frac{\alpha_t + \phi_t}{k}, \tag{11}$$

where

$$\alpha_t = (\alpha_0 - \alpha') \frac{V_0}{V_t} + \alpha' \quad \text{and} \quad \phi_t = \frac{F_t}{V_t} .$$

In order for the rate of capital inflow to decline, the marginal saving rate α' must exceed the investment rate $k\bar{r}$ required by the growth target. If this condition is satisfied, the system can be solved for the level of GNP in year p when the saving rate has risen sufficiently to eliminate the capital inflow:

$$V_p = \frac{(\alpha' - \alpha_m)}{(\alpha' - k\bar{r})} V_m. \tag{12}$$

Since α' reflects the total effect of government policies on saving, there is no reason to assume that it will remain constant throughout the period of the transition.[18]

3. *Trade Limited Growth.* The process of growth with a varying inflow

[17] The time to complete Phase I can be determined by solving for m in the following equations:

$$\frac{I_m}{I_0} = (1 + \beta)^m = \frac{\bar{r}}{r_0} \frac{(\beta - r_0)}{(\beta - \bar{r})} \tag{10}$$

where $r_0 = I_0/k V_0$ and $\bar{r} = r_m$.

[18] The effects on the transition of plausible variations in the saving rate are illustrated by the Pakistan example in the next section.

of capital requires a continual adjustment in imports and exports to make the trade gap equal the desired gap between investment and saving. We have assumed so far that this adjustment process—whether achieved through the market mechanism or through government controls—does not affect the growth path or the aid requirements. For many of the countries in Phase II, however, this assumption may not be valid.

Although in Phase I the rising capital inflow needed reduces the pressure on the balance of payments, the tapering off of the capital inflow in Phase II requires exports to rise more rapidly than imports. The empirical analysis of Section II suggests that many countries have been unable to bring about this required adjustment in their productive structure. While this situation may have been caused by overvalued exchange rates or other inefficient policies, the resulting trade gap is often "structural" in the sense that it can only be reduced over time without reducing the rate of growth by a redirection of investment and other resources.[19]

The trade limit can be incorporated into the preceding analysis in a form quite analogous to the saving-investment limit. We postulate a minimum import level ($\overline{M}_t$) required to sustain a given level of GNP at time t, which is similar to the capacity requirement of equation (3). This import requirement results from the relatively inelastic demand for a large proportion of the manufactured goods currently imported—particularly intermediate goods and investment goods—arising from the lack of domestic supply and their necessity in production. Actual imports may, of course, exceed this minimum. This requirement may be stated as:

$$M_t \geq \overline{M}_t = \overline{M}_0 + \mu'(V_t - V_0) \tag{13}$$

where the minimum marginal import ratio μ' may be derived as the average of the incremental ratios for different components of demand.[20] While the marginal import ratio is probably more subject to policy control than the capital-output or saving ratios, it represents an important structural limitation over planning periods as long as 10–15 years.

The existing economic structure at any moment in time also limits the feasible growth of export earnings. Since export earnings for many primary products are largely determined by demand conditions, a rapid increase in exports typically requires the development of new export products, which is limited by productive capacity as well as organiza-

[19] The nature of the trade limit is discussed further in [5] [6] [15].

[20] These estimates have been made for countries such as Israel, Italy, Pakistan, India, and Argentina by use of input-output models in which import substitution is incorporated on a sector basis. The procedure is described in [5] and [6].

tional and institutional factors. The order of magnitude of this limit is indicated in Table 1 by the recent growth rate of exports, whose median value is 5.1 per cent and upper quartile value is 8.0 per cent. The effects of government policies to increase exports are summarized by the parameter ϵ in the following expression for the export limit:[21]

$$E_t = E_0(1 + \epsilon)^t. \tag{14}$$

The *combined trade limit* is expressed by the requirement that the capital inflow be at least large enough to cover the minimum gap (F_t^m) between import requirements and export earnings:

$$F_t \geq F_t^m = \overline{M}_t - E_t. \tag{15}$$

When the capital inflow determined by the saving-investment gap in equation (2) is greater than the minimum trade gap, the two gaps can be equated by having imports in excess of the specified minimum or exports less than the assumed maximum of equation (14). When the minimum trade gap is the larger, however, it controls the rate of growth of GNP and the inflow of capital. In this case, either saving will fall below the saving potential specified by equation (5) or less productive investment will take place. In either case the saving limit ceases to be binding.

Theoretically the trade limit may replace the saving limit as a determinant of the capital inflow in either Phase I or Phase II. Empirically, this is less likely to happen in Phase I, since the rising capital inflow does not usually require exports to increase as fast as imports.[22] Once a target growth in GNP is attained, however, exports must rise more rapidly than imports if aid is to be reduced. If the trade limit becomes effective at all, it is therefore more likely to be during Phase II. We shall denote the new set of restrictions which would be binding in this event as Phase III.[23]

In *Phase III* inequalities (3), (6), and (15) become equalities, while limits (4) and (5) are redundant. For a given target rate of growth, GNP is determined by equation (6) as in Phase II. The capital inflow is determined by (15) and exceeds that required by the saving gap.[24] In order for

[21] It is probable that ϵ depends on the growth of GNP to some extent, but we have taken account only of the relationship (14) in applying the model.

[22] The relative growth rates required depend on the initial ratio of exports to imports.

[23] The three phases described here can follow each other in any order if we allow the structural parameters to change at random over time. With fixed parameters, the commonest sequence (as shown in Section II) is from Phase I to either Phase II or Phase III. We have not tried to trace such a sequence of phases historically except in the Adelman-Chenery model of Greek development [1] and the Chenery-Bruno analysis of Israel [6].

[24] When the trade gap determines the capital inflow in Phase I, we will denote the corresponding set of restrictions as Phase IB. This combination does not seem to be of great significance empirically. The more common case in which the ability to invest and the saving limit are controlling will be renamed Phase IA.

the capital inflow to be reduced, either export growth must exceed the target rate for GNP or the marginal import ratio must be substantially less than the initial average. From equations (13) and (14) we derive the following condition for the elimination of the trade gap with constant structural parameters within a given period $(q-j)$:

$$\frac{E_j}{M_j}(1+\epsilon)^{q-j} - \frac{\mu'}{\mu_j}(1+\bar{r})^{q-j} \geq \left(1-\frac{\mu'}{\mu_j}\right). \tag{16}$$

Since the parameters ϵ and μ' are more amenable to policy control in the long run than are k and α', Phase III conditions are less likely to persist throughout the transition than are those of Phase II unless the efforts of the underdeveloped countries to increase their exports are frustrated. For the projections to 1975 that are made in Section II, however, Phase III is of great importance.

4. *Total Requirements for External Capital.* The total capital required under our assumptions to complete the transition to self-sustaining growth can be determined as the sum of the capital requirements for each phase that the economy goes through. In Phases IA and II, external capital is determined by the cumulative difference between investment and savings. In Phases IB[25] and III, it is the cumulative difference between import requirements and exports.

The equations for capital inflow in each phase are given in a symmetrical form in Table 2. All variables are expressed as a ratio to the initial level of GNP (V_0). Summing these equations over time and assuming constant parameter values gives the total capital inflow during any period that the economy remains in that phase. These formulas for cumulative capital inflow are used in subsequent comparisons of growth paths and capital requirements.

B. *The Transition in Pakistan*

We can best illustrate the operation of our basic model by applying it to a specific case. Pakistan will be chosen for this purpose because it has actually started from a very low level of income and accelerated its rate of growth through the use of external resources. Unlike the more advanced countries cited earlier, however, Pakistan has only completed the first decade of a process which may take several decades more. The projections which we will make for Pakistan, therefore, illustrate the procedure to be followed for a large number of countries in Section II in calculating the range of future needs for external capital.

For the past eight to ten years Pakistan has been following the sequence envisioned in our Phase I of a rapid expansion in investment,

[25] See footnote 24.

Phase	Growth Constraint	Determinant of Foreign Capital	Dependent Variable	Investment or Imports (equals)	Savings or Exports (minus)
IA	Ability to Invest (4)	Investment-Saving	F_t/V_0	$= \frac{I_0}{V_0}(1+\beta)^t$	$-\left\{\alpha_0 + \frac{I_0}{V_0}\frac{\alpha'}{k\beta}[(1+\beta)^t - 1]\right\}$
			$\sum_0^t F_t/V_0$	$= \frac{I_0}{\mu_0}\left[\frac{(1+\beta)^{t+1}-1}{\beta}\right]$	$-\left\{(t+1)\left(\alpha_0 - \frac{I_0}{V_0}\frac{\alpha'}{k\beta}\right) + \frac{I_0\alpha'}{V_0 ki}\left[\frac{(1+\beta)^{t+1}-1}{\beta}\right]\right\}$
			or	$= \frac{I_0}{V_0}\left[\frac{(1+\beta)^{t+1}-1}{\beta}\right]\left(1 - \frac{\alpha'}{k\beta}\right)$	$-(t+1)\left(\alpha_0 - \frac{I_0}{V_0}\frac{\alpha'}{k\beta}\right)$
IB	Ability to Invest (4)	Imports-Exports	F_t/V_0	$= \mu_0 + \frac{\mu'}{k\mu}\cdot\frac{I_0}{V_0}[(1+\beta)^t - 1]$	$-\frac{E_0}{V_0}(1+\epsilon)^t$
			$\sum_0^t F_t/V_0$	$= (t+1)\left(\mu_0 - \frac{\mu'}{k\beta}\frac{I_0}{V_0}\right) + \frac{\mu'}{ki}\frac{I_0}{V_0}\left[\frac{(1+\beta)^{t+1}-1}{\beta}\right]$	$-\frac{E_0}{V_0}\left[\frac{(1+\epsilon)^{t+1}-1}{\epsilon}\right]$
II	Growth Target (6)	Investment-Saving	F_t/V_0	$= k\bar{r}(1+\bar{r})^t$	$-\{\alpha_0 + \alpha'[(1+\bar{r})^t - 1]\}$
			$\sum_0^t F_t/V_0$	$= k[(1+\bar{r})^{t+1} - 1]$	$-\left\{(t+1)(\alpha_0 - \alpha') + \alpha'\left[\frac{(1+\bar{r})^{t+1}-1}{\bar{r}}\right]\right\}$
			or	$= (k - \alpha'/\bar{r})[(1+\bar{r})^{t+1} - 1)]$	$-(t+1)(\alpha_0 - \alpha')$
III	Growth Targets (6)	Imports-Exports	F_t/V_0	$= \mu_0 + \mu'[(1+\bar{r})^t - 1]$	$-\frac{E_0}{V_0}(1+\epsilon)^t$
			$\sum_0^t F_t/V_0$	$= (t+1)\left(\mu_0 - \mu'\right) + \frac{\mu'}{\bar{r}}[(1+\bar{r})^{t+1} - 1]$	$-\frac{E_0}{V_0}\left[\frac{(1+\epsilon)^{t+1}-1}{\epsilon}\right]$

saving and external assistance. In 1956[26] Pakistan was in the lower quartile of countries with respect to its investment, saving, and growth rates. As shown in Table 3, its performance since then has approximated the upper-quartile values for the main performance measures in our model: absorptive capacity, capital-output ratio, marginal saving rate, and export growth. We take 1962 as the base year for these and all subsequent projections. Base-year values for the variables in the model are given in Table 4, expressed as ratios to 1962 GNP.

The growth process from 1962–75 will be determined from the basic model under two sets of assumptions as to the values of the parameters. The more pessimistic (A) assumptions are based on a conservative interpretation and projection of performance in recent years; the corresponding parameter values are labeled "historical performance" in Table 3. The more optimistic (C) assumptions are derived from the Pakistan Perspective Plan for 1965–85; they are labeled "upper limit" perfor-

TABLE 3—STRUCTURAL PARAMETERS FOR PAKISTAN PROJECTIONS

	Growth Target $\bar{r}$	Absorptive Capacity β	Capital-Output Ratio k	Marginal Saving Rate α'	Marginal Import Requirement μ'	Export Growth Rate ϵ
Historical Estimates						
(1957–62)[a]	.041	.15	2.35	.25	.20	.075
(1959–65)[b]	.05	.20	2.8	.22		.070
Projections for 1962–75[c]						
"Historical" Performance (A)	.045	.13	3.0	.16	.10	.049
"Upper Limit" Performance (C)	.060	.13	3.0	.24	.10	.070
Pakistan Perspective Plan[b]	.075	—	2.9	.25	.06	.079
Representative Values[d]						
Median		.14	3.5	.19	.20	.051
Upper Quartile		.19	2.8	.26	.01	.080

[a] *Source:* Table A-1.
[b] *Source:* Pakistan's *Third Five Year Plan*, (1965–70) [10].
[c] General bases for the projections are discussed in the Annex. The parameters are the same as those used for the 50-country projections (Table A-2) except for the export growth rate, which has been revised upward to 7 per cent in the light of recent experience and the revised plan estimate.
[d] From Table 1.

[26] The year 1956 marked the beginning of the first five-year plan, although the plan had little effect on the economy for several years thereafter.

(All values expressed as ratios to initial GNP)

Year	1956[b]	1962[b]	1963	1964	1965	1966	1967	1968	1970	1975
Historical Performance										
GNP	.838	1.000	1.041	1.087	1.136	1.188	1.241	1.296	1.416	1.764
Investment	.059	.122	.138	.147	.153	.160	.168	.175	.191	.238
Potential saving	.039	.090	.097	.104	.112	.121	.129	.138	.157	.213
I-S gap[c]	.020	.032	.041	.042	.041	.040	.039	.037	.034	.025
Potential imports	.074	.100	.104	.109	.114	.119	.124	.130	.142	.177
Exports	.054	.068	.072	.075	.079	.083	.087	.091	.100	.128
M-E gap[c]	.020	.032	.032	.033	.035	.036	.037	.038	.041	.049
Consumption	.799	.910	.944	.983	1.024	1.067	1.112	1.159	1.266	1.575
Phase	I	I	IA	II	II	II	II	III	III	III
Upper-Limit Performance										
GNP	.838	1.000	1.041	1.087	1.139	1.198	1.264	1.339	1.504	2.012
Investment	.059	.122	.138	.156	.176	.199	.225	.241	.272	.364
Potential saving	.039	.090	.100	.111	.124	.138	.154	.172	.212	.334
I-S gap[c]	.020	.032	.038	.045	.052	.061	.071	.069	.059	.029
Potential imports	.074	.100	.104	.109	.115	.120	.126	.134	.151	.201
Exports	.054	.068	.073	.078	.083	.089	.095	.102	.117	.164
M-E gap[c]	.020	.032	.031	.031	.032	.031	.031	.032	.034	.037
Consumption	.799	.910	.941	.976	1.015	1.060	1.110	1.167	1.292	1.687
Phase	I	I	IA	IA	IA	IA	IA	II	II	III
Growth rates:										
GNP	.021	.041	.044	.048	.052	.055	.059	.060	.060	.060
Investment	.130	.130	.130	.130	.130	.130	.130	.071	.060	.060
Consumption	.015	.034	.037	.040	.044	.047	.051	.052	.053	.060

[a] Projections derived from the base-year data for 1962 and parameter values in Table 3.

[b] The 1956 and 1962 figures are trend values for the period 1956–62. The latest revision of the Pakistan national accounts [11] gives a similar investment level but higher initial savings and a negative marginal savings rate for the period 1954–55 to 1959–60.

[c] For 1956 and 1962, potential and actual savings and imports are the same, and the two gaps are identical; from 1963 onward, the larger gap is

mance.[27] In the case of Pakistan, the upper-limit assumptions are higher than the historical estimate for the growth target, the saving rate, and the growth of exports; the other three parameters already seem optimistic and have been kept unchanged.

Figure 1 and Table 4 show trend values of the variables from 1956 to 1962 and the two sets of projections derived from the model for 1962–75. Although the data before 1960 are not very reliable, it is clear that investment from 1956 to 1964 has grown very rapidly and doubled its share in GNP.[28] The rate of output growth has increased from about 2 per cent prior to 1958 to over 4 per cent since 1960.

The two sets of projections give the following sequence of phases:

	(A) Historical Performance	(C) Upper-Limit Performance
Phase I[29]	1956–63	1956–67
Phase II	1964–67	1968–73
Phase III	1968–	1974–
End of Transition	After 1985	After 1979
Target Growth Rate	4.5 per cent	6.0 per cent

In case C it takes ten years of steadily rising investment from the 7 per cent level of 1956 to reach the rate of 18 per cent of GNP required by a growth rate of 6 per cent. The capital inflow would reach a maximum of 6 per cent of GNP in 1967; throughout Phase I it would finance about 30 per cent of total investment. If the saving-investment limit were the only constraint on the system, the capital inflow could then be reduced to zero by 1980 if the marginal saving rate of .24 were maintained.[30] However, even with the relatively high export projection of 7 per cent per year, the model projects a switch to Phase III in 1974 with a marginal import requirement of .10. There is also a switch to Phase III in case A, even though export growth is assumed higher than the growth of GNP. The same phenomenon occurs in the projections in Table 7 below for the majority of developing countries.

[27] The projections in Section II also contain an intermediate set of estimates and growth targets for each country designated as "realistic plan performance." When the country's own plan seems quite optimistic, as in Pakistan, we have taken it as the basis for the "upper limit" estimates.

[28] The figures in Table 4 are derived from trends fitted to the time series for each variable and differ somewhat from estimates based on the initial and terminal years of each series. A detailed account of the decade 1955–65 is given in the Pakistan Third Five-Year Plan [11]. The general picture that it gives is similar to our upper-limit projections through 1965 except that both investment and foreign capital inflow are higher in the latter year. The statements in the text are consistent with both sets of estimates.

[29] There is considerable evidence that the trade limit was the controlling factor from 1956–59, which would identify this period as Phase IB.

[30] This is approximately the assumption of the revised Pakistan Perspective Plan [11 which aims at a 7 per cent growth rate and a termination of aid by 1985.

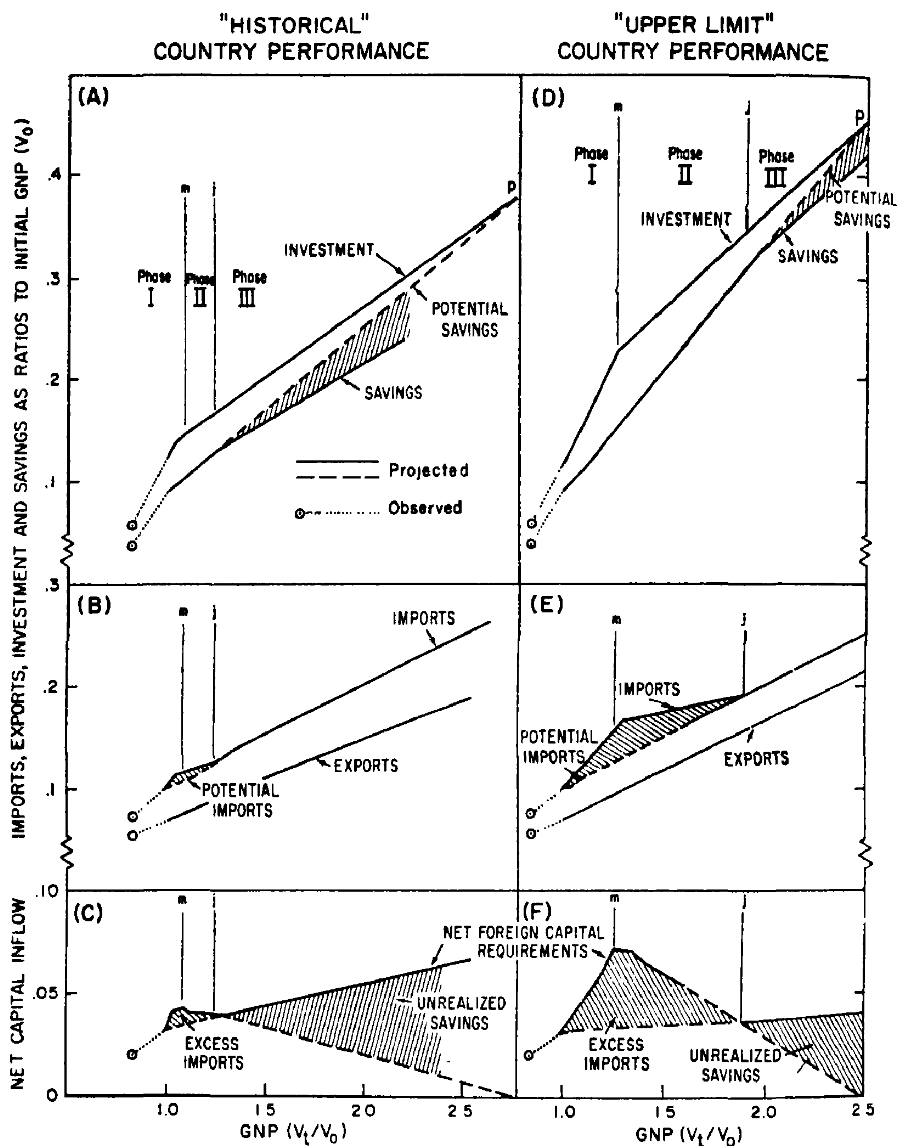


FIGURE 1. ILLUSTRATIVE GROWTH PATHS: PAKISTAN, PAST AND PROJECTED

The importance of the marginal saving rate is demonstrated by a comparison of the two projections. If the balance of payments parameters are subject to sufficient policy control in the longer run (as suggested below), the economy will reach self-sustaining growth at a rate of 4.5 per cent in 1985 under the lower assumption of a 16 per cent marginal saving rate. With a 24 per cent saving rate, a self-sustaining growth

rate of 6 per cent can be attained six years sooner. While more aid is required in the latter case, there is a much larger increment of saving and investment as well.[31]

C. *More Efficient Growth Paths*

The more time that is allowed for an economy to adjust its productive structure to the changing pattern of demand, the less likely it becomes that the rigidities assumed in the basic model will persist. We shall therefore construct a second model which assumes coordinated development policies and a planned adjustment of the trade gap and the saving gap. Actual cases will probably lie somewhere between these extremes.

Under the assumptions of model 1, the amount of external resources needed to fill the larger gap in a given year more than fills the smaller one. As compared to the minimum needs of the economy, there will be a surplus of imports ($M > \overline{M}$) in Phase II or a loss of potential saving, ($S < \overline{S}$) in Phase III. A better coordinated development policy would attempt to reduce the required capital inflow by substituting investment for imports—or vice versa—in order to equate the two gaps *ex ante* over the long run.[32]

If we assume efficient resource allocation, the equilibrium exchange rate—reflecting the opportunity cost of earning or saving foreign exchange at the margin—will be a function of the amount of inflow of external resources, F. Under *ceteris paribus* assumptions, a reduction in F normally implies an increase in the value of foreign exchange as marginal activities of import substitution or additional exports are undertaken. Since capital is the only scarce domestic resource in our model, we assume that a higher capital coefficient is needed to reduce the import requirements of model 1 and conversely that a saving in capital will result when imports increase above the minimum level.[33]

These assumptions form the basis for model 2, in which an import substitution activity is added to model 1.[34] Investment in import substitution I_m requires imported capital goods and a greater amount of capital (bk) per unit of imports replaced by domestic production. The net

[31] A generalization of these relationships is given in Section I.D.

[32] Given the durability of capital, it may not be efficient to equate the two in the short run, especially when there is a significant degree of disequilibrium to start with.

[33] The efficient reallocation of resources to accord with variations in the capital inflow is analyzed in detail in Chenery [5], which provides the basis for the aggregate formulation given here. We have approximated the diminishing marginal productivity in import substitution by a single incremental ratio.

[34] The same argument can be made for export expansion, using the marginal revenue product of additional investment to allow for the inelasticity of export demand. For convenience, we assume only import substitution here.

reduction in import requirements at time t (M_{m_t}) is given by:

$$M_{m_t} = \frac{1}{bk} \sum_{0}^{t-1} I_{m_t} - aI_{m_t}, \tag{17}$$

where a is the import content of I_m above the average for the economy and b is greater than 1.

Equation (13) of the basic model will then be replaced by:

$$M_t \geq \overline{M}_0 + \mu'(V_t - V_0) - M_{m_t}. \tag{13'}$$

The capacity limit, equation (3), must also be modified to allow for the lower productivity of capital in import substitution.

Model 2 will be used to estimate the minimum capital inflow needed to achieve a given level of GNP, first in the Pakistan example and later in comprehensive projections. For this purpose we assume (1) that total import substitution (positive or negative) is sufficient to eliminate the difference between the two structural gaps over the period 1962–75, and (2) that this type of investment increases linearly throughout the period. Solutions calculated for Pakistan for varying growth targets are shown in Table 5.[35] Cumulative values of the two gaps in model 1 and of the single gap in model 2 are plotted in Figure 2.[36]

Figure 2 shows that at a growth rate of GNP of 5.2 per cent, the cumulative values of the two resource gaps in model 1 are the same and equal to the total requirement for foreign capital in model 2. At lower growth rates, the trade gap predominates in model 1 and the difference between the two curves represents an excess of consumption. In model 2, about a third of this excess is utilized to finance the additional investment needed for import substitution; the remaining two-thirds is eliminated by reducing the capital inflow.[37]

At growth rates above 5.2 per cent, Phase II predominates in the basic model and the possibilities of reducing the capital inflow through (negative) import substitution are less. On our assumptions, substituting imports for investment would produce a reduction in the dominant

[35] We have used a value of b of 1.5, which implies that additional import substitutes would become profitable at an average exchange rate 50 per cent higher than the present effective rate. This value was judged to be the upper limit to the cost of import substitution or increased exports in the amounts needed to reconcile the two gaps. A comparable value was found in the investment programming model for Southern Italy [5]. The additional import content of investment (a) is taken as .25, corresponding to a total import content of .35.

[36] For model 1, we have assumed that the total capital inflow would be equal to the larger of the cumulative resource gaps because of the possibilities of adjustment through stock change, variations in imports, building ahead of demand, etc. Taking the larger resource gap on an annual basis would give somewhat higher totals for model 1 in Table 5 and Figure 2.

[37] For example, at a growth rate of 4 per cent excess consumption of .59 in model 1 is converted into increased investment of .19 and reduced imports and capital inflow of .40 in model 2. The same proportions hold at other growth rates below 5.2 per cent.

Table 5—Effects of Import Substitution Policy on Capital Inflow Pakistan (1962–75)[a]
(All figures expressed as ratios to 1962 GNP)

	ALTERNATIVE GROWTH TARGETS				
1. Target 1975 GNP	1.468	1.665	1.886	2.133	2.410
2. (GNP compound growth rate)	(.03)	(.04)	(.05)	(.06)	(.07)
3. Cumulative Exports (both models)	1.33	1.33	1.33	1.33	1.33
Model 1 (Cumulative Values)					
4. GNP	17.09	18.29	19.60	21.02	22.55
5. Investment	1.54	2.19	2.94	3.78	4.74
6. Savings: (Potential)	(2.00)	(2.29)	(2.60)	(2.94)	(3.31)
7. Savings: Realized	1.16	1.70	2.31	2.94	3.31
8. Imports: (Potential)	(1.71)	(1.83)	(1.96)	(2.10)	(2.26)
9. Imports: Realized	1.71	1.83	1.96	2.17	2.76
10. Excess Consumption	.84	.59	.29	.0	.0
11. Excess Imports	.0	.0	.0	.07	.50
12. Net Capital Inflow	.38	.50	.63	.84	1.42
13. (Dominant Phase)	(III)	(III)	(III)	(II)	(II)
Model 2 (Cumulative Values)[b]					
14. GNP	17.09	18.29	19.60	21.02	22.55
15. Investment	1.81	2.38	3.03	3.76	4.58
16. (Per cent Investment in Import Substitution)	(44%)	(24%)	(9%)	(−2%)	(−10%)
17. Savings	2.00	2.29	2.60	2.94	3.31
18. Imports	1.14	1.43	1.76	2.15	2.60
19. Net Capital Inflows	−.20	.09	.43	.82	1.26
Welfare Effects					
20. Consumption, Model 1	15.93	16.60	17.29	18.07	19.24
21. Consumption, Model 2	15.09	16.00	16.99	18.07	19.24
22. Change in Consumpion (line 21 minus 20)	−.84	−.59	−.29	.0	.0
23. Change in Capital Inflow (line 19 minus 12)	−.57	−.40	−.20	−.02	−.16
24. (Ratio, line 22÷23)	1.47	1.47	1.47	.0	.0

[a] Assumes no constraints on growth of investment or GNP. This means that country could invest sufficient capital in each year to attain the GNP growth rate given in line 2. Actual 1962 investment was sufficient for an initial growth rate of about .04.

[b] The basis for Model 2 is given in the text. The formulas for calculation are detailed in [8, Annex B].

saving gap of about a third of the difference between the two gaps in model 1.

A more efficient method of reducing the cumulative capital inflow in cases where the saving gap exceeds the trade gap is to accelerate the rate of growth at the beginning of the period instead of maintaining a con-

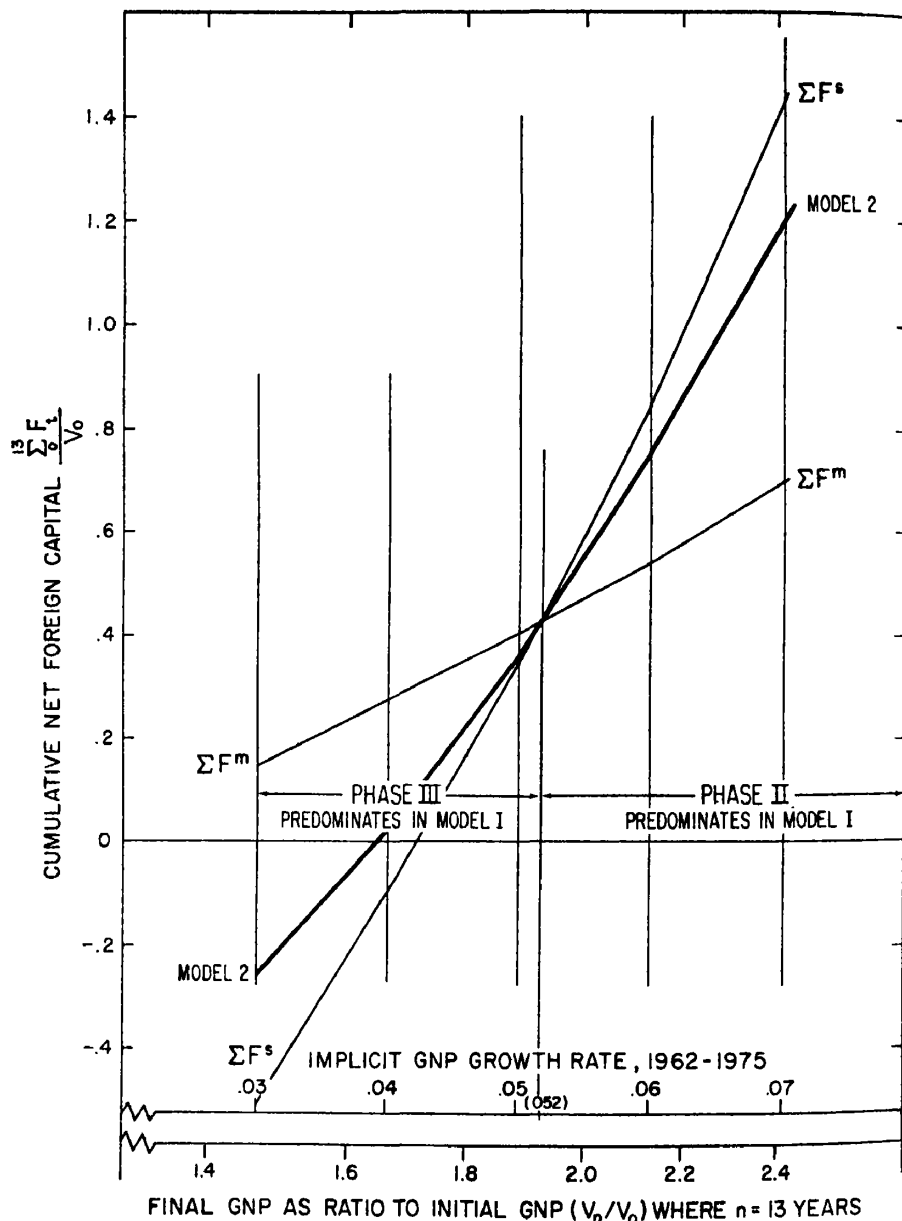


FIGURE 2. GAP EQUALIZATION THROUGH ADDITIONAL IMPORT SUBSTITUTION (MODEL 2), PAKISTAN, 1962–75

stant growth rate. This has the effect of increasing total saving as well as total imports, but the net reduction in the capital inflow is greater than with model 2.[38] The practical scope for raising the growth rate in most countries is limited by absorptive capacity constraints, however.

A more comprehensive analysis of the efficiency of alternative growth patterns would require us to abandon the assumption of a given target growth rate and to determine the growth target and the pattern of capital inflow from the objectives of the economy and the limits to the use of various policy instruments. This has been done in a linear programming analysis of optimal growth patterns in Pakistan [7]. The results confirm our assumption that it is efficient to eliminate the *ex ante* difference between the two resource gaps to the extent feasible. The main features of the growth pattern determined from the basic model also characterize the optimal solutions to the more general planning model.[39]

D. *The Productivity of External Resources*

The productivity of an increment in external resources supplied to a developing country can be measured by the corresponding increase in consumption or total income which it makes possible. The value of external resources depends on the extent to which they facilitate the fuller use of domestic factors. In our models, it is possible to measure the effect of increasing the supplies of investment funds and foreign exchange, but we have no estimate of the possibilities for raising the skill limits to growth.

Figure 2 provides one measure of the marginal productivity of external resources in Pakistan over the range of growth rates indicated.[40] Under the assumptions of model 1, the productivity of aid is much higher in Phase III, when the balance of payments is the factor limiting growth. This result is stated in more general terms in the following formulas for the derivative of the terminal year income with respect to the total capital inflow from the equations in Table 2.

[38] The theoretical aspects of the variable growth mechanism are more fully explored in [8]. There it is shown that slowing down growth to save on external capital in Phase III is a very inefficient alternative to import substitution. In the present example it results in a loss of $11 of consumption per dollar of capital inflow saved.

[39] The principal differences between model 1 and the linear programming solutions are (1) a continuation of Phase I beyond the point at which the target rate is achieved in order to maximize the benefits of accelerated growth; (2) replacement of Phases II and III by a single regime in which the capital inflow is reduced to zero with the two gaps kept equal by import substitution, as in model 2 above.

[40] Since Figure 2 is designed to illustrate the effects of import substitution, we have omitted the complicating element of absorptive capacity, which would raise the total capital inflow required for higher growth rates and put an absolute ceiling on the maximum growth achievable of about 6.3 per cent by 1975 in the Pakistan example. Discounting the total capital inflow at 8 per cent would reduce the total value by 30–40 per cent and raise its marginal productivity.

For Phase II:

$$\frac{d(V_{t+1})}{d(\sum F_t^s)} = \frac{1}{k - \alpha'\gamma}, \tag{18}$$

For Phase III:

$$\frac{d(V_{t+1})}{d(\sum F_t^m)} = \frac{1}{\mu'\gamma}, \tag{19}$$

where

$$\gamma = \left[\frac{t - \dfrac{1-(1+\bar{r})^{-t}}{\bar{r}}}{\bar{r}(t+1)}\right].$$

Values of γ for relevant time periods and growth rates are:

	Values of γ		
$\bar{r}$	$t=4$	$t=9$	$t=14$
.03	1.8	4.1	5.9
.05	1.8	3.8	5.5
.07	1.7	3.5	4.9
.10	1.7	3.4	4.4

These formulas give the following values for the increase in terminal year income per dollar of increase in cumulative assistance for Pakistan and for the median parameter values of Table 1 (assuming $\bar{r}=.05$ and $t=14$).

	Pakistan	Median Values
Productivity in Phase II	.44	.35
Productivity in Phase III	1.14	.91

For periods under 20 years, there is a pronounced tendency for the two productivity curves to have the relative slopes indicated for Pakistan, with Phase II predominating at high rates of growth.[41] As the length of time increases, the productivity of assistance in Phase II rises because of the additional saving generated, while the producitivity in Phase III falls. Under the more optimal policies assumed in model 2, there is a single productivity curve with a slope closer to that of Phase II in model 1.[42]

For long-term development policy, it is more useful to consider the total assistance required to complete the transition to self-sustaining

[41] This result was also obtained by Chenery and Bruno [6] for Israel and by McKinnon [15] for more specialized assumptions. An estimate of the productivity of aid to Greece is given in [1].

[42] The marginal productivity curve derived from the linear programming model of [7] is similar to that for model 2.

growth in relation to the country's performance. This can be done by varying the parameters for Phase II of model 1 over the range of values observed in Table 1. The results are given graphically in Figure 3, which shows the total undiscounted capital inflow required to produce a self-sustaining growth rate of 5 per cent from the low initial saving rate of 8 per cent of GNP.[43]

To show the effect on aid requirements of a change from average performance to "good" performance, we have plotted points corresponding to median values of k and α' (point H) and also upper quartile values (point U). Median performance requires a total capital inflow of more than $2\frac{1}{2}$ times the initial GNP and a period of 43 years to complete the transition to self-sustaining growth. Upper-quartile performance requires a capital inflow equal to only a quarter of the initial GNP and a period of eight years to reach self-sustaining growth (if we ignore the absorptive capacity limitation). Between these extremes, we might distinguish as "good performance" combinations of k and α' which accomplish the transition with a total capital inflow of not more than the initial GNP, such as $k=3.2$ and $\alpha'=.20$. These results will be utilized in the discussion of assistance policy in Section III.

II. *Prospects for the Transition*

The preceding analysis provides a way of thinking about external resources as an element in the development process. Their contribution to growth may be large or small depending on the response of the recipient country. We shall now try to evaluate the recent performance of the less developed countries and assess their possibilities for further growth and their needs for external resources.

Since efforts to accelerate growth through foreign assistance have been concentrated in recent years, our statistical analysis is based on the period 1957–62. Rough estimates of the basic relations in model 1 have been made for 50 countries which account for 90 per cent of the GNP of the underdeveloped world.[44] Principal attention will be given to 31 of these countries for which the data are judged to be more reliable.[45] Our interpretation of the results will also utilize more detailed analyses of a dozen of the most important recipients of U. S. assistance.[46]

[43] Model 2 can be approximated by Phase II of model 1 by taking a weighted average of the two incremental capital output ratios in model 2. Figure 3 ignores the absorptive capacity limitations, which would tend to raise the time required for the transition.

[44] Omitting Communist China, Cuba, and N. Korea.

[45] The only large countries omitted from the 31-country sample are: Ceylon, Ethiopia, Indonesia, South Vietnam, the Sudan, and the United Arab Republic.

[46] Preliminary results of the more detailed studies are available for Greece [1], Turkey [25], Colombia [24], and Taiwan [12]. Other countries for which more detailed models have been constructed by AID in order to test the "two gap" analysis of aid requirements and performance include India, Pakistan, Argentina, Brazil, Korea, Jordan, Nigeria, and Chile.

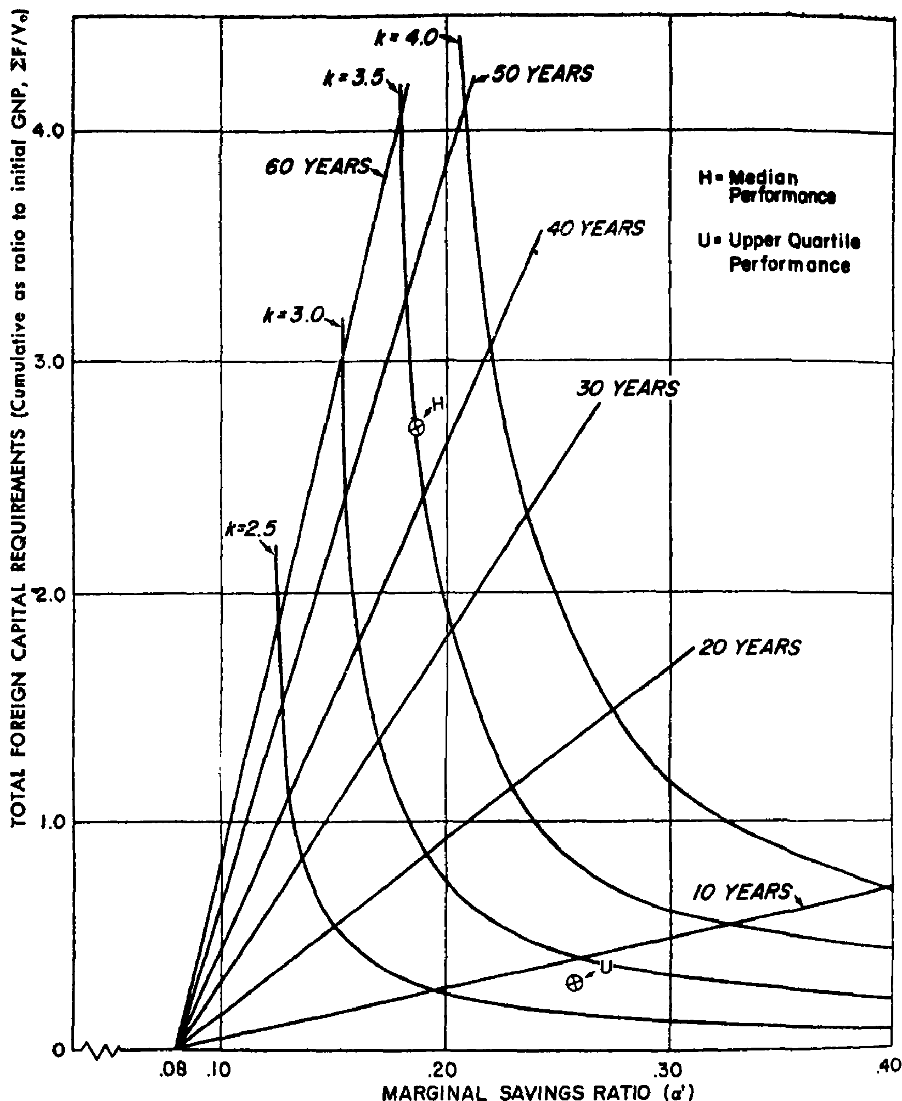


FIGURE 3. TOTAL CAPITAL INFLOW REQUIRED TO REACH SELF-SUSTAINING GROWTH (Assumes 5 per cent GNP growth rate, initial saving/GNP ratio of .08 and Phase II throughout)

A. *Evaluation of Current Performance.*

The statistical evaluation of current performance is designed to show the extent to which underdeveloped countries have established the structural conditions required to utilize aid effectively in carrying out a transition to self-sustaining growth. It also sheds some light on the validity of our simplified model and provides a basis for subsequent projections of future growth.

For this survey we have adopted a uniform statistical procedure that is applied to all 31 countries. Estimates of the parameters in the basic model are given in Table A-1 below. The main features of the statistical procedure are as follows:

1. All estimates were made from linear trends fitted to time series for the period 1957–62. Marginal saving rates (α'), marginal import rates (μ'), and marginal capital-output ratios (k) were computed directly from these trends.

2. The magnitude of the absorptive capacity parameter (β) is indicated by the highest compound growth of investment for any five-year period in the past decade. The growth of investment (i) observed for 1957–62 is often well below this limit because development has been constrained by other factors.

3. Trend values for 1962 of the investment, saving, and import ratios to GNP are computed as a basis for future projections.

The estimates in Table A-1 will first be used to determine the extent to which recent performance satisfies the criteria for a successful transition to self-sustaining growth. While six years is too short a period to establish reliable estimates for any single country, a comparative assessment for the whole group of countries is quite suggestive.

We have proposed three sets of criteria in Section I to measure progress toward a given rate of self-sustaining growth;

(1) *Investment criteria.* In Phase I, the rate of growth of investment must be greater than the target growth rate ($i>\bar{r}$). Thereafter, the investment rate must be adequate to sustain the target GNP growth rate ($I/V\geq k\bar{r}$).

(2) *Saving criteria.* The marginal savings rate must be greater than the target investment rate ($\alpha'>k\bar{r}$) unless the average rate of saving is already above this level.

(3) *Trade criteria.* Either export growth must exceed the growth target for GNP or the marginal import ratio (μ') must be substantially less than the initial average ratio. The complete statement of the trade criterion is given by equation (16).

These criteria have been used to classify the 31 countries into the four main groups shown in Table 6. The classification is based on the saving and trade performance needed to achieve a self-sustaining growth rate of 5 per cent.[47] As to the investment criteria, all countries except Burma have shown an absorptive capacity for investment of greater than 5 per cent in the recent past, but the five countries indicated by an asterisk do

[47] The parameters should reflect underlying structural characteristics rather than "realized" values in this historical period. The high marginal import ratios for Chile, Nigeria, and Turkey, for example, may reflect Phase II slackness in the foreign trade constraint. The true structural import ratios may be enough lower to move Chile from Group D to C and Nigeria from B to A. Use of parameters for the period 1953–63 would move Mexico from Group C to A.

TABLE A-1—SUMMARY OF PAST STRUCTURAL RELATIONSHIPS FOR LESS DEVELOPED COUNTRIES[a]

No.[a]	Country	Highest 5 Years in Recent Past[b]		Relationships during 1957–62[b]										
		β	Period	i	k	r	I_0	Φ_0	α_0	α'	μ_0	μ'	ϵ	ρ'
3	Greece	.210	1954–58	.12	3.02	.060	.21	−.06	.15	.26	.18	.19	.051	.097
4	India	.154	1953–57	.07	2.89	.048	.14	.02	.12	.20	.07	.00	.014	−.165
5	Iran	.103	1957–61	.01	3.54	.049	.15	.01	.14	.11	.23	.24	.080	−.044
6	Israel	.129	1953–57	.10	3.07	.103	.31	−.20	.11	.15	.41	.48	.194	.541
7	Jordan	.215	1958–62	.19	1.36	.111	.17	−.24	−.07	.09	.41	.29	.080	.288
8	Pakistan	.198	1960–64	.15	2.35	.041	.12	−.04	.09	.25	.10	.20	.075	−.007
9	Turkey	.102	1959–63	.04	4.79	.030	.15	.03	.12	−.02	.11	.33	.050	.049
11	Argentina	.113	1956–60	.09	10.67	.019	.24	.03	.21	.83	.15	.41	.043	.072
12	Bolivia	.084	1960–64	−.02	4.32	.029	.11	−.07	.04	−.16	.17	.06	−.005	−.029
13	Brazil	.154	1956–60	.09	2.64	.067	.19	.03	.15	.19	.13	.00	−.023	−.001
15	Chile	.169	1958–62	.12	2.77	.038	.13	−.06	.07	.10	.22	.46	.061	−.057
16	Colombia	.078	1958–62	.04	4.15	.050	.20	−.04	.16	−.12	.16	.20	−.031	−.053
17	Costa Rica	.108	1950–54	−.01	4.72	.039	.16	−.05	.11	−.10	.28	.17	.016	−.033
20	Guatemala	.208	1953–57	−.05	3.52	.036	.10	.02	.08	−.03	.14	−.09	.014	−.148
21	Honduras	.155	1960–64	.01	4.05	.033	.13	−.01	.13	.25	.19	−.03	.028	−.074
23	Mexico	.144	1953–57	.02	3.06	.050	.14	.01	.13	.11	.12	.06	.051	−.032
25	Panama	.144	1958–62	.10	3.12	.051	.18	−.06	.12	.37	.39	.51	.100	−.114
26	Paraguay	.100	1951–55	.01	6.37	.026	.16	.03	.13	.08	.17	.11	.025	.001
27	Peru	.155	1959–63	.03	3.10	.073	.20	−.01	.21	.31	.24	.21	.143	.105
28	Trinidad-Tobago	.192	1955–59	.05	4.33	.078	.31	.10	.22	.11	.83	1.04	.107	(n.a.)

29	Venezuela	.056	1955–59	−.08	6.53	.043	.19	−.08	.27	−.26	.33	−1.13	−.065	−.691
34	Liberia	.446	1958–62	.57	7.81	.046	.67	−.56	.11	.21	1.13	3.23	.033	.067
36	Nigeria	.051	1956–60	.09	3.71	.033	.14	−.05	.09	.19	.20	.39	.059	−.594
40	Tunisia	.275	1958–62	.27	4.91	.034	.26	−.18	.08	−.84	.42	1.10	−.086	.350
42	Burma	0	1957–61	−.00	4.10	.046	.16	−.00	.17	.21	.16	−.15	.021	.095
43	Taiwan	.164	1956–60	.13	2.68	.074	.22	−.07	.15	.29	.21	.19	.083	.078
45	Korea	.187	1960–64	.00	3.44	.040	.12	−.10	.03	.27	.16	−.06	.165	.216
46	Philippines	.078	1958–62	.05	2.78	.050	.14	.02	.12	.30	.20	.01	.046	−.093
47	Thailand	.126	1958–62	.10	2.11	.080	.17	.01	.16	.22	.19	.15	.077	.226
49	Malaya	.157	1957–61	.18	2.33	.062	.18	−.04	.22	.26	.47	.62	.059	.926
50	Mauritius	.143	1956–60	.08	4.97	.034	.19	−.09	.10	−.39	.49	.67	−.010	(n.a.)

[a] All data derived by fitting time trends to actual points (as estimated in early 1965) for period covered. All data except imports, exports, and reserves expressed in 1962 prices. Imports, exports and reserves expressed in current U. S. dollars.

[b] Symbols for column headings are:

βi = compound growth rate of gross investment.

k = incremental capital-output ratio (assuming 1-year lag).

r = compound growth rate of GNP.

I_0 = ratio of gross investment to GNP in 1962 (after time-trend fitting).

Φ_0 = ratio of net foreign capital inflow to GNP in 1962 (after time-trend fitting).

α_0 = ratio of national gross savings to GNP in 1962 (after time-trend fitting).

α' = marginal national savings ratio (change in savings ÷ change in GNP)

μ_0 = ratio of gross imports of goods and services to GNP in 1962 (after time-trend fitting).

μ' = marginal import ratio (change in gross imports of goods and services ÷ change in GNP).

ϵ = compound growth rate of exports of goods and services.

ρ' = change in gold and convertible foreign currency reserves, December 1956 to December 1962 ÷ change in GNP, 1957–62. GNP first converted to 1962 U. S. dollars (after time-trend fitting).

[c] Country numbers correspond to those in Tables A-2 and A-3.

Source: Imports, exports and reserves largely from IMF, *Balance of Payments Yearbook*. Other data from U. N. *Yearbook of National Accounts* and from AID, Statistics and Reports Division.

Table 6—Indicators of Progress in Attaining Self-Sustaining Growth, 1957–62

No.	Country	Capital Inflow	Investment Performance			Saving Performance		Trade Performance			Growth in GNP
		F_0/V_0	$k\bar{r}$	I_0/V_0	i	α_0	α'	E_0/M_0	ϵ	μ'/μ_0	r
	A. Countries meeting both saving and trade criteria[a]										
42	Burma*	.00	.205	.16	−.003	.17	.21	1.01	.021	−.91	.046
6	Israel	.20	.154	.31	.10	.11	.15	.51	.194	1.16	.103
7	Jordan	.24	.068	.17	.19	−.07	.09	.43	.080	.70	.111
45	Korea*	.10	.172	.12	.001	.03	.27	.42	.165	.39	.040
49	Malaya	−.04	.116	.18	.18	.22	.26	1.08	.059	1.31	.062
8	Pakistan	.04	.117	.12	.15	.09	.25	.64	.075	1.97	.041
25	Panama	.06	.156	.18	.10	.12	.37	.85	.100	1.31	.051
27	Peru	−.01	.155	.20	.03	.21	.31	1.04	.143	.86	.073
46	Philippines	.02	.139	.14	.05	.12	.30	.90	.046	.06	.050
43	Taiwan	.07	.134	.22	.13	.15	.29	.65	.083	.90	.074
47	Thailand	.01	.106	.17	.10	.16	.22	.93	.077	.82	.080
28	Trinidad-Tobago	.10	.217	.31	.05	.22	.11	.88	.107	1.25	.078
	B. Countries meeting saving criterion only[a]										
11	Argentina	.03	.533	.24	.09	.21	.83	.80	.043	2.66	.019
13	Brazil	.03	.132	.19	.09	.15	.19	.74	−.023	.03	.067
3	Greece	.06	.151	.21	.12	.15	.26	.67	.051	1.05	.060
21	Honduras*	−.01	.203	.13	.01	.13	.25	1.03	.028	−.18	.033
4	India	.02	.145	.14	.07	.12	.20	.68	.014	.07	.048
36	Nigeria	.05	.185	.14	.09	.09	.19	.76	.059	1.98	.033
	C. Countries meeting trade criterion only[a]										
5	Iran	.01	.177	.15	.01	.14	.11	.95	.080	1.04	.049
23	Mexico	.01	.153	.14	.02	.13	.11	.91	.051	.54	.050
29	Venezuela	−.08	.326	.19	−.08	.27	−.26	1.25	−.065	−3.41	.043

D. Countries meeting neither trade nor saving criterion										
12 Bolivia*	.07	.216	.11	−.02	.04	−.16	.62	−.005	.34	.029
15 Chile	.06	.138	.13	.12	.07	.10	.73	.061	2.12	.038
16 Colombia	.04	.208	.20	.04	.16	−.12	.76	−.031	1.21	.050
17 Costa Rica	.05	.236	.16	−.01	.11	−.10	.83	.016	.60	.039
20 Guatemala*	.02	.176	.10	−.05	.08	−.03	.86	.014	−.64	.036
34 Liberia	.56	.390	.67	.57	.11	.21	.50	.033	2.85	.046
50 Mauritius	.09	.249	.19	.08	.10	−.39	.81	−.010	1.37	.034
26 Paraguay	.03	.318	.16	.01	.13	.08	.81	.025	.69	.026
40 Tunisia	.18	.245	.26	.27	.08	−.84	.59	−.086	2.59	.034
9 Turkey	.03	.240	.15	.04	.12	−.02	.72	.050	2.95	.030

Source: Table A-1.

Symbols:
- r = GNP growth rate
- $k\bar{r}$ = ratio of investment to GNP needed for 5 per cent GNP growth rate
- I_0/V_0 = investment/GNP ratio in 1962
- i = annual growth rate of investment
- α_0 = 1962 ratio of saving to GNP
- α' = marginal saving/GNP ratio
- E_0/M_0 = 1962 ratio of exports to imports
- ϵ = export growth rate
- μ'/μ_0 = ratio of marginal to average import/GNP coefficients
- F_0/V_0 = ratio of capital inflow to GNP in 1962 $(I_0/V_0-\alpha_0)$,

ᵃ *Criteria:*

(a) Saving criteria: $\alpha' \geq k\bar{r}$, or $\alpha_0 \geq k\bar{r}$, where $\bar{r}=.05$

(b) Trade criteria:

$$\frac{\mu'}{\mu_0} \leq \frac{(E_0/M_0)(1+\epsilon)^p - 1}{(1+\bar{r})^p - 1}, \text{ for some } p \leq 50 \text{ years where } \bar{r}=.05$$

(c) Minimum investment criteria: The initial investment rate of the countries marked by an asterisk is insufficient to maintain a 5 per cent GNP growth rate, even if the capital-output ratio were to fall to 3.0 (i.e. $I_0/V_0<.15$), *and* the rate of increase in investment is insufficient to ever achieve a 5 per cent GNP growth rate (i.e., $i<.05$).

* Do not meet either set of investment criteria.

not meet either investment criterion for the period 1957–62.

The 12 countries in group A satisfy the criteria for approaching or maintaining self-sufficiency and nine of them have already achieved growth rates of 5 per cent or more. Half of this group is heavily dependent on external capital for its continued growth, while most of the others have favorable exports and little or no net capital inflow.

Of the 19 countries that fail to satisfy one or both criteria for approaching self-sustaining growth, failure on the trade side seems to be at least as important as deficiencies in saving and investment. More detailed studies suggest that a number of these countries—India, Greece, Turkey, Chile, Colombia, Costa Rica, Bolivia, Guatemala, and Honduras—have recently shown symptoms associated with Phase III in our model: import shortages, substantial excess capacity, and in some cases falling saving rates.[48]

One of the most suggestive features of this grouping of countries is the predominant role played by exports. Ten of the 12 countries in group A have export growth rates of 6 per cent or more and hence could eventually reach self-sustaining growth of 5 per cent even if the ratio of imports to GNP remained constant. Conversely, one of the most significant aspects of the unsatisfactory performance of countries in Group D is the stagnation of their exports, which has typically led to increased requirements for external capital and falling saving rates. There is almost no example of a country which has for a long period sustained a growth rate substantially higher than its growth of exports through continuing import substitution. In the past Brazil, Colombia, Turkey, and India have done so for considerable periods, but each has run into severe balance-of-payments difficulties in recent years.

This comparative assessment also tends to dispel the notion that performance as measured here is necessarily associated with the initial income level. In this period, at least, there is little correlation between initial income levels and either marginal saving rates or balance-of-payments performance.

B. *Projections of Future Growth*

Since less developed countries vary widely in their ability to mobilize their own resources and to utilize external resources, estimates of future assistance requirements based on aggregate models are not very useful. We have therefore made a series of projections for each of 50 countries in order to explore the range of future growth possibilities and corresponding assistance requirements. While the projection for any single country is fairly crude, this approach has the great advantage of taking into

[48] Aggregate evidence is given in [8]; examples of more detailed analyses of the trade gap are found in [1] [14] [24] [25].

account absorptive capacity, import requirements, and other limitations which can only be judged on a country basis.

Our analysis is designed to explore the possibilities for accelerating growth through a combination of improved country performance and additional external resources. We therefore specify a considerable range of performance possibilities, based on the preceding survey of current performance. The range of values chosen for each parameter is designed to show the extent to which the performance variables affect the country's growth and its aid requirements.

1. *Methodology.* The methodology to be used follows closely that used for Pakistan in Section I. A similar range of variation in performance has been specified for each of the 50 countries in the sample. Principal attention has been given to the 25 countries having the largest effect on assistance requirements.

As a starting point we estimated the six parameters in model 1 from the historical performance in each country, modified in some cases by the experience of similar countries. The average of the resulting target growth rates for all countries (projected to 1975) is 4.4 per cent, approximately the same as the recent past.[49]

To evaluate the possibilities for accelerated growth, we divided the six policy parameters into three groups: the growth limits for investment and GNP (β and $\bar{r}$); internal performance factors (k, α', and μ'); and export growth (ϵ). Starting from the historical estimates, we specified two sets of more optimistic assumptions for growth limits and internal performance factors and one alternative set of export projections.[50] These alternative sets of parameters values are shown in Table A-2. The possible combinations of the sets of values for the parameters provide a basis for 18 projections for each of the 50 countries.

In judging the range of possible performance for each country, we took into account its historical performance, its development plan, the observed performance of other countries and some aspects of political performance. We relied heavily on the development programs of the major countries in making the intermediate or "plan" estimates of both growth targets and internal performance. "Plan" targets and performance are defined here as those achievable with moderate improvements in development policies in relation to past experience. The most optimistic (upper-limit) estimates assumed that almost all countries could attain the median observed value of the marginal saving rate (.20) and

[49] These historically based estimates are shown in Table A-2 of the Annex. They differ from the estimates in Table A-1 for 1957-62 primarily in the elimination of abnormal or biased values that need not persist with reasonable policies—e.g., falling export and saving rates, abnormally high capital coefficients, etc. In large part, these abnormal values represent the effects of disequilibrium conditions on our estimates. The revisions reduce the estimates of aid requirements.

[50] Details are given in [8].

TABLE A-2—VALUE OF PARAMETERS USED IN PROJECTIONS

No.	Country	Target Growth Rate of GNP ($\bar{r}$)			Maximum Rate of Growth of Investment			Incremental Aggregate Capital-Output Ratio			Marginal Gross Savings Ratio			Marginal Import Ratio			Annual Growth Rates: Exports		Annual Growth Rates: Population
		(H)istorical	(P)lan	(U)pper Limit	H	P	U	A	B	C	A	B	C	(a)	(b)	(c)	1	2	(1963)
	Near East																		
2	Cyprus	.009	.030	.050	.060	.060	.070	5.00	4.00	3.50	.140	.190	.230	.470	.470	.410	.0088	.0116	.017
3	Greece	.060	.065	.070	.100	.100	.100	3.10	3.10	3.10	.230	.230	.250	.190	.190	.180	.0544	.0712	.009
5	Iran	.044	.055	.065	.060	.070	.090	3.70	3.70	3.50	.140	.150	.250	.232	.232	.180	.0544	.0712	.025
6	Israel	.090	.090	.100	.120	.120	.150	3.19	3.00	3.00	.220	.300	.300	.400	.400	.300	.1122	.1468	.035
7	Jordan	.056	.056	.080	.160	.160	.160	3.37	3.37	3.37	.200	.200	.250	.370	.370	.330	.0571	.0748	.027
9	Turkey	.053	.060	.070	.080	.080	.090	2.91	2.91	2.91	.200	.256	.256	.110	.170	.110	.0306	.0400	.029
10	U.A.R.	.045	.055	.060	.050	.070	.080	2.68	2.68	2.68	.150	.170	.200	.200	.200	.150	.0136	.0178	.025
	South Asia																		
1	Ceylon	.042	.050	.060	.095	.100	.100	3.24	3.24	3.24	.110	.150	.200	.220	.220	.190	.0177	.0231	.028
4	India	.043	.053	.065	.100	.100	.100	3.20	3.20	3.20	.180	.210	.250	.070	.070	.050	.0204	.0267	.024
8	Pakistan	.045	.053	.060	.130	.130	.130	3.00	3.00	3.00	.160	.240	.240	.100	.150	.100	.0374	.0489	.026
	Latin America																		
11	Argentina	.031	.043	.055	.150	.150	.150	7.21	5.30	4.30	.220	.220	.250	.070	.170	.020	.0286	.0374	.017
12	Bolivia	.033	.045	.056	.060	.080	.080	4.00	4.00	4.00	.100	.150	.200	.220	.220	.220	.0068	.0089	.023
13	Brazil	.055	.055	.070	.080	.080	.080	2.90	2.90	2.50	.270	.270	.280	.090	.090	.070	.0286	.0374	.031
14	British Guiana	.029	.040	.050	.100	.100	.100	5.00	5.00	5.00	.200	.250	.330	.470	.470	.470	.0544	.0712	.030
15	Chile	.035	.050	.055	.060	.080	.100	3.40	3.40	3.00	.120	.160	.200	.120	.190	.120	.0190	.0249	.023
16	Colombia	.050	.061	.070	.060	.080	.100	4.80	4.80	4.80	.200	.260	.300	.200	.230	.170	.0272	.0356	.029
17	Costa Rica	.055	.060	.069	.060	.080	.100	3.27	3.27	3.27	.130	.200	.250	.280	.280	.280	.0354	.0463	.039
18	Ecuador	.042	.050	.055	.060	.080	.080	3.74	3.74	3.74	.140	.200	.240	.206	.206	.206	.0340	.0445	.030
19	El Salvador	.050	.060	.065	.060	.100	.100	2.50	2.50	2.50	.110	.180	.210	.268	.268	.210	.0374	.0489	.027
20	Guatemala	.040	.050	.055	.060	.080	.080	4.67	3.50	3.50	.150	.200	.250	.149	.149	.149	.0340	.0445	.030
21	Honduras	.037	.045	.050	.064	.070	.080	3.90	3.50	3.50	.120	.150	.200	.195	.195	.195	.0190	.0249	.030
22	Jamaica	.040	.045	.055	.060	.080	.100	4.00	3.50	3.50	.160	.180	.200	.206	.206	.206	.0340	.0445	.013

23	Mexico	.050	.060	.070	.067	.080	.100	2.52	2.52	2.52	.170	.170	.220	.110	.110	.100	.0537	.0703	.031
24	Nicaragua	.042	.050	.055	.063	.080	.080	3.72	3.72	3.72	.150	.200	.220	.281	.281	.281	.0340	.0445	.034
25	Panama	.050	.050	.060	.144	.140	.140	2.50	2.50	2.50	.120	.200	.200	.385	.385	.350	.0143	.0187	.030
26	Paraguay	.020	.030	.040	.060	.140	.140	5.00	4.00	4.00	.130	.130	.150	.249	.249	.249	.0054	.0071	.022
27	Peru	.055	.055	.070	.100	.100	.100	4.94	4.94	4.77	.285	.285	.285	.240	.240	.200	.0524	.0685	.023
28	Trinidad-Tobago	.050	.060	.088	.090	.100	.100	3.65	3.65	3.65	.200	.250	.250	.700	.500	.300	.0952	.1246	.030
29	Venezuela	.045	.060	.070	.080	.080	.100	3.64	3.64	3.64	.290	.290	.290	.314	.180	.160	.0211	.0276	.034
	Africa																		
30	Algeria	.020	.035	.050	.060	.060	.060	3.30	3.30	3.30	.060	.100	.200	.200	.200	.090	.0272	.0356	.025
31	Ethiopia	.045	.045	.050	.150	.150	.150	2.50	2.50	2.50	.140	.170	.200	.116	.116	.116	.0544	.0712	.014
32	Ghana	.045	.055	.060	.098	.098	.098	3.70	3.50	3.20	.130	.150	.200	.220	.220	.220	.0156	.0205	.025
33	Kenya	.017	.035	.050	.060	.060	.060	5.00	4.00	4.00	.120	.150	.200	.266	.266	.040	.0340	.0445	.030
34	Liberia	.057	.060	.060	.150	.150	.150	5.00	5.00	5.00	.110	.150	.200	.573	.573	.573	.0422	.0552	.015
50	Mauritius	.034	.034	.034	.083	.080	.080	4.97	4.97	4.97	.080	.080	.080	.456	.456	.456	.0000	.0000	.032
35	Morocco	.028	.040	.060	.050	.060	.070	5.00	4.00	3.50	.130	.150	.200	.150	.150	.150	.0204	.0267	.027
36	Nigeria	.040	.045	.050	.082	.082	.082	3.80	3.80	3.80	.090	.110	.200	.280	.280	.280	.0544	.0712	.020
37	Rhodesia-Nyasaland	.043	.040	.045	.060	.060	.060	5.00	5.00	5.00	.160	.180	.200	.513	.513	.220	.0755	.0988	.028
38	Sudan	.051	.055	.055	.140	.140	.140	2.50	2.50	2.50	.110	.150	.200	.220	.220	.190	.0612	.0801	.028
39	Tanganyika	.042	.050	.056	.060	.060	.080	2.93	2.93	2.93	.110	.150	.200	.188	.188	.100	.0333	.0436	.020
40	Tunisia	.041	.050	.060	.150	.150	.150	4.62	4.00	4.00	.150	.200	.250	.260	.190	.190	.0340	.0445	.021
41	Uganda	.017	.040	.050	.060	.060	.080	5.00	4.00	4.00	.110	.150	.200	.168	.168	.090	.0272	.0356	.025
	Far East																		
42	Burma	.032	.040	.050	.060	.060	.060	5.00	4.00	4.00	.160	.180	.200	.177	.177	.177	.0782	.1023	.022
44	Indonesia	.010	.030	.040	.010	.035	.050	2.75	2.75	2.75	.050	.100	.150	.070	.070	.070	.0109	.0142	.023
45	Korea, South	.043	.050	.060	.050	.060	.080	3.27	3.27	3.27	.100	.150	.200	.240	.260	.180	.0578	.0756	.029
49	Malaya	.040	.050	.060	.119	.120	.120	2.52	2.50	2.50	.190	2.00	.200	.419	.419	.419	.0211	.0276	.032
46	Philippines	.050	.055	.060	.051	.060	.070	2.58	2.58	2.58	.260	.260	.260	.170	.170	.170	.0313	.0409	.032
43	Taiwan	.060	.070	.080	.133	.133	.133	2.62	2.62	2.62	.210	.210	.250	.205	.190	.190	.0544	.0712	.029
47	Thailand	.050	.060	.065	.091	.091	.091	2.50	2.50	2.50	.250	.250	.250	.160	.160	.150	.0462	.0605	.031
48	South Vietnam	.029	.035	.040	.060	.060	.060	3.69	3.70	3.70	.000	.100	.150	.217	.217	.217	.0252	.0329	.028

TABLE 7—PROPORTION OF COUNTRIES WITH FOREIGN CAPITAL REQUIREMENTS DETERMINED BY INVESTMENT-SAVING GAPS

GNP Growth Targets	Internal Performance Characteristics					
	Historical		Plan		Upper Limit	
	Low Exports	High Exports	Low Exports	High Exports	Low Exports	High Exports
1965						
Historical	28%	40%	22%	24%	18%	24%
Plan	52	62	32	46	34	44
Upper Limit	72	80	54	70	48	58
1975						
Historical	32	40	20	34	18	24
Plan	38	58	24	36	18	30
Upper Limit	50	68	30	48	22	40

Source: AID, Office of Program Coordination, "23-Year Projections" of September 16, 1964, for model 1, 50-country sample.

could limit the marginal import coefficient to the normal value derived from intercountry comparisons.

Our notion of the upper limit implies a probability of perhaps one in four that the given target growth and performance could be attained. For all countries, the average of the plan growth targets through 1975 turns out to be 5.2 per cent and the average of the upper-limit targets is about 6 per cent. The "plan" estimates range from 3–9 per cent with a heavy concentration between 5 per cent and 7 per cent.[51]

In order to explore the range of growth possibilities systematically, we have adopted the same degree of optimism for all countries in each trial calculation. Projections on this basis are designed to reveal the range of possibilities that is interesting for policy purposes rather than to forecast the most probable course of development in each country. The projections were made from year to year according to the formulas of the appropriate phase in model 1.[52] Cumulative results for the 18 combinations of growth targets, country performance, and exports are given in Table 8 and regional projections for 1970 and 1975 in Table 9.

The projections based on model 1 include measures of excess consumption and excess imports, which show the extent to which aid requirements could be reduced through policies designed to equalize the

[51] Whatever the validity of our subjective judgments as to the possibility of improved performance, this procedure has seemed preferable to a more mechanical approach to testing the sensitivity of the results to various types of change. Our principal conclusions are not greatly affected by differences in judgment as to the possibilities for individual countries.

[52] Machine computations involve a test in each year to determine the appropriate growth phase and set of equations to apply for the next year.

TABLE 8—AGGREGATE PROJECTIONS FOR 1962–75
(All figures in billions of 1962 U. S. dollars; cumulative values include the years 1962 through 1975[a])

Line No.		Historical Country Performance			Plan Country Performance			Upper Limit Country Performance		
		Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets	Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets	Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets
1	1975 Gross National Product	297	327	354	297	328	356	298	329	360
2	(Implicit GNP growth rate)	(.044)	(.051)	(.058)	(.044)	(.052)	(.058)	(.044)	(.052)	(.059)
	Cumulative values of variables									
3	Exports: Low growth (3.8% per year)	441	441	441	441	441	441	441	441	441
4	Exports: High growth (5.2% per year)	480	480	480	480	480	480	480	480	480
5	Gross National Product	3,186	3,356	3,485	3,188	3,363	3,502	3,195	3,373	3,522
6	Gross Investment	476	591	703	461	572	684	448	557	670
7	National Savings (potential)	(475)	(502)	(526)	(495)	(528)	(558)	(521)	(562)	(598)
8	National Savings, realized	365	435	491	353	430	505	364	451	538
9	Imports (potential)	(533)	(561)	(581)	(533)	(562)	(583)	(509)	(541)	(560)
10	Imports, realized	552	596	652	548	582	620	525	547	573

For footnotes see end of table.

TABLE 8—(*Continued*)

Line No.		Historical Country Performance			Plan Country Performance			Upper Limit Country Performance		
		Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets	Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets	Historical Growth Targets	Plan Growth Targets	Upper Limit Growth Targets
11	Unrealized Savings (line 7–8)	110	67	35	142	98	53	157	111	60
12	Excess Imports (line 10–9)	19	35	71	15	20	36	16	6	23
13	Total Unrealized Savings and Excess Imports (line 11+12)	129	101	106	157	118	89	173	117	82
14	Net Capital Inflow: Low exports	111	156	212	108	142	179	84	106	133
15	Net Capital Inflow: High exports	90	138	201	83	119	164	60	84	116
	Capital Inflow, excluding countries with net capital outflow:[b]									
16	Low exports	120	165	220	106	152	187	94	117	141
17	High exports	105	150	211	100	131	173	85	99	125
18	Consumption: Low exports	2,821	2,920	2,995	2,835	2,933	2,997	2,831	2,922	2,984
19	Consumption: High exports	2,800	2,903	2,984	2,811	2,909	2,981	2,807	2,899	2,967

Source: Agency for International Development, Office of Program Coordination, machine listings of September 16, 1964.
[a] See Table A-2 for values of parameters used and Table A-3 for initial values of variables used.
[b] Foreign resource flows are measured on a net basis. In any particular year most net flows are capital inflows, but some countries (e.g., Venezuela, Malaya, Burma) may have estimated potential capital outflows under the assumptions made. This alternative net capital estimate shown here excludes these potential capital outflows.

TABLE 9—REGIONAL PROJECTIONS, 1970 AND 1975
(All values in billions of 1962 U. S. dollars)

	1962	1970[a]				1975[a]				Per cent of Annual Growth, 1962–1975[a]				
Targets and Performance		Histori-cal	Plan		Upper Limit	Histori-cal	Plan		Upper Limit	Histori-cal	Plan			Upper Limit
Exports		Low	Low	High	High	Low	Low	High	High	Low	Low	Low	High	High
Near East (7 countries)[b]														
GNP	20.94	32.05	33.22	33.22	31.48	42.12	44.92	44.92	48.55	5.5%		6.0%		6.7%
(% of GNP Total)	(78%)													
Investment	3.76	5.56	6.27	6.27	7.27	7.46	8.70	8.70	10.63	5.4%		6.7%		8.3%
Savings	2.51	3.26	3.45	3.92	5.18	4.35	4.48	5.49	7.73					
Imports	4.61	7.28	7.80	8.02	7.76	9.71	10.82	11.53	11.23					
Exports	3.34	4.98	4.98	5.67	5.67	6.60	6.60	8.32	8.32	5.4%	5.4%		7.3%	7.3%
Foreign Resources[c]	1.26	2.30	2.82	2.35	2.09	3.11	4.22	3.21	2.90	7.2%	9.7%		7.5%	6.6%
South Asia (3 countries)														
GNP	46.22	64.83	69.52	69.52	73.45	80.13	89.96	89.96	100.20	4.3%		5.3%		6.1%
(% of GNP Total)	(97%)													
Investment	7.57	8.89	11.66	11.66	14.93	10.99	15.08	15.08	20.37	2.9%		5.4%		7.9%
Savings	6.46	6.85	9.08	9.26	12.83	8.12	11.12	11.47	17.27					
Imports	3.73	5.20	5.74	5.74	5.45	6.42	7.52	7.52	7.02					
Exports	2.63	3.16	3.16	3.35	3.35	3.56	3.56	3.91	3.91	1.0%	1.0%		3.1%	3.1%
Foreign Resources[c]	1.10	2.04	2.58	2.39	2.10	2.86	3.96	3.61	3.11	7.6%	10.4%		9.6%	8.3%
Latin America (19 countries)														
GNP	62.64	88.60	93.44	93.44	98.56	111.01	121.56	121.56	134.42	4.5%		5.2%		6.1%
(% of GNP Total)	(95%)													
Investment	11.05	15.04	17.61	17.61	20.19	18.85	23.13	23.13	28.49	4.2%		5.8%		7.5%
Savings	10.26	13.28	14.95	15.82	18.39	16.13	18.55	20.24	25.92					
Imports	11.00	15.23	16.12	16.48	16.50	18.92	20.79	21.74	21.40					
Exports	10.20	13.46	13.46	14.69	14.69	16.19	16.19	18.83	18.83	3.6%	3.6%		4.8%	4.8%
Foreign Resources[c]	1.55[e]	2.21[e]	3.27[e]	2.58[e]	2.27[e]	2.87[e]	5.00[e]	3.80[e]	3.23[e]	4.9%	9.4%		7.1%	5.6%

For footnotes see end of table.

TABLE 9—(*Continued*)

Targets and Performance	1962	1970[a]				1975[a]				Per cent of Annual Growth, 1962–1975[a]				
		Histori-cal	Plan		Upper Limit	Histori-cal	Plan		Upper Limit	Histori-cal		Plan		Upper Limit
Exports		Low	Low	High	High	Low	Low	High	High	Low	Low	Low	High	High
Africa (13 countries)														
GNP	17.04	22.28	23.58	23.58	24.56	26.56	29.11	29.11	31.36	3.5%		4.2%		4.8%
(% of Total GNP)	(73%)													
Investment	2.69	2.97	3.61	3.61	4.25	3.63	4.55	4.55	5.59	2.3%		4.1%		5.8%
Savings	1.58	1.65	2.07	2.20	2.85	2.13	2.68	2.94	4.07					
Imports	5.49	7.42	7.63	8.16	8.12	9.09	9.45	10.68	10.59					
Exports	4.38	6.08	6.08	6.75	6.75	7.59	7.59	9.07	9.07	4.3%	4.3%		5.8%	5.8%
Foreign Resources[c]	1.11	1.36*	1.53	1.40	1.40*	1.53*	1.86*	1.61*	1.58*	2.5%	4.0%		2.9%	2.8%
Far East (8 countries)														
GNP	23.68	30.92	33.53	33.53	34.68	36.91	42.07	42.07	44.97	3.5%		4.5%		5.1%
(% of Total GNP)	(86%)													
Investment	3.11	3.11	4.28	4.28	5.01	3.86	5.50	5.50	6.85	1.7%		4.5%		6.3%
Savings	2.26	1.80	2.65	3.00	3.57	2.14	3.10	3.79	4.76					
Imports	4.80	6.41	6.73	6.82	6.98	7.78	8.46	8.70	9.08					
Exports	3.95	5.10	5.10	5.54	5.54	6.06	6.06	6.99	6.99	3.3%	3.3%		4.5%	4.5%
Foreign Resources[c]	.95*	1.31	1.64*	1.31*	1.45*	1.71*	2.41*	1.78*	2.11*	4.6%	7.4%		4.9%	6.3%
50-Country Total														
GNP	170.52	238.68	253.31	253.31	262.72	296.73	327.62	327.62	359.50	4.4%		5.2%		5.9%
(% of All LDC's GNP)[d]	(89%)													
Investment	28.18	35.59	43.45	43.45	51.65	44.79	56.96	56.96	71.93	3.6%		5.6%		7.5%
Savings	23.07	26.84	32.20	34.20	42.82	32.87	39.93	49.93	59.75	2.8%	4.3%		5.1%	7.6%
Imports	29.63	41.54	44.02	45.22	44.81	51.92	57.04	60.17	59.32	4.4%	5.2%		5.6%	5.5%
Exports	24.50	32.81	32.81	36.00	36.00	40.00	40.00	47.12	47.12	3.8%	3.8%		5.2%	5.2%
Foreign Resources[c]	5.97*	9.22*	11.84*	10.03*	9.31*	12.08*	17.45*	14.01*	12.93*	5.6%	7.8%		6.6%	5.9%

[a] See Table 8 for sources and concepts. Column headings identify both targets and performance standards.
[b] Excludes the oil-producing countries except for Iran. Includes Greece, Turkey, and the UAR.
[c] The figures shown here exclude potential capital outflows (see Table 8, note b); regional totals are denoted by an asterisk (*) if they include such a case.
[d] Excludes Cuba, China, North Korea.

two resource gaps. Since the empirical possibilities for such policies cannot be ascertained without detailed studies of each country, we will apply the over-all factors derived in Section I to estimate the reduction in capital inflow that might be achieved in this way.

2. *The Phases of Growth.* The projection of growth paths under alternative assumptions provides a more general evaluation of the relative importance of the two resource limitations than does our attempted identification of these limits in current situations. Table 7 shows the proportion of the 50 countries in which the saving-investment gap was the limiting factor—and hence the determinant of capital inflow—in each of the 18 trial projections. The most striking result of this tabulation is the predominance of the trade limit; it is more important than the saving limit in 1975 in 15 of the 18 sets of alternatives.

This breakdown shows the quantitative significance of three factors that have been discussed previously in general terms.

(1) At higher growth rates the saving limit tends to become more important, for reasons analyzed in Section I. Under most assumptions as to the other parameters, a rise in the growth rate from the historical average of 4.4 per cent to the upper-limit average of about 6 per cent increases the number of countries in which the saving limit is controlling by 50 per cent or more.

(2) The saving limit is increasingly dominated over time by the trade limit under historical conditions of internal performance. This result points to the need for more import substitution unless export prospects can be drastically improved.

(3) A 40 per cent increase in the assumed rates of growth of exports (from the low to the high assumptions) removes the trade limit in only four to six of the 50 countries under most assumptions. Unrealistically large increases in exports would be required to reduce greatly the importance of the balance of payments limitation by 1975.

C. *Development Performance and Assistance Needs*

The projections in Table 8 are designed to show the way in which assistance need vary with the export possibilities and internal performance of the developing countries. For this purpose the 900 separate country projections have been aggregated using the same degree of optimism as to exports and internal performance for each country. To summarize the results graphically, the 18 aggregate solutions of Table 8 are plotted in Figure 4, giving three points on each of the six curves. Curve *P*2, for example, shows the increase in cumulative capital inflow from $100 billion ($7.7 billion per year) needed to sustain an average growth rate of 4.4 per cent to $173 billion ($13 billion per year) to sustain a growth rate of 5.8 per cent, assuming plan performance and high exports.

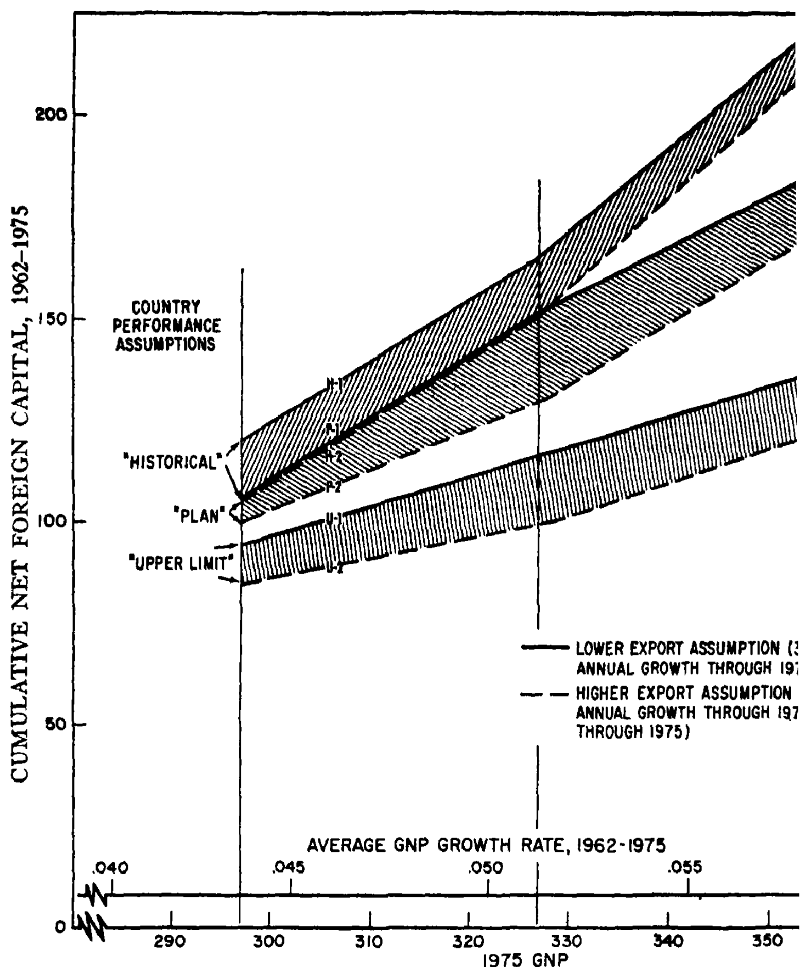


FIGURE 4. FOREIGN CAPITAL REQUIREMENTS OF 50 DEVELOPING COUNTRI
ALTERNATIVE DEVELOPMENT PATTERNS, 1962–75
(All values in billions of 1962 U.S. dollars)

Source: Table 8.

The graphical presentation facilitates analysis of the produc
external assistance under alternative assumptions. Along curv
increase of a billion dollars of GNP in 1975 can be secured for a
tive aid input of $1.2–$1.3 billion between 1962 and 1975.[53] The
tivity of aid is significantly lower with historical performance
nificantly higher with upper-limit performance, as shown by the

[53] An alternative calculation would show a cumulative addition to GNP over th
nearly five dollars per dollar of capital inflow. There is some decline in aid prod
higher growth rates due to the shifting of countries from Phase III to Phase II.

slopes of the curves.[54] Variation in export optimism affects the level of total aid but not its marginal productivity.

The effects of individual elements can be isolated in Table 8. Starting from the central estimate of $131 billion in capital imports for plan growth, high exports, and plan performance, we can identify the following effects of changes in different sets of policy variables:

(1) *A decrease in export growth from 5.2 per cent to 3.8 per cent* causes a reduction of exports of $39 billion and an increase of total capital inflow of $21 billion.

(2) *An increase in internal performance to the upper limit* (with a constant growth rate) causes a reduction of capital inflow by $32 billion.

(3) *A fall in the growth rate to 4.4 per cent* (with no change in internal performance) causes a reduction in external capital requirements of $31 billion and of consumption by $98 billion.

The relative importance of these changes varies with the starting point and depends largely on which of the growth limits predominates. At the upper limit growth rates, where the saving constraint is more important, the effect of increasing exports on aid requirements is less.[55] At plan growth rates, about half of any increase in exports is reflected in a reduced need for external resources in the model 1 solutions, since the external requirements of countries in Phase II are not affected.

Perhaps the most notable feature of this analysis is the sensitivity of aid requirements to variations in internal performance. At historical growth rates, the maximum reduction due to improved performance is about 20 per cent, but at the 6 per cent growth rate, upper-limit performance would reduce external capital needs by 40 per cent. Put in other terms, the capital inflow required to sustain 4.4 per cent growth with historical performance would sustain 5.4 per cent growth if all countries could achieve the upper-limit standards. The main cause of the greater sensitivity at higher growth rates is the greater importance of saving out of increased income as GNP grows. This sensitivity would be even more pronounced if we assumed that saving depends on per capita rather than total income levels.

To compare our results to other estimates, we can state them in terms of the net capital inflow in 1970 and the implied increase in external assistance between 1962 and 1970. Omitting the less likely combinations of assumptions, the indicated range of capital requirements in 1970 is

[54] The marginal productivity of aid in the three high export cases is .54 for historical performance, .81 for plan performance, and 1.54 for upper-limit performance. Corresponding values derived above from equations (18) and (19) using median observed values of the parameters were .35 for Phase II and .91 for Phase III.

[55] This effect is more pronounced at low growth rates if we do not exclude countries having capital exports.

from $10–$17 billion,[56] corresponding to the rate of growth of external capital of 3 per cent to 10 per cent from its $7.4 billion value in 1962.[57] This range compares to the U.N. estimate for 1970 of $20 billion and to Balassa's range of $9–$12 billion [4]. Our estimates have the advantage of making explicit assumptions as to country performance and of showing how the total depends on them.

The possibility of further reduction in assistance needs through better development policies is indicated in Table 8 by the magnitude of the excess imports for countries in Phase II and unrealized saving for countries in Phase III. With the moderate improvement of performance that is represented by the plan growth targets and plan performance, there would be $98 billion of unrealized saving and $20 billion of excess imports. The predominant need is to convert the unrealized saving into additional investment which will substitute for imports or increase exports.

The theoretical limits to the possibilities for reducing aid requirements in this way are shown in Table 9. As explained in Section I.D, the efficiency of the import substitution mechanism in converting surplus saving into a reduction in capital requirements may be on the order of 50–65 per cent under plausible assumptions. More massive import substitution would raise the marginal capital coefficient for the additional production and thus lower the possibilities for efficient reductions in external capital.

To illustrate the extent to which further import substitution or additional exports might reduce assistance requirements by 1975, we have made a set of projections with model 2 on the assumption that not more than 10 per cent of total investment in each country could be devoted to this purpose. The results are shown in Table 10. They suggest that optimum planning for structural change might reduce requirements for external capital in 1975 by a third or more at plan growth rates compared to the more rigid trade assumptions of model 1.

Some of the regional implications of the projections are brought out in Table 9. The regional growth rates corresponding to the average plan target of 5.2 per cent vary from 4.2 per cent for the sample of African countries to 6.0 per cent in the Near East. South Asia shows the most rapid increase in capital inflow relative to its growth in GNP, reflecting its relatively high absorptive capacity and relatively low initial savings

[56] The principal combinations for 1970 are given in Table 9 and the full range for 1975 in Table 10.

[57] We have used a factor of 1.25 to convert our sample results to the requirements of all less developed countries. The 1962 figure of $7.4 billion is based on balance of payments figures in [21], Tables 1 and 11, and is lower than the OECD estimate of $8.5 billion of capital inflow in the same year. It includes capital flows to Turkey and Greece, and excludes Puerto Rico and $0.7 billion of capital outflows from major oil exporters. The discrepancies between the U.N. and OECD estimates are discussed in [22, Annex, pp. 6–8].

TABLE 10—COMPARISON OF MODEL 1 AND MODEL 2 PROJECTIONS OF 1975 CAPITAL INFLOW[a]
(Billions of 1962 dollars)

GNP Growth Targets	Internal Performance Characteristics					
	Historical		Plan		Upper Limit	
	Low Exports	High Exports	Low Exports	High Exports	Low Exports	High Exports
	Model 1—Projections					
Historical	12.1	10.0	11.8	9.0	9.4	7.1
Plan	18.7	16.2	17.4	14.0	12.0	9.2
Upper Limit	26.1	24.2	22.5	19.6	15.9	12.9
	Model 2—Optimal Adjustment[b]					
Historical	7.9	6.1	7.7	5.4	5.5	3.5
Plan	11.1	10.3	0 3	6.9	5.5	2.9
Upper Limit	20.8	20.8	14.1	13.7	7.6	6.0

Source: AID, Office of Program Coordination, "23-year Projections," machine listings of September 16, 1964.

[a] External capital requirements exclude negative flows (capital outflows) from countries estimated to be net potential capital exporters by 1975.

[b] Estimated by assuming conversion of "excess imports" (Table 8) to additional import substituting investment in amounts not exceeding 10 per cent of total investment estimated for equivalent model 1 development alternative. The 1975 external capital "saving" under this assumption range from $3.4 to $9.5 billions.

rates. Perhaps more significant than the actual estimates is the demonstration that the allocation of external assistance in accordance with comparable standards of performance would be likely to result in substantial shifts in the regional distribution of foreign assistance.

III. *International Assistance Policies*

Our analysis has shown the conditions under which external assistance may make possible a substantial acceleration in the process of economic development. It has focused on the interrelations among external resource requirements and the development policies of recipient countries. Analysis of these interrelations leads to several principles of general applicability to international assistance policy.

The central questions for assistance policy are the measurement of the effectiveness of external assistance, the policies which recipient countries should follow to make best use of external resources, and the basis for allocating assistance among countries. This concluding section summarizes the main implications of our analysis for each of these questions and adds some qualitative elements which have been omitted from the formal analysis.

A. *The Effectiveness of Assistance*

In the short run the effectiveness of external resources depends on their use to relieve shortages of skills, saving, and imported commodities. The productivity of additional amounts of assistance over short periods can be measured by the increase in output resulting from the fuller use of domestic resources which they make possible.

Over longer periods, the use that is made of the initial increase in output becomes more important. Even if the short-run productivity of aid is high, the economy may continue to be dependent on external assistance indefinitely unless the additional output is allocated so as to increase saving and reduce the trade gap. Over the whole period of the transition to self-sustaining growth, the use that is made of the successive increments in GNP is likely to be more important than the efficiency with which external assistance was utilized in the first instance. This was demonstrated in the discussions of Figure 3 in Section I, which showed the dependence of total aid requirements on the marginal saving rate. To emphasize this point, let us assume that the productivity of investment in the first five years of the upper-limit development sequence outlined above for Pakistan had been one-third lower, requiring a correspondingly larger amount of investment and external aid to achieve the same increase in GNP. The effect would be to increase the total aid required over the 17-year period to achieve self-sufficiency by some 45 per cent. This, however, is less than the effect on aid requirements of a reduction in the marginal saving rate from .24 to .22. The critical elements in the development sequence are getting the initial increase in the rate of growth, channeling the increments in income into increased saving, and allocating investment so as to avoid balance-of-payments bottlenecks. These long-run aspects are likely to be considerably more important than the efficiency with which external capital is used in the short run.[58]

The long-run effectiveness of assistance is also likely to be increased by supporting as high a growth rate as the economy can achieve without a substantial deterioration in the efficiency of use of capital. This conclusion was derived in [8] and is elaborated in [7]. There are also several factors omitted from the formal models that argue for more rapid growth:

(1) The fact that a smaller portion of the increase in GNP is offset by population growth;

(2) The gain in political stability and governmental effectiveness that is likely to result;

[58] This conclusion is demonstrated in the evaluation of the effectiveness of aid to Greece in [1].

(3) The greater likelihood of being able to raise marginal saving rates and export growth when GNP is growing more rapidly;[59]

(4) The greater likelihood of attracting foreign private investment to finance the needs for external capital.

While the last three factors cannot be measured with any accuracy, they appear to have been important in most countries that are successfully completing the transition, such as Israel, Greece, Taiwan, Mexico, Peru, and the Philippines. These examples support the theoretical conclusion that the achievement of a high rate of growth, even if it has to be initially supported by large amounts of external capital, is likely to be the most important element in the long-term effectiveness of assistance. The substantial increases in internal saving ratios that have been achieved in a decade of strong growth—from 7 per cent to 12 per cent in the Philippines, 11 per cent to 16 per cent in Taiwan, 6 per cent to 14 per cent in Greece, and −9 per cent to 12 per cent in Israel—demonstrate the speed with which aid-sustained growth can be transformed into self-sustained growth once rapid development has taken hold.

B. *Policies for Recipient Countries*

While the receipt of external assistance may greatly reduce the time required for a country to achieve a satisfactory rate of growth, dependence on substantial amounts of external resources creates some special policy problems. One lesson from the preceding analysis is that the focus of policy should vary according to the principal limitations to growth. Just as optimal countercyclical policy implies different responses in different phases of the business cycle, optimal growth policy requires different "self-help" measures in different phases of the transition.

In Phase I, where the growth rate is below a reasonable target rate, the focus of policy should be on increasing output, implying an increase in the quality and quantity of both physical capital and human resource inputs. Our statistical comparisons suggest that a rate of growth of investment of 10–12 per cent is a reasonable target for countries whose initial investment level is substantially below the required level. Phase I can be completed by most countries in a decade if this increase in investment is accompanied by sufficient improvement in skills and organization to make effective use of the additional capital that becomes available. Although it is probably more important in this phase to focus on securing increases in production and income, a start must also be made on raising taxes and saving if international financing is to be justified by performance.

As Phase I is completed, the rate of increase in investment can be

[59] The advantages of more rapid growth with constant per capita marginal saving rates are demonstrated by Fei and Paauw [9].

allowed to fall toward a feasible target rate of GNP growth, which is unlikely to be more than 6–7 per cent. The focus of development policy should then be increasingly on (a) bringing about the changes in the productive structure needed to prevent further increases in the balance of payments deficit, and (b) channeling an adequate fraction of increased income into saving. Although theoretical discussion has tended to stress the second requirement, the first appears to have been more difficult in practice for many countries. Since substantial import substitution is required just to prevent the ratio of imports to GNP from rising, export growth at least equal to the target growth of GNP is likely to be necessary in order to reduce external aid.

As the focus of development policy changes, the instruments of policy must change accordingly. Somewhat paradoxically, successful performance in Phase I, which would justify a substantial and rising flow of foreign assistance, may make success in Phase III more difficult. If investment and other allocation decisions are based on the exchange rate that is appropriate for a substantial flow of aid, they are not likely to induce sufficient import substitution or increased exports to make possible a future reduction in the capital inflow. Planning should be based on the higher equilibrium exchange rate that would be appropriate to a declining flow of aid in order for the necessary changes in the productive structure to be brought about in time.

It is the need for rapid structural change which sets the lower limit to the time required to complete the transition to self-sustaining growth. Even though the simplified model underlying Figure 3 suggests the possibility of completing this transition in less than 20 years starting from typical Asian or African conditions, it is very unlikely that any such country can meet all the requirements of skill formation, institution building, investment allocation, etc. in less than one generation.

C. *Policies for Donor Countries*

Donors are concerned with criteria for the allocation of aid among recipients, and the means for controlling its use. Allocation and control policies are complicated by the mixture of objectives that motivate international assistance, the most important of which are (i) the economic and social development of the recipient, (ii) the maintenance of political stability in countries having special ties to the donor, and (iii) export promotion. This mixture of motives has led to a complex system of aid administration in all countries.

The predominant basis for development loans is the individual investment project, for which external financing is provided to procure capital goods from the donor country. Loans not limited to equipment for specific projects are provided to a few selected countries against the

balance-of-payments needs of development programs.[60] Substantial but declining amounts of grants are also furnished for budgetary support of ex-colonies and other dependent areas.

Our analysis suggests some directions in which improvements can be sought in the present methods of supporting economic development, which is the objective on which all parties agree. We consider first methods of transferring resources to individual countries and then allocation of assistance among countries.

1. *The Transfer of Assistance.* Any system for transferring resources must include: (i) a basis for determining the amount of the transfer, (ii) specification of the form of resources to be furnished, and (iii) a basis for controlling their use. On all these counts the project system has the virtue of simplicity. It also provides for detailed evaluation of the investments that are directly financed from external aid—which may be 10 per cent or so of total investment—and for increasing their productivity through technical review.

While the project system has much to commend it when the main focus is on increasing the country's ability to invest, it becomes increasingly inappropriate as the development process gets under way. As the rate of growth increases, we have shown that the effectiveness of aid depends more on the use that is made of the additional output than on the efficiency with which a limited fraction of investment is carried out. Furthermore, an attempt to finance the amount of external resources needed during the peak period of an optimal growth path—which may imply aid equal to 30–40 per cent of total investment—by the project mechanism alone may greatly lower the efficiency of use of total resources. Limiting the form of assistance to the machinery and equipment needed by substantial investment projects is likely either to lower the rate of growth or to distort the pattern of investment.

In these circumstances, assistance would be more effective if the range of commodities supplied could be broadened to permit the recipient's pattern of investment and production to evolve in accordance with the principle of comparative advantage.[61] While domestic supply can—and indeed must—lag behind demand in some sectors to accommodate the needed resource transfer, the country should also be preparing to balance its international accounts by the end of a specified transitional period.

Since donors fear that uncontrolled imports may be wasted in increased consumption without the restraints imposed by the project

[60] In the terminology of AID [2], the latter are called program loans. About half of U.S. development lending is on a program basis in contrast to a much smaller proportion for other OECD Development Assistance Committee members or the World Bank.

[61] This observation applies to aid in the form of agricultural commodities as well as to aid in the form of machinery or any other specified goods.

mechanism, an alternative means of control is needed. Part of the solution lies in relating the amount of aid supplied to the recipient's effectiveness in increasing the rate of domestic saving, so that the added aid will necessarily increase saving and investment as income grows. As development planning and statistics on over-all performance improve, this type of "program approach" is becoming increasingly feasible, both from the point of view of determining the amounts of assistance needed and of assessing the results.[62]

The strongest argument for the program approach arises for countries in Phase III, where the balance of payments is the main factor limiting growth and there is typically excess capacity in a number of productive sectors. In this situation, the highest priority use of imports is for raw materials and spare parts to make more effective use of existing capacity; project priorities should give primary weight to import substitution and increased exports. In this situation donor controls should be primarily concerned with the efficient use of total foreign exchange resources, which can only be assessed adequately in the framework of a development program.

2. *Allocation of Assistance.* If the objectives of the donor countries could be expressed as some function of the growth of each recipient, it would be possible to allocate aid primarily on the basis of expected development performance. The varying political objectives of the donors complicate the problem because each would give somewhat different weights to a unit of increase in income as among recipients. Even with this limitation, however, there may be considerable scope for reallocating a given amount of aid or for selective increases in individual country totals in accordance with criteria of self-help.

The predominant project approach now in use favors countries whose project preparation is relatively efficient. Other qualities that are equally important to successful development—tax collection, private thriftiness, small-scale investment activity, export promotion—are ignored in focusing on this one among many aspects of better resource use.[63]

Where fairly reliable statistics are available, an alternative procedure would be to establish minimum over-all performance standards for each country and to share the aid burden among interested donors through a consortium or other coordinating mechanism. For example, a country starting in Phase I might have as its principal performance criteria: (i) growth of investment at 10 per cent per year at a minimum standard

[62] The U.S. government has been using the program approach in India, Pakistan, Turkey, Tunisia, Chile, Colombia, and Brazil. See AID [2] and [3].

[63] It is perhaps more than coincidence that most of the striking successes in development through aid—Greece, Israel, Taiwan, etc.—were financed largely on a nonproject basis.

of productivity, and (ii) the maintenance of a marginal saving rate of .20 (or alternatively a specified marginal tax rate). There would be little possibility to waste aid on these terms, since the required increase in savings would finance a large proportion of total investment. Appropriate over-all standards for saving rates and balance-of-payments policies for countries in Phase II and Phase III could also be established without great difficulty. A country maintaining high standards—say a marginal savings rate of .25 and a marginal capital-output ratio of less than 3.3—could safely be allotted whatever amount of aid it requested in the knowledge that the larger the amount of aid utilized, the higher would be its growth rate, and the more rapid its approach to self-sufficiency.

Statistical Annex

The three tables in this annex contain the values of the structural parameters, both observed (Table A-1) and projected (Table A-2), as well as base-year (1962) values of the six variables (Table A-3), on which the 50-country projections of model 1 were based. Data sources were the U.N. *National Accounts Yearbooks*, Statistics and Reports Division of the Agency for International Development, and the *Balance of Payments Yearbook* of the IMF. Projections of the structural parameters were based on individual country studies or, where these were not available, informed judgments of country experts. These projections were made in the autumn of 1964 and reflect the best estimates available in the spring and summer of that year.

Revisions of both the historical data and the projections of structural parameters were made a year later, in the autumn of 1965, as a part of AID's continuing study of prospective worldwide foreign capital requirements. Only Table A-1, showing the structural relationships observed over the period 1957–62, has been revised here, however.

Important changes in the base-year data include downward revisions in both investment and savings for India and Argentina, smaller capital inflows for Argentina and Brazil, and a higher investment figure for Brazil. These changes would not, however, greatly influence the projected foreign resource requirements for the fifty-country aggregate. On the other hand, the 1965 projections gave substantially different results in some cases than the earlier ones had because of changes in the projected structural parameters. Most important of these were increased export growth rates[64], higher upper-limit target growth rates for GNP, and lower upper-limit capital-output ratios for India and Pakistan. The net effect of all changes was to

[64] Projections based upon the export parameters initially obtained for this sample gave 50-country export growth rates ranging from 5.5 to 6.5 per cent. Since these potential increases were considerably higher than most forecasters believe possible for the less developed world, each country's rate was proportionately reduced so as to give a 1962–70 combined export growth rate of 3.7 per cent as one alternative and a rate of 4.9 per cent as a second alternative. These scaled down parameters are shown in Table A-2. The same optimistic attitude toward export potential produced a subsequently revised set of export parameters, used for the 1965 AID projections, implying a combined 1963–70 export growth of 4.6 per cent under the low alternative and a rate of 6.9 per cent under the high option.

TABLE A-3—BASE-YEAR DATA[a]
(Millions of 1962 U. S. dollars)

No.	Country	(1) Gross National Product	(2) Gross Invest-ment	(3) Gross National Savings	(4) Net Foreign Capital Inflow	(5) Trade in Goods and Services: Imports	(6) Trade in Goods and Services: Exports
	Near East						
2	Cyprus	250	52	35	17	132	115
3	Greece	3,861	777	547	231	704	474
5	Iran	4,610	705	654	50	1,070	1,020
6	Israel	2,107	635	229	405	854	448
7	Jordan	339	52	−45	97	141	43
9	Turkey	6,082	968	770	198	699	501
10	U.A.R.	3,692	575	312	263	1,002	739
	South Asia						
1	Ceylon	1,454	223	196	27	447	420
4	India	37,211	6,423	5,584	839	2,529	1,690
8	Pakistan	7,551	922	683	239	756	517
	Latin America						
11	Argentina	12,166	2,956	2,625	331	1,656	1,326
12	Bolivia	470	61	20	41	104	62
13	Brazil	14,053	1,912	1,494	418	1,792	1,374
14	British Guiana	149	50	26	23	100	77
15	Chile	3,458	468	271	197	765	568
16	Colombia	4,259	909	759	150	722	572
17	Costa Rica	467	74	52	22	130	108
18	Ecuador	857	138	112	26	180	154
19	El Salvador	527	64	56	8	144	136
20	Guatemala	1,077	112	81	31	161	130
21	Honduras	418	60	63	−3	81	83
22	Jamaica	737	137	98	39	296	257
23	Mexico	14,175	2,180	2,039	141	1,639	1,498
24	Nicaragua	369	60	51	9	103	94
25	Panama	478	90	57	34	186	153
26	Paraguay	233	18	6	12	59	47
27	Peru	2,444	500	525	−24	595	620
28	Trinidad-Tobago	558	177	117	60	479	419
29	Venezuela	5,741	1,085	1,812	−726	1,801	2,527

[a] Data shown pertain to the year 1962; they are averages derived from 1957–62 time trends.

TABLE A-3—(*Continued*)

No.	Country	(1) Gross National Product	(2) Gross Invest-ment	(3) Gross National Savings	(4) Net Foreign Capital Inflow	(5) Trade in Goods and Services: Imports	(6) Trade in Goods and Services: Exports
	Africa						
30	Algeria	3,680	560[b]	156	404	1,207[b]	804
31	Ethiopia	881	91	64	28	133	105
32	Ghana	1,513	298	195	103	577	474
33	Kenya	718	99	87	12	297	285
34	Liberia	139	93	13	80	159	79
50	Mauritius	167	32	13	18	86	67
35	Morocco	1,977	209	150	60	515	455
36	Nigeria	3,434	564	381	183	738	555
37	Rhodesia-Nyasaland	1,505	268	245	23	795	773
38	Sudan	1,237	177	139	38	283	245
39	Tanganyika	597	67	40	27	223	196
40	Tunisia	739	185	64	121	296	175
41	Uganda	454	44	30	14	182	167
	Far East						
42	Burma	1,405	209	231	−22	248	270
44	Indonesia	8,348	745	486	259	1,206	947
45	Korea, South	2,178	315	82	233	393	159
49	Malaya, Fed. of	1,896	347	419	−72	941	1,013
46	Philippines	3,789	479	404	75	762	687
43	China (Taiwan)	1,805	401	273	128	371	243
47	Thailand	2,879	455	414	41	572	530
48	South Vietnam	1,381	157	−50	207	305	98

[b] Reflects largely arbitrary downward adjustment of 1957–62 averages to reflect post civil-war conditions.

Source: AID, Statistics and Reports Division and Office of Program Coordination, data as of September, 1964.

narrow the range of foreign resource requirements found for the various parameter combinations. The 50-country totals for 1975, shown as \$12.1–\$17.4 billion in Table 9, dropped to \$10.9–\$12.7 billion in the 1965 projections. The difference is very largely due to the greater export optimism

REFERENCES

1. I. ADELMAN AND H. CHENERY, "Foreign Aid and Economic Development: The Case of Greece," *Rev. Econ. Stat.*, Feb. 1966, *48*, 1–19.
2. AGENCY FOR INTERNATIONAL DEVELOPMENT: Program Coordination Staff, *Principles of Foreign Economic Assistance.* Washington 1963.

3. ———, *Proposed Mutual Defense and Development Programs for FY 1966*. Washington 1965.
4. B. BALASSA, *Trade Prospects for Developing Countries*. Homewood, Ill. 1964.
5. H. CHENERY, "The Role of Industrialization in Development Programs," *Am. Econ. Rev., Proc.*, May 1955, *45*, 40–57.
6. ——— AND M. BRUNO, "Development Alternatives in an Open Economy: The Case of Israel," *Econ. Jour.*, March 1962, *72*, 79–103.
7. ——— AND A. MACEWAN, "Optimal Patterns of Growth and Aid: The Case of Pakistan," *Pakistan Develop. Rev.*, Summer 1966.
8. ——— AND A. STROUT, *Foreign Assistance and Economic Development*, AID Discussion Paper 7, June 1965.
9. J. C. H. FEI AND D. S. PAAUW, "Foreign Assistance and Self-Help: A Reappraisal of Development Finance," *Rev. Econ. Stat.*, Aug. 1965, *67*, 251–67.
10. A. GERSCHENKRON, "Reflections on the Concept of 'Prerequisites' of Modern Industrialization," Ch. 2 in *Economic Backwardness in Historical Perspective*, Cambridge 1962.
11. GOVERNMENT OF PAKISTAN, *The Third Five-Year Plan* (1965–1970). Karachi 1965.
12. N. JACOBY, *An Evaluation of U.S. Economic Aid to Free China, 1951–1965*. AID Discussion Paper 11, 1966.
13. W. A. LEWIS, "Economic Development with Unlimited Supplies of Labor," *Manchester School*, May 1954, *22*, 139–92.
14. M. MAMALAKIS AND C. REYNOLDS, *Essays on the Chilean Economy*. Homewood, Ill. 1965.
15. R. MCKINNON, "Foreign Exchange Constraints in Economic Development and Efficient Aid Allocation," *Econ. Jour.*, June 1964, *74*, 388–409.
16. ORGANIZATION FOR ECONOMIC CO-OPERATION AND DEVELOPMENT, *Development Assistance Efforts and Policies: 1965 Review*, Report of the Chairman of the Development Assistance Committee. Paris 1965.
17. G. RANIS AND J. C. H. FEI, "A Theory of Economic Development," *Am. Econ. Rev.*, Sept. 1961, *51*, 533–65.
18. P. ROSENSTEIN-RODAN, "International Aid for Underdeveloped Countries," *Rev. Econ. Stat.*, May 1961, *43*, 107–38.
19. ———, "Determining the Need for and Planning the Use of External Resources," in *Organization, Planning and Programming for Economic Development*, Vol. 8 of U.S. Papers Prepared for the U.N. Conference on the Application of Science and Technology for the Benefit of the Less Developed Areas. 1963.
20. W. ROSTOW, "The Take-off into Self-Sustaining Growth," *Econ. Jour.*, March 1965, *66*, 25–48.
21. UNITED NATIONS, Department of Economic and Social Affairs, *International Flow of Long-Term Capital and Official Donations, 1960–1962*, U.N. document no. E/3917/Rev. 1, New York, United Nations, 1964.
22. ———, "Accelerated Flow of Capital and Technical Assistance to the

Developing Countries: Measurement of the Flow of Long-Term Capita and Official Donations to Developing Countries; Concepts and Method ology," *Report of the Secretary-General*, U.N. document no. A/573 (processed), Feb. 1, 1965.
23. ———, *World Economic Survey*, 1963, Part 1.
24. J. VANEK, *A Case Study in Development Analysis: Future Foreign Re source Requirements of Colombia*. Agency for International Develop ment, 1964 (processed).
25. J. G. WILLIAMSON, *Projected Aid Requirements for Turkey, 1960–1975* AID Discussion Paper 10, December 1965.

THE SOVIET COLLECTIVE FARM AS A PRODUCER COOPERATIVE

By Evsey D. Domar*

Imagine that most of the obstacles facing Soviet kolkhozes (collective farms) today, such as output and delivery quotas, administrative interference, shortage of strategic inputs (materials, spare parts, fertilizer), depressed prices of outputs, etc., suddenly vanish, and the kolkhozes find themselves in a Lange-Lerner type of a competitive world where everything can be bought and sold at a market price, and where peasants are free to run their own affairs *provided the essential structure of the kolkhoz is retained*. How would Soviet agriculture, or for that matter any economic sector so organized, fare in such a wonderland?[1]

Freed from existing restrictions and abuses, the kolkhoz would presumably revert to its prototype—a producer cooperative which utilizes the labor of its members, purchases other inputs, sells its outputs, pays a rent and/or taxes, and divides all or a part of its net proceeds among its members. The presumed democratic nature of such a co-op and its freedom from capitalist exploitation has made it highly attractive to socialists and social reformers for ages. But its popularity has not prompted its proponents to analyze it with the same loving curiosity that the "bourgeois" economists have shown toward the capitalist firm. And yet it must have been obvious, at least to some of these proponents, that co-op members are likely to be ordinary human beings bent on maximizing the benefits from their participation in the co-op.[2] The first and only attempt to construct a model of a co-op that I have seen belongs to

* [The author is professor of economics at Massachusetts Institute of Technology.] I am very grateful to Abram Bergson, Michael R. Dohan, John G. Gurley, Michael D. Intriligator, Nancy Nimitz, David McGarvey, and Egon Neuberger for their generous assistance and helpful comments. Questions raised by Mr. Neuberger made me rewrite the whole paper. David Conklin acted as my research assistant, and Martin Weitzman went over the mathematics. I had intended to include in the paper a brief survey of relevant recent Soviet literature, and James R. Millar of Cornell University kindly lent me three chapters of his dissertation [8]. This project was abandoned because of lack of space; besides, Mr. Millar's survey is more comprehensive and thorough than mine could have been. I am also grateful to the RAND Corporation for its facilities, encouragement and support. Some of the research was supported by the National Science Foundation as well. None of these persons or organizations is of course responsible for my conclusions or for any errors which may still be lurking around.

[1] The question is not as academic as it sounds. Recent changes in Soviet agricultural policies represent another step toward that wonderland, though there is still a long way to go [16] [17] [18] [20] [21] [22] [23] [24].

[2] I do not discuss here producer co-ops organized for essentially noneconomic reasons, such as by religious orders, Israeli pioneers, etc.

Benjamin Ward in a path-breaking paper published in 1958 [12].[3] But like many a pioneering work, it has not attracted much attention.[4]

The present study consists of three parts: in the first, Ward's creation, called here the "Pure Model" of a co-op, is reworked with a generalized production function. The tenor of Ward's findings (based on a single-output, one-or-two-input function) is confirmed, but some of the results are made more definite, and one is reversed. In the second part, the co-op is faced with a supply schedule of labor; this makes the model much more realistic and reverses the paradoxical results of the "Pure Model." Finally, a summary and a few conclusions are presented in the third part.

LIST OF SYMBOLS

(in order of appearance)

R—a fixed rent paid by the co-op ($R>0$)

v—($-\pi/x_n$)—dividend rate or dividend per labor unit ($v>0$)

x_n—labor input ($x_n<0$)

n—number of outputs and inputs

π—profit of the co-op gross of dividend payments ($\pi>0$)

p_i—price of x_i ($i=1, \cdots, n-1$)

x_i—an output when $x_i>0$, an input when $x_i<0$ ($i=1, \cdots, n-1$)

p_n—wage rate paid by the "capitalist twin"; originally $p_n=v$

λ—Lagrange's multiplier

$E_{x_i p_j}=(\partial x_i/\partial p_j)\cdot(p_j/x_i)$—elasticity of demand for, or supply of, x_i in response to change in p_j ($i, j=1, \cdots, n$)

$u_i=p_i x_i$—value of an output when $x_i>0$, or of an input when $x_i<0$.

A few other symbols are defined when introduced.

An asterisk indicates that the expression pertains to the "capitalist twin."

[3] There have of course been a number of analyses of firms which do not maximize total profit in the usual way. See for example Scitovsky [10]; F. and V. Lutz [7]; Baumol [5]; Averch and Johnson [4]; and Westfield [13]. A very interesting book on cooperatives was published by Tugan-Baranovsky [11a] in 1921. His conclusions were very similar to mine given in Sec. III of this paper. I owe this reference to Steven Rosefielde of Harvard.

[4] In my sample of some forty or fifty reputable economists about a third of whom work in the Soviet and related fields, Ward's article had been read by three or four persons at most. Perhaps the paper's title gave the wrong impression that it pertained to Yugoslavia only. Or—who knows—this might have been the normal fate of an excellent paper.

I. *The "Pure Model" of a Co-op*

Assumptions:

1. All nonlabor inputs are bought and all outputs are sold by the co-op at given (parametric) prices.
2. The production function of the co-op, if possessed by a profit-maximizing firm, would have all necessary and sufficient properties for a stable equilibrium under perfect competition [2] [3].
3. The co-op pays a fixed rent $R>0$ per year.[5]
4. Instead of paying wages, the co-op divides all (or a constant fraction) of its income net of all other costs and rent equally among its members or among homogeneous labor units in the form of a dividend.[6]
5. The objective of the co-op is the maximization of the dividend per unit of labor or of the dividend rate $v>0$. There is complete certainty.[7]

[5] Soviet kolkhozes do not pay rent as such, but the system of compulsory deliveries and differentiated zonal prices is directed against the richer farms and regions and allows the government to extract some rent. Until 1966, the farms paid a 12.5 per cent tax imposed on income net of nonlabor cost (excluding 80 per cent of income from animal products), but gross of dividend payments. The rate could be modified by regional authorities in favor of poorer and against richer farms [11].

Beginning with 1966, the tax rate is set at 12 per cent, and the taxable income seems to exclude two items: (1) profit equal to 15 per cent of nonlabor costs, and (2) dividend payments not exceeding a certain average per member, to be set by the government [17]. If the post-tax dividend rate is indicated by v_t, it follows that

$$v_t = v(1-t) + \frac{Cet}{L} + wt,$$

where t is the tax rate (12 per cent), e is the 15 per cent exclusion, w is the exemption per member, C is the nonlabor costs

$$\left(C = -\sum_{k}^{n-1} p_i x_i, \text{and } k \text{ is the first input}\right),$$

and $L=-x_n$ (to avoid negative numbers). Now, t, e, and w are constants, but C/L is not. Hence the imposition of the tax in its new form will affect economic decisions in the kolkhoz. I am not sure, however, that my interpretation of this tax reform is correct (the official statement being rather confusing). For this reason and to save space, I will disregard the complexities of the tax law both before and after 1966, and mean by the word "tax" a simple proportional levy on profits before dividend payments. But a further investigation of the effects of this new tax may be worthwhile.

[6] Actually it is the Soviet practice to transform most of the labor of kolkhoz members into a homogeneous sum by a system of weights depending on the required skill and the nature of work. The weights vary from one kolkhoz to another, but the range seems to be around $2\frac{1}{2}:\frac{1}{2}$. If the relative weights correspond to the ratios of the values of the marginal products of the several kinds of labor this is a reasonable procedure. It would not matter if the co-op first paid uniform wages per labor unit (as Ward assumed) and then used the balance of income for dividends.

It should be also noted that a substantial fraction of the income of the kolkhoz is retained by it (the so-called "Indivisible Fund") for reinvestment and improvements, and that this fraction varies from one farm to another and from one year to the next, depending on economic conditions and on administrative decisions.

[7] No dynamic elements are considered either. Even though some comments about investment decisions will be made, the model essentially refers to the short run.

6. The co-op is *actually able* to employ the optimum number of labor units maximizing the dividend rate. This assumption (used by Ward) distinguishes this model from those presented in Part II.

7. Finally, there exists a profit-maximizing firm, the "capitalist twin," with the same production function and prices as the co-op, and with a wage rate initially equal to the co-op's dividend rate.

We are concerned in this model with a rent rather than with a tax because a glance at expression (1) below will show that neither an income tax (imposed on net income before dividends), nor a poll tax (per unit of labor) would affect the co-op's decisions in the context of this model: the optimum allocation of resources yielding the maximum dividend before the tax remains unchanged by the tax.[8]

The rent is assumed to be positive because an $R<0$ (a subsidy) would induce the co-op to maximize the dividend rate v by reducing labor input to zero. Even an $R=0$ can produce this effect if v declines from the very beginning and has no maximum point. We shall assume that R is large enough to give us a meaningful problem but not to eliminate the co-op's net income or to convert it into a loss.

Note that in the generalized production function given in (2), it is customary to express outputs in positive units, and inputs, including labor (x_n), in negative. Hence many derivations in the Mathematical Appendix have seemingly perverse signs, and a minus sign is attached to expression (1).

Our basic problem consists of maximizing the dividend rate

$$v = -\frac{\pi}{x_n} = -\frac{\sum_{1}^{n-1} p_i x_i - R}{x_n}, \tag{1}$$

subject to the production function

$$f(x_1, \cdots, x_n) = 0. \tag{2}$$

The solution of equations (1) and (2), while not difficult, is somewhat involved, and can be safely relegated to the Appendix. It is shown there that the equilibrium position of the co-op is identical in every respect to that of its capitalist twin defined in assumption 7. On reflection, this is to be expected: for any given labor input the co-op simply maximizes total profit like the twin, and hence chooses the same outputs and nonlabor inputs. When it comes to labor, the attitudes of the two organizations differ: the twin hires labor until the value of the marginal product equals the wage; the co-op uses labor to the point where the value of the margi-

[8] In models presented in Part II an income tax does affect economic decisions. A gross receipt tax would have the same effects as a proportional reduction in the price of every output. See note 5.

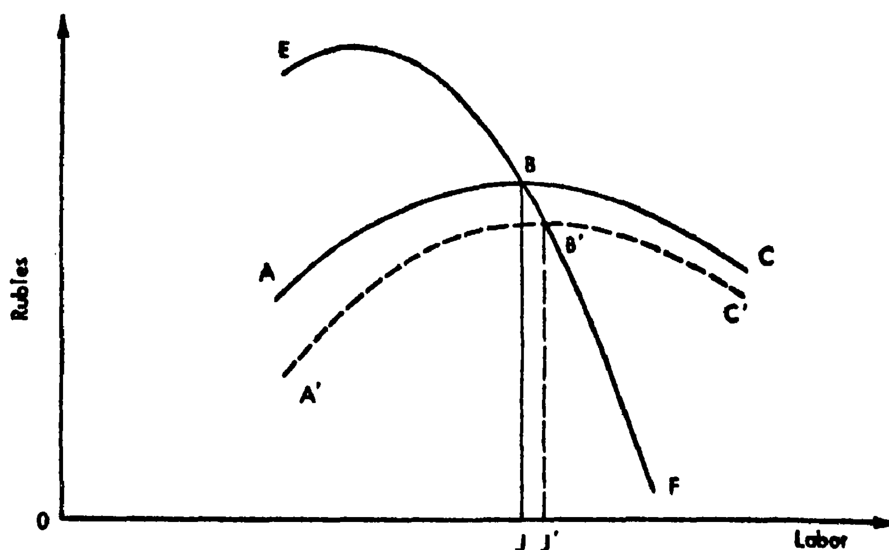


FIGURE 1

nal product equals the dividend paid (a unit of labor contributing less than the going dividend rate will not be used).[9] But since the two schedules of the value of the marginal product of labor are identical and the wage paid by the twin initially equals the dividend paid by the co-op (by assumption 7), the labor inputs used by both organizations are identical as well.

The *reactions* of the two enterprises to changes in rent or in prices, however, are altogether different. An increase in R has no (short-run) effects on the twin because it changes neither the value of the marginal product of labor nor the wage rate. But it reduces the dividend rate paid by the co-op and therefore moves the point of intersection between the dividend rate ABC (on Figure 1) and the value of the marginal product of labor EBF further to the right, from B to B' because of the assumed negative slope of EBF. So, as Ward has shown, more labor will be used (OJ' instead of OJ) and (in the absence of Hicks's "regression" [6]) output will increase as well.[10] But before the reader concludes that the best way to increase peasant participation in Soviet kolkhozes is by imposing (or increasing) rents, he should take a look at Part II.

The effect of a price change on the co-op is more complex because both the dividend rate and the value of the marginal product of labor are

[9] The schedules of the dividend rate and of the value of the marginal product of labor, as used here, are based on the assumption that equilibrium conditions are satisfied for all outputs and all nonlabor inputs all along the schedules. See, however, note 18.

[10] See the Appendix, Sec. 3.

affected.[11] If there is only one output (x_1) and labor is the only input, the outcome is certain: a doubling of p_1 will double the value of the marginal product of labor, while the dividend rate, as shown by expression (1) rewritten for this occasion as

$$v = -\frac{p_1x_1 - R}{x_n},^{12} \tag{3}$$

will *more* than double. The intersection point of the curves EBF and ABC will now move to the left, from B to B' on Figure 2, and both the labor input and hence the output will contract, as proved by Ward.

With one output and several inputs the situation becomes clouded, but a plausible assumption (that the isoquants of the production function are radially parallel to each other) saves the day and preserves the restrictive conclusion just reached.[13] The solution of the general case of several outputs and inputs is given in the Appendix;[14] the results are summarized in Table 1.

On the whole, the co-op's reactions to an increase in p_1 are rather peculiar. When x_1 is an output, there is a general tendency to restrict operations; when x_1 is an input—to expand them. Even when the co-op moves in the same direction as a capitalist firm, its response is usually more sluggish. For market stability, the picture is not particularly reassuring.

True enough, if x_1 is one of several outputs, a rise in its price is likely to *increase* its production; its own market can therefore be stable, contrary to Ward's expectations based on the one-output production function. But the input of labor is very likely to decrease and hence lead to a contraction of other outputs. Since many agricultural products are reasonably good substitutes for each other, the conditions which have led to a rise in the price of, say, wheat may very well raise the prices of rye and corn as well, The simultaneous rise in several prices, being similar to the rise in the price of some *important* single user of labor (see the first line of Table 1), can cause a general restriction of output. Although a negatively sloping supply curve is not a sufficient condition for market instability, it is too close to it for comfort.

[11] The values of the marginal products of other inputs will also be affected.

[12] The relative increase in v is greater than in p_1 because R is subtracted from p_1x_1 in the numerator. However, x_1 and x_n also change. It is shown in the last section of the Appendix that

$$\frac{dv}{dp_1}\cdot\frac{p_1}{v} = \frac{p_1x_1}{p_1x_1 - R} > 1$$

if the production function consists of x_1 and x_n only. See also Sec. 3(a) of the Appendix.

[13] Appendix, Sec. 3(b).

[14] Appendix, Secs. 3(c) and 3(d).

Of course the undesirable negative effect of the rise in p_1 can be counteracted by an appropriate increase in rent. But such manipulations of rent in response to changing prices would require more knowledge and skill than are likely to be possessed by the Soviet or, for that matter, by most other governments. And besides, the whole idea of rent implies a sum fixed in advance for a reasonably long period of time. It would not help to replace it with a tax on net income because (as mentioned above) such a tax does not affect co-op's decisions. Some other tax might, but it is hardly worth investigating in the light of Part II to come.

The model augurs little good for the allocation of resources among the co-ops. As Ward has observed, the labor market will be rigid. Indeed, if a rich and a poor co-op should each be in its respective equilibrium, no movement of labor from the poor co-op to the rich is possible (except through a merger) because any movement would reduce the dividend rates in both co-ops. Since the dividend rate in equilibrium equals the net value of the marginal product of labor, there is a definite misallocation of labor, and of course of other resources as well, among the co-ops. The hiring of the members of the poor co-op by the rich is the obvious solution, but it is not permitted in the present model.

The best measure both on equity and resource allocation grounds to be taken here is an increase in the rent paid by the rich co-op (or a relief for the poor one, if it pays any). Equity alone could be satisfied by a system of differential prices both for outputs and for inputs discriminat-

FIGURE 2

TABLE 1—A SUMMARY OF THE EFFECTS OF AN INCREASE IN p_1 ON THE MAGNITUDES OF OUTPUTS AND INPUTS
(in absolute terms)

Effect on	Usual Capitalist Reaction	Probable Co-op Reaction
	If x_1 is an Output:	
x_1	Positive	Positive, unless x_1 is a very important user of labor (or the only output)
$\lvert x_n \rvert$	Positive	Negative (except in a special case in the Appendix)
x_2 as all other outputs	Positive, if x_2 is complementary to x_1	Negative if input proportions remain unchanged; otherwise, indeterminate
	Negative, if x_2 is competitive with x_1	Negative
$\lvert x_2 \rvert$ as an input	Positive	Negative with high complementarity between x_2 and x_n
		Indeterminate with partial complementarity
	If x_1 is an Input:	Positive if x_2 and x_n are substitutes
$\lvert x_1 \rvert$	Negative	Negative, except for constant proportions between x_1 and x_n when the effect is zero
$\lvert x_n \rvert$	Negative if x_1 and x_n are complementary Positive if they are substitutes	Positive, except for a zero effect if x_1 and x_n are used in constant proportions
x_2 as an output (or all output)	Negative	Indeterminate
$\lvert x_2 \rvert$ as an input	Negative if x_1 and x_2 are complementary Positive if they are substitutes	Indeterminate

ing against the rich co-op,[16] but the equalization of the dividend rate so achieved would obviously not equate the social value of the marginal product of labor (and of other inputs) in the two co-ops.

All these equalizing measures can backfire if the wealth of the rich co-op is due not to its better natural conditions and location, but to greater effort and interest of its members not accounted for in the conventional measures of labor input. But these considerations are outside of the scope of this model and even of this paper.

In making investment decisions, each co-op will behave like its own capitalist twin paying a wage rate equal to the co-op's dividend rate. For the rich co-op labor is expensive, for the poor—it is cheap. Hence the former will prefer ready-made labor-saving machinery, while the latter will look for labor-using projects and be inclined, for instance, to use its own labor in construction. To the extent that the poor co-op is poor

[16] A standard remedy in Soviet literature and practice.

because of shortage of capital, it will be more inclined to invest than the rich. Whether it will have the means to do so is less likely.

II. *The Model with a Supply Schedule of Labor*

The "Pure Model," for all its interesting and amusing (I hope) paradoxes, has one slight defect: it is unreal. It assumes that labor input can be varied with changing prices and rent in order to maximize the dividend rate (assumption 6), a highly unlikely situation once the co-op has been organized. Surely the co-op, by its very nature, cannot admit and expel members at will. Hours of labor contributed by each member can of course be varied, but it is rather improbable that the members' welfare functions should call for a maximum dividend either per hour or per year irrespective of the number of hours worked.

Two possibilities will be considered here. First, the number of members is given (at least in the short run), and so is the number of hours contributed (according to some custom) by them. Then labor input is fixed, and the co-op simply maximizes total profit. Second, the co-op members may have other opportunities for employment and for leisure. In Soviet kolkhozes, they may cultivate their own plots, work in town, on a neighboring state farm, or even "lie on the stove" and do nothing according to the age-old Russian custom. In other words, the co-op is faced with a supply schedule of labor, which will be assumed here to have the usual positive slope. The equilibrium position of the co-op is found in two steps: first the co-op maximizes total profit for every given labor input, and obtains the familiar dividend rate curve ABC on Figure 3. The latter now serves as the demand schedule for labor as well, and its intersection with the labor-supply curve determines the labor input contributed by members *acting as such*. It need not correspond to the highest point B on the ABC curve, where it is intersected by the marginal curve EBF.

The appearance of this innocent-looking supply curve of labor produces a drastic difference in the reactions of the co-op to changes in prices and in rent as compared with the "Pure Model." Also, an income tax imposed on the co-op's income gross of dividend payments, ineffective previously, becomes relevant here.[16] Depending on the position of the labor-supply curve in relation to ABC, three variations of the present model will be discussed.

Case 1. Moderate Labor Shortage

The supply curve of labor HKM in Figure 3 intersects the dividend-rate curve ABC at K, to the left of the maximum point B. If, to preserve

[16] For practical reasons, we need not bother with a poll tax. In any case, its effects are similar (but not identical) to those of the income tax. Income or poll taxes *imposed on the members* (rather than on the co-op) are not considered here. See note 5.

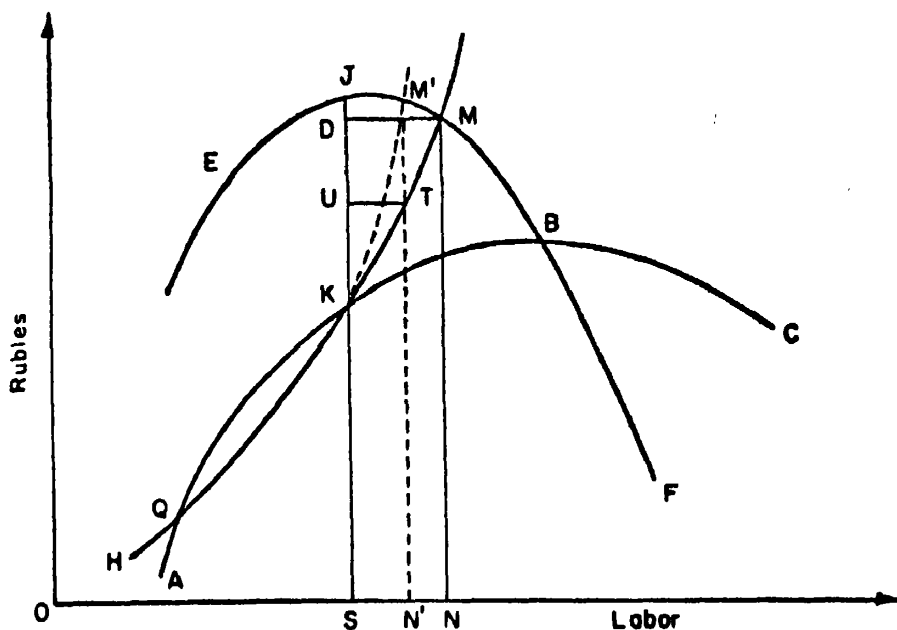


FIGURE 3

the purity of the co-op, no discrimination among its members is permitted, the co-op has to reconcile itself to the situation and simply stay at point K.[17] But its paradoxical behavior disappears. Any improvement in the dividend rate, be it from lower rent or taxes, or from better prices, lifts the ABC curve, increases the employment of labor, and presumably raises output. In the light of this model, which is much closer to Soviet reality than the "Pure" one, price reforms promulgated by Khrushchev and his successors are justified.

It is not clear what labor cost will be used in making economic decisions. The correct cost should be SJ—the net value of the marginal productivity of labor—but since no one is actually paid this rate, the management may not know what it is.

If it is possible to discriminate among members and to hire those with particularly attractive outside opportunities (or the laziest ones) between K and M, new possibilities open up. In treating its hired members, the co-op will behave *more or less* like a capitalist employer, depending on the standing of these members in the co-op and other circumstances. Three subcases will be considered:

(a) If the hired members have different skills (or if the co-op manage-

[17] Provided it gets to point K in the first place. Point K is stable in both directions, but Q is not. An upward movement from Q will bring the co-op to K, but unfortunately even a slight downward movement from Q can destroy the co-op altogether.

ment is very adroit), the co-op may act as a discriminating monopsonist and pay each of them the wage indicated by the supply curve. The area *KJM* represents the profit so obtained; presumably it will be added to the dividend paid to the regular members.

(b) The hired members may persuade or force the co-op to hire all of them at the highest wage *NM*. A smaller profit equal to the area *DJM* is now made.

(c) As a compromise between these two extremes, the co-op may operate like an ordinary monopsonist, draw the dotted curve *KM'*, representing the marginal labor cost, to its intersection with the value of the marginal product curve *EBF*, employ altogether *ON'* units of labor, pay a wage of *N'T*, and make a profit from hiring of *UJM'T*.[18]

In all these cases, the total employment of labor exceeds *OS*, output expands, and dividend rate rises over the original level of *SK*.[19] Everyone is better off provided the ordinary members are not consumed with envy. They need not be if the hired members do possess special skills. But if they are common members who refuse to work for the co-op more than the minimum required to retain their membership, the social situation can be rather difficult.[20]

The reactions of the co-op to changes in rent, income tax and prices now take still another turn and become more similar to those of a capitalist firm. An increase in rent lowers the average *ABC* curve but leaves the marginal *EBF* schedule intact. Hence, in cases (a) and (b) no reduction in labor input takes place, except that some members who were previously satisfied with dividends will now demand a wage. But output remains undisturbed. In case (c), however, the fall in the *ABC* curve has a special effect: now that hiring begins earlier (that is, to the left of point *K*) the curve *KM'* moves leftward and intersects *EBF* to the left of the old point *M'*. Hence, total employment of labor falls and so does output.

An increase in the income tax rate lowers both the average and the

[18] There is some incongruity among the several curves in Figure 3. Schedules *ABC* and *EBF* are drawn on the assumption that the co-op is in equilibrium position in respect to all outputs and all nonlabor inputs for every given labor input, or that with every change in the labor input everything else is adjusted accordingly. The supply curve of labor *HKM* and the marginal cost of labor curve *KM'* indicate labor cost only, without allowing for these adjustments. I can take refuge in the approximate nature of Figure 3 used here for illustration only. A similar qualification applies to Figure 4 as well.

[19] It will be above *SK* but somewhat below the corresponding point on the *ABC* curve because the latter implies that no members are paid a wage in excess of the dividend.

[20] Strictly speaking, we assume either that the labor force is homogeneous, or that more skilled labor is transformed into ordinary labor according to some fixed weights, as it is done in Soviet kolkhozes (see note 6). So a refusal of a carpenter to work for the kolkhoz means his unwillingness to accept a multiple of the dividend rate paid to common workers.

In the text, all members working for the co-op receive *either* the dividend *or* a wage. In reality, some combinations of the two are sometimes encountered (see note 21). But this paper is already too long to analyze such a situation.

marginal curves (because it falls on profit *gross* of dividends) and thus reduces the employment of labor, while better prices raise both curves and have a positive effect on labor and on output.

Without the income tax, the wage rate paid to the marginal hired worker (or to all of them) in cases (a) and (b) equals the net value of the marginal product of labor. In case (c) there is the usual disparity created by monopsony. But the presence of the income tax creates a special gap between the social and private (as seen by the co-op) values of labor's marginal product in all three cases, and distorts allocation of resources.

So far only the hiring of its own members by the co-op has been considered. If the co-op can hire outsiders as well, as for instance a rich co-op hiring workers from a poor one, the allocation of labor may be further improved. The hiring co-op will simply behave like a profit-maximizing firm. We'll return to the hiring-out co-op in Case 3.[21]

Case 2. Severe Labor Shortage

When the supply curve of labor HM lies completely to the left of the ABC dividend curve, the co-op as such cannot function at all. And yet it *may* be possible to hire every member at the wage demanded by him and still pay a rent! The members may be divided into groups (by skill, for instance) and each group paid a wage demanded by its marginal member. Freedom in hiring certainly increases the flexibility of the organization. Perhaps this is one of the reasons why Soviet state farms who hire all their labor are doing better than the kolkhozes, and why some weak kolkhozes have been transformed into state farms[22] [19, p. 19].

Case 3. An Excess Supply of Labor

The supply curve of labor HKM intersects the ABC curve at M to the right of the optimum point B in Figure 4. The co-op may be either very

[21] In the actual operation of Soviet kolkhozes, some discrimination and hiring are permitted. The more skilled members are paid dividends at a higher rate (see note 6). In Abramov's delightful story about a kolkhoz [1] carpenters are paid one ruble a day in addition to the dividend rate. In a *Krokodil* story, two neighboring kolkhozes hire *each other's* carpenters (presumably the carpenters have refused to work for dividends on either farm and demanded market wages). Finally, kolkhozes hire agricultural specialists and other experts and skilled workers. It seems that the richer farms even hire ordinary workers in busy times.

The Soviet government has been recommending to the kolkhozes to pay their members a wage in money equal to some 80 per cent of the expected dividend (for the given type of work). But this was an advance payment of a dividend rather than a true wage. It seems that some of the kolkhozes who have tried to follow this recommendation became short of funds [14]. New measures for alleviating the seasonal shortage of funds in the kolkhozes were announced on January 5, 1966 [23].

[22] It seems that the definite wage paid by the state farms has a better incentive effect than the promise of an uncertain dividend. An official promise to investigate the possibility of "guaranteed compensation of labor in all kolkhozes" was recently made [21]. But there are many other reasons for the relative success of state farms, such as the payment of higher wages, and most important—access to the government budget. For that matter, not all state farms have been successful.

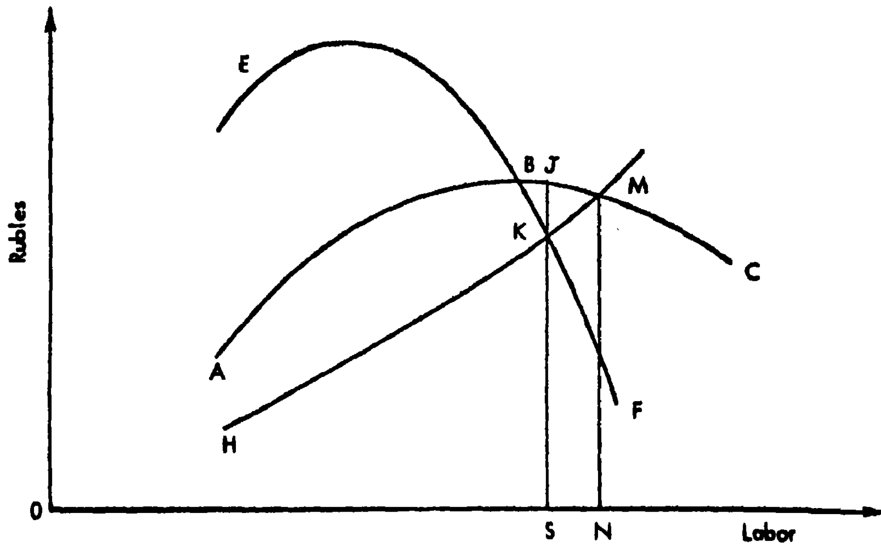


FIGURE 4

rich or located far from other employment opportunities. The situation is analytically similar to Case 1. If it is impossible to get rid of some members or to reduce labor input by rationing, the co-op will simply stay at M. Any improvement in conditions, such as lower rent and taxes, or better prices, will raise the ABC curve and increase the quantity of labor used, as in Case 1. But if each member between K and M can indeed take an outside job and earn a wage at least equal to the corresponding point of the supply curve, it will be better for all concerned to *hire out* the members in KM, and in fairness to them, collect their earnings and pay them the regular dividend. The members who are to the right of point M will prefer to remain on their own. In the absence of an income tax, the net value of the marginal product of labor will equal its supply price at K, which is good, but since the dividend rate paid to the members will be approximately equal to SJ rather than SK, there is a danger of overevaluation of labor cost by the management.[23] The presence of an income tax creates the same distortions as in Case 1. When the hiring out of labor is permitted, changes in rent, income tax rates, and in prices in Case 3 will have the same effects on the total employment of labor by the co-op as in Cases 1(a) and 1(b).

The (positive or negative) hiring of members improves the allocation of resources within each co-op, but so long as it is confined to its own

[23] It will be somewhat higher than SJ because the ABC curve disregards the higher wages earned outside. See note 19.

Perhaps a Soviet kolkhoz can act as a monopolist in hiring out labor. An analysis of this case is similar to that given in Cases 1(b) and 1(c) above.

It should be noted that Figure 4 suffers from the same incongruity as Figure 3. See note 18.

members (as assumed here) the distribution of labor among the co-ops remains unchanged.[24] In contrast to the "Pure Model" where the inequality of dividend rates among the co-ops indicated misallocation of labor (and of other resources) and where the transfer of labor from the poor to the rich units was clearly in the social interest, the present situation is ambiguous because the value of the marginal product of labor in each co-op no longer equals (except accidentally) the dividend rate: in Figure 3 it is above the dividend rate, in Figure 4—below it. In the improbable (at least among Soviet kolkhozes) case when Figure 3 depicts a rich unit, and Figure 4—a poor one, the transfer of labor from the poor to the rich co-op will be welcomed by all parties. But in the more realistic case when the poor co-op is to the left of the point B (as in Figure 3) and the rich—to its right (as in Figure 4) both co-ops will object to this transfer because the poor co-op seeks more labor (so as to reach point B), and the rich—less, while social desirability remains obscured by the differences (in opposite directions) between the value of the marginal product of labor and the dividend rate in each co-op.

III. *Concluding Remarks*

The results of all our models are summarized in Table 2. I now turn to

TABLE 2—THE EFFECTS OF CHANGES IN RENT, TAX RATES, AND PRICES ON THE EMPLOYMENT OF LABOR BY THE CO-OP

Nature of Change	"The Pure Model"	The Model with a Supply Curve of Labor	
		Without Hiring	With Hiring
Increase in rent	Positive	Negative	Neutral, except in Case 1(c)
Increase in income tax rate	Neutral	Negative	Negative
Improvement in prices	Negative	Positive	Positive

those aspects of Soviet agricultural policy to which this paper may be relevant. I think they can be fairly listed as follows:

1. To increase labor participation in the kolkhozes.
2. To improve resource allocation on the farms.
3. To improve allocation of labor among the farms.
4. To bring about greater equality of income per member (or per labor unit) among the farms.
5. To siphon off the excess purchasing power which will be acquired by some peasants if agricultural prices are moved closer to their marginal costs.

[24] Except when the hired-out members of one co-op are hired by another.

All these (and other) aspects have been discussed in Soviet literature in recent years, with point (4)—the high degree of inequality among peasant incomes—receiving particular attention. I have not found a comprehensive distribution of peasant incomes, but it is fair to conclude from various sources that ratios of four to one, of six to one, and even higher, between rich and poor farms are not uncommon.[25] This inequality is attacked on equity grounds—unequal pay for equal work, the suggested remedy consisting of price manipulations in favor of the poor and against the rich farms. That both the existing inequality and the remedy for it are linked with allocation of resources is hardly ever mentioned. Perhaps the discussion of these finer points in Soviet literature is premature, plagued as their agriculture still is with crude central planning, administrative interference and other problems.

In making the list of my recommendations based on Table 2, however, I shall retain our initial, though still unrealistic under Soviet conditions, assumptions that the peasants will be free to run the kolkhozes as they see fit (short of disbanding the kolkhoz system altogether)—there have been definite moves in that direction lately—and that relative price ratios will reflect real scarcities. Now a difficult (for an economic theorist) choice must be made between being original, if unrealistic, and being conventional and practical. For what could be more original and striking than recommendations derived from the "Pure Model," namely

[25] According to V. N. Starovsky, Director of the USSR Central Statistical Administration, ". . . in 1961 payments per man-day on about 30 per cent of the USSR's collective farms came to only one-fourth to one-fifth as much as on the 20 per cent of the farms where the pay was the highest" [15]. It is not clear whether Starovsky referred to total or to money incomes only.

In the last few years many changes in Soviet agriculture have taken place, but it is hard to judge whether income inequality among the peasants has gone up or down. The following compilation of the official average kolkhoz money income per household among 66 provinces (*oblasti*) of the Russian Republic in 1960 and 1963 indicates little change [25]:

	1960	1963
Ratio between the fourth and first quartiles of the distribution of the provinces	2.0	1.8
Ratio between the means of the highest and lowest 25 per cent of the provinces	3.1	3.2
Ratio between the highest and the lowest single provinces	8.4	8.6

Among the several republics comprising the Soviet Union, the richest one (in the above terms)—Turkmenia—had almost four times as large an income in 1963 as the poorest one—Georgia [26]. A number of scattered sources indicates that ratios of 2 to 1, 3 to 1, and even higher *within* a republic or a province are quite usual. If so, income disparities between the richer and poorer farms become large indeed, and considerably larger than the ratios stated in the text above. On the other hand, a comparison of money income alone can exaggerate the advantage enjoyed by a cotton-producing area like Turkmenia, or by the seemingly rich Far-Eastern provinces where the price level is likely to be higher than in the more central parts of the country. But it is important to note that these inequalities persist in spite of price differentials against the richer regions.

that rent should be increased (or imposed) and terms of trade turned against the peasants in order to make them work longer and harder for the kolkhoz? This would indeed vindicate Stalin's agricultural policies, even though he had arrived at them without building any models.

Dismissing the "Pure Model," we come to the following list of recommendations:

1. To allow the prices of material inputs and of agricultural outputs to move to their equilibrium levels as determined by demand-supply conditions, even at the expense of a sharp rise in peasant gross incomes.
2. To abolish the income tax paid by the kolkhozes.
3. To impose a rent on each farm related to its location, soil fertility, and other natural conditions.[26]

If this list is so conventional as to create an anticlimax, I apologize. Now and then conventional economic theory does give the right answers, even when applied to a Soviet kolkhoz. And—I almost forgot to make explicit a rather essential recommendation—that the kolkhoz be permitted to engage in as much positive or negative hiring of labor as it can and wants. If this freedom brings it suspiciously close to a profit-maximizing capitalist firm, the similarity could be declared to be purely coincidental.

* * *

Two *caveats* are in order: (1) These conclusions have been derived from a model which proved to be highly sensitive to seemingly innocent changes in assumptions. If I think that my final creation is fairly realistic, so must Ward have thought about his.

(2) Judged by strictly economic criteria, the co-op has not come out well. But even on these grounds, it is quite possible that a co-op may be more efficient than a capitalist or a state-owned firm in societies where membership in the co-op, as contrasted with hiring out for a job, has a strong positive effect on workers' incentives (though hardly the case in Soviet kolkhozes). And so may the co-op's capitalist cousin—a firm with a profit-sharing scheme.

Mathematical Appendix to the "Pure" Model

1. Equilibrium Conditions for the Co-op

Maximize (1a) subject to (2a)

[26] I do not mean to imply that these three measures are all that is required to create a healthy agriculture in the Soviet Union. Others, such as increased investment, may be as, or even more, important, but they are not relevant to this paper. Recent agricultural reforms [16] [17] [18] [21] [22] [23] [24] have raised prices on agricultural products, reduced prices on machinery and vehicles, cut taxes, promised more investment financed by the state, short- and long-term credit, and greater freedom to peasants in decision-making (see note 5). But there was no mention of the imposition of rent as such. Since the Soviet constitution assures the kolkhozes of the free use of their land, the imposition of a formal rent would require a major change which would encounter ideological difficulties.

(1a) $$v = -\frac{\pi}{x_n} = -\frac{\sum_{1}^{n-1} p_i x_i - R}{x_n},$$

(2a) $$f(x_1, \cdots, x_n) = 0,$$

and form with a Lagrange's multiplier

(3a) $$z = -\frac{\pi - \lambda f(x_1, \cdots, x_n)}{x_n}.$$

Equating $\partial z/\partial x_i = 0$ $(i = 1, \cdots, n)$ we obtain

(4a) $$\lambda f_i = p_i \qquad (i = 1, \cdots, n-1),$$

(5a) $$\lambda f_n x_n = -\pi.$$

To derive second-order conditions, take

(6a) $$dz = -\frac{x_n d\pi - \pi dx_n}{x_n^2},$$

(7a) $$d^2 z = -\frac{-\pi x_n d^2 x_n - 2x_n d\pi dx_n + 2\pi dx_n^2}{x_n^3},$$

on the assumption that x_n is the only dependent variable, so that

$$d^2\pi = \sum_{1}^{n-1} p_i d^2 x_i = 0.$$

From (2a) and (7a),

(8a) $$d^2 z = -\frac{\pi x_n \sum_{1}^{n}\sum_{1}^{n} f_{ij} dx_i dx_j - 2f_n x_n d\pi dx_n + 2f_n \pi dx_n^2}{f_n x_n^3}.$$

From (1a) and (4a)

(9a) $$d\pi = \lambda \sum_{1}^{n-1} f_i dx_i.$$

Substituting (5a) and (9a) into (8a), we obtain

(10a) $$d^2 z = \frac{\lambda}{x_n^2}\left(x_n \sum_{1}^{n}\sum_{1}^{n} f_{ij} dx_i dx_j + 2dx_n \sum_{1}^{n-1} f_i dx_i + 2f_n dx_n^2\right);$$

(11a) $$d^2 z = \frac{\lambda}{x_n}\left(\sum_{1}^{n}\sum_{1}^{n} f_{ij} dx_i dx_j + 2\frac{dx_n}{x_n}\sum_{1}^{n} f_i dx_i\right),$$

which can be expressed as

$$d^2z = \frac{\lambda}{x_n}\Big[f_{11}dx_1^2 + f_{12}dx_1dx_2 + \cdots \left(f_{1n} + \frac{f_1}{x_n}\right)dx_1dx_n$$

$$+ f_{12}dx_1dx_2 + f_{22}dx_2^2 + \cdots \left(f_{2n} + \frac{f_2}{x_n}\right)dx_2dx_n$$

$$\text{(12a)} \qquad + \cdots\cdots\cdots\cdots\cdots\cdots$$

$$+ \left(f_{1n} + \frac{f_1}{x_n}\right)dx_1dx_n + \left(f_{2n} + \frac{f_1}{x_n}\right)dx_2dx_n$$

$$+ \cdots \left(f_{nn} + \frac{2f_n}{x_n}\right)dx_n^2\Big],$$

subject to

$$\text{(13a)} \qquad \sum_1^n f_i dx_i = 0.$$

Since $\lambda/x_n < 0$ (because $x_n < 0$), second order conditions for a maximum of of d^2z require that the determinant

$$\text{(14a)} \quad D = \begin{vmatrix} 0 & f_1 & f_2 & \cdots & f_n \\ f_1 & f_{11} & f_{12} & \cdots & \left(f_{1n} + \frac{f_1}{x_n}\right) \\ f_2 & f_{12} & f_{22} & \cdots & \left(f_{2n} + \frac{f_2}{x_n}\right) \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ f_n & \left(f_{1n} + \frac{f_1}{x_n}\right) & \left(f_{2n} + \frac{f_1}{x_n}\right) & \cdots & \left(f_{nn} + \frac{2f_n}{x_n}\right) \end{vmatrix} < 0.$$

Multiplying the first row by $1/x_n$ and subtracting it from the last, and performing the same operation on the first and the last columns as well, we reduce D to the well known "bordered" determinant F.

$$\text{(15a)} \qquad F = \begin{vmatrix} 0 & f_1 & f_2 & \cdots & f_n \\ f_1 & f_{11} & f_{12} & \cdots & f_{1n} \\ f_2 & f_{12} & f_{22} & \cdots & f_{2n} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ f_n & f_{1n} & f_{2n} & \cdots & f_{nn} \end{vmatrix} < 0.$$

Thus the second order conditons for the equilibrium of the co-op and of the capitalist firm are the same.

* * *

In order to find the reactions of the co-op to changes in rent and prices, equations (2a), (4a), and (5a) are differentiated in the usual manner. The solutions of the resulting system of equations prove to be functions of the determinant F and of its co-factors [3, p. 615]. So, of course, are the reactions of a capitalist firm to a change in price. Hence it is possible to express the reactions of the co-op in terms of the corresponding reactions of the twin, marked here with asterisks. As stated in the List of Symbols,

$$E^*_{x_ip_j} = \left(\frac{\partial x_i}{\partial p_j}\right)^* \cdot \frac{p_j}{x_i} \qquad (i, j = 1, \cdots, n),$$

all indicating capitalist reactions. No asterisk is required for x_i because the twin and co-op are originally in the same equilibrium position.

2. The Effects of a Change in R

The differentiation of (2a), (4a), and (5a) in respect to R gives the system of equations

$$\text{(16a)} \qquad \begin{aligned} &\sum_1^n f_i \frac{\partial x_i}{\partial R} = 0 \\ &f_1\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial R}\right) + \sum_1^n f_{1i}\frac{\partial x_i}{\partial R} = 0 \\ &f_2\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial R}\right) + \sum_1^n f_{2i}\frac{\partial x_i}{\partial R} = 0 \\ &\cdots\cdots\cdots\cdots\cdots\cdots \\ &f_n\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial R}\right) + \sum_1^n \left(f_{in} + \frac{f_i}{x_n}\right)\frac{\partial x_i}{\partial R} = \frac{1}{\lambda x_n}. \end{aligned}$$

Multiplying the first equation by $1/x_n$ and subtracting it from the last we reduce the latter to

$$\text{(17a)} \qquad f_n\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial R}\right) + \sum_1^n f_{in}\frac{\partial x_i}{\partial R} = \frac{1}{\lambda x_n}.$$

Now the system of equations (16a) and (17a) is based on the matrix given in (15a). We can immediately derive

$$\text{(18a)} \qquad \frac{\partial x_n}{\partial R} = \frac{1}{x_n}\cdot\frac{F_{nn}}{\lambda F} = \frac{1}{x_n}\left(\frac{\partial x_n}{\partial p_n}\right)^*,$$

where F_{ij} is the co-factor of f_{ij} in F, and

$$\text{(19a)} \qquad \frac{\partial x_i}{\partial R} = \frac{1}{x_n}\cdot\frac{F_{in}}{\lambda F} = \frac{1}{x_n}\left(\frac{\partial x_i}{\partial p_n}\right)^* \qquad (i \neq n).$$

Since

$$\left(\frac{\partial x_n}{\partial p_n}\right)^* > 0 \quad \text{and} \quad x_n < 0, \frac{\partial x_n}{\partial R} < 0, \quad \text{and} \quad \left(-\frac{\partial x_n}{\partial R}\right) > 0.$$

If x_i is an output and

$$\left(\frac{\partial x_i}{\partial p_n}\right)^* < 0, \frac{\partial x_i}{\partial R} > 0.$$

If x_i is a nonlabor input, the sign of $\partial x_i/\partial R$ depends on whether x_i is complementary to or substitutable for x_n.

3. The Effects of a Change in p_1

Differentiate (2a), (4a), and (5a) in respect to p_1, and using the same methods as in the preceding section obtain the system of equations

$$\text{(20a)} \qquad \begin{aligned} \sum_1^n f_i \frac{\partial x_i}{\partial p_1} &= 0 \\ f_1\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial p_1}\right) + \sum_1^n f_{1i}\frac{\partial x_i}{\partial p_1} &= \frac{1}{\lambda} \\ f_2\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial p_1}\right) + \sum_1^n f_{2n}\frac{\partial x_i}{\partial p_1} &= 0 \\ \cdots\cdots\cdots\cdots\cdots \\ f_n\left(\frac{1}{\lambda}\frac{\partial \lambda}{\partial p_1}\right) + \sum_1^n f_{in}\frac{\partial x_i}{\partial p_1} &= -\frac{x_1}{\lambda x_n}, \end{aligned}$$

again based on the matrix given in (15a). Hence,

$$\text{(21a)} \qquad \frac{\partial x_1}{\partial p_1} = \frac{F_{11}}{\lambda F} - \frac{x_1}{x_n}\cdot\frac{F_{1n}}{\lambda F} = \left(\frac{\partial x_1}{\partial p_1}\right)^* - \frac{x_1}{x_n}\left(\frac{\partial x_n}{\partial p_1}\right)^*,$$

$$\text{(22a)} \qquad \frac{\partial x_n}{\partial p_1} = \frac{F_{1n}}{\lambda F} - \frac{x_1}{x_n}\cdot\frac{F_{nn}}{\lambda F} = \left(\frac{\partial x_1}{\partial p_n}\right)^* - \frac{x_1}{x_n}\left(\frac{\partial x_n}{\partial p_n}\right)^*,$$

$$\text{(23a)} \qquad \frac{\partial x_2}{\partial p_1} = \frac{F_{12}}{\lambda F} - \frac{x_1}{x_n}\cdot\frac{F_{2n}}{\lambda F} = \left(\frac{\partial x_1}{\partial p_2}\right)^* - \frac{x_1}{x_n}\left(\frac{\partial x_n}{\partial p_2}\right)^*.$$

By multiplying both sides of expressions (21a)–(23a) by the proper variables, they can be expressed in a more convenient form:

$$\text{(24a)} \qquad E_{x_1p_1} = E^*_{x_1p_1} - E^*_{x_np_1},$$

$$\text{(25a)} \qquad E_{x_np_1} = \frac{u_1}{u_n}\left(E^*_{x_1p_n} - E^*_{x_np_n}\right),$$

(26a) $$E_{x_2p_1} = \frac{u_1}{u_2}(E^*_{x_1p_1} - E^*_{x_np_1}).$$

It is important to note that $u_i = p_i x_i < 0$ when x_i is an input.

Two assumptions are made here: (1) a given relative increase (or decrease) in all inputs in the twin results in a smaller relative increase (or decrease) in outputs taken as a whole, either because the production function has this attribute throughout the relevant range, or because the firm makes a profit (with $R>0$) and hence operates to the right of the maximum average product point; (2) the absence of Hicks's "regression," so that $E^*_{x_ip_j}<0$ if x_i is an output, x_j an input, and $E^*_{x_ip_j}>0$ if x_i is an input, x_j an output.

Actually, the presence of regression would weaken some results while strengthening others. It does not seem to me that the problem in hand is sufficiently important to warrant additional explorations of cases arising from the great variety of relationships among inputs and outputs possible in a generalized production function (even subject to the constraint given in (15a)). Only the more probable cases are considered here.

(*a*) *One output, labor the only input.* In (24a), $E^*_{x_1p_1} < E^*_{x_np_1}$ because of diminishing returns; therefore, $E_{x_1p_1}<0$ and output contracts. So does the labor input.

(*b*) *One output, several inputs.* The general case is indeterminate. We can establish the sign of $E_{x_1p_1}$ in (24a) by making the plausible assumption that the isoquants are radially parallel to each other, so that a change in price (p_1) of the output leaves input ratios unchanged, or that $E^*_{x_ip_1} = E^*_{x_jp_1}$ $(i, j = 2, \cdots n)$.

From (2a),

(27a) $$\sum_1^n p_i \left(\frac{\partial x_i}{\partial p_1}\right)^* = 0.$$

(28a) $$p_i\left(\frac{\partial x_i}{\partial p_1}\right)^* = p_i\left(\frac{\partial x_i}{\partial p_1}\right)^* \cdot \frac{p_1 x_i}{p_1 x_i} = \frac{u_i E^*_{x_ip_1}}{p_1}.$$

(29a) $$\sum_1^n u_i E^*_{x_ip_1} = 0.$$

If now

$$E^*_{x_ip_1} = E^*_{x_jp_1} = E^*_{x_np_1} \qquad (i, j = 2, \cdots, n),$$

(30a) $$u_1 E^*_{x_1p_1} + E^*_{x_np_1} \cdot \sum_2^n u_i = 0,$$

(31a) $$E^*_{x_np_1} = -\frac{u_1 E^*_{x_1p_1}}{\sum_2^n u_i}.$$

The substitution of (31a) into (24a) gives

$$E_{x_1p_1} = \frac{\sum_1^n u_i}{\sum_2^n u_i} \cdot E^*_{x_1p_1}. \qquad (32a)$$

Now,

$$\sum_1^n u_i > 0,\ E^*_{x_1p_1} > 0, \quad \text{and} \quad \sum_2^n u_i < 0$$

because $x_i < 0$ $(i = 2, \cdots, n)$. Therefore $E_{x_1p_1} < 0$.

In (25a) both $E^*_{x_1p_n} < 0$ and $E^*_{x_np_n} < 0$, but because of the presence of other inputs (and diminishing returns) $|E^*_{x_1p_n}| < |E^*_{x_np_n}|$. It follows that $E_{x_np_1} < 0$ (because $u_n < 0$).

For the sign of $E_{x_2p_1}$ in (26a) see section (c–iv) below.

(*c*) *Several inputs and outputs; x_1 is an output.*

(i) *The sign of $E_{x_1p_1}$ in (24a).* If x_1 is a very important user of labor, diminishing returns may make $E^*_{x_1p_1} < E^*_{x_np_1}$ (though this also depends on the behavior of other inputs and outputs), and $E_{x_1p_1} < 0$, similar to the two preceding cases. But if x_1, being one of the several outputs, uses only a moderate part of total labor, then $E^*_{x_1p_1} > E^*_{x_np_1}$ and $E_{x_1p_1} > 0$.

(ii) *The sign of $E_{x_np_1}$ in (25a).* $E^*_{x_np_n} < 0$ and, by assumption, $E^*_{x_1p_n} < 0$. In the presence of other inputs (and diminishing returns), we can expect $|E^*_{x_1p_n}| < |E^*_{x_np_n}|$ and $E_{x_np_1} < 0$. But if wages constitute the major part of the cost of x_1 and if the value of the marginal product in the production of x_1 declines more slowly than in other uses of labor, it is possible that the sign of $E_{x_np_1}$ may be reversed.

(iii) *The sign of $E_{x_2p_1}$ in (26a), x_2 being an output.* Let x_2 be at first all other outputs, or the only other output. Assume $E^*_{x_np_2} > 0$. If x_1 and x_2 are produced in constant proportions, the result is the same as if x_1 were the only output given in (b) above. If there is some, but not perfect, complementarity between x_1 and x_2 the result is indeterminate unless we assume that all ratios among inputs in the twin remain constant in spite of a change in p_2, in which case diminishing returns will give $E^*_{x_2p_2} < E^*_{x_np_2}$. Since $0 < E^*_{x_1p_2} < E^*_{x_2p_2}$, $E_{x_2p_1} < 0$. If x_1 and x_2 are competing outputs, $E^*_{x_1p_2} < 0$ and $E_{x_2p_1} < 0$.

In general, a rise in p_1 is likely to increase the output of x_1 and reduce the input of labor as shown above. Hence a reduction in other outputs taken as a whole is highly probable.

If x_2 is one of several outputs and is competitive with x_1, $E_{x_2p_1} < 0$. Otherwise, the result is indeterminate.

(iv) *The sign of $E_{x_2p_1}$ in (26a), x_2 being an input.* Assume $E^*_{x_1p_2} < 0$. If x_2 is highly complementary with x_n, it is likely (but not certain) that $|E^*_{x_1p_2}| < |E^*_{x_np_2}|$ and $E_{x_2p_1} < 0$. If the complementarity is slight, the result is indeterminate. If x_2 and x_n are substitutes, $E^*_{x_np_2} > 0$ and $E_{x_2p} <_1 0$.

(*d*) *Several inputs and outputs: x_1 is an input.*

(i) *The sign of $E_{x_1p_1}$ in* (24a). Here $E^*_{x_1p_1}<0$. If there is some complementarity between x_1 and x_n, $|E^*_{x_1p_1}|>|E^*_{x_np_1}|$ and $E^*_{x_1p_1}<0$, unless they are used in constant porportions, in which case $E_{x_1p_1}=0$. If x_1 and x_n are substitutes, $E^*_{x_np_1}>0$ and $E_{x_1p_1}<0$.

(ii) *The sign of $E_{x_np_1}$ in* (25a). Here $E^*_{x_1p_1}<0$. With some complementarity between x_1 and x_n, $|E^*_{x_1p_n}|<|E^*_{x_np_n}|$ and $E_{x_np_1}>0$. If they are substitutes, $E^*_{x_1p_n}>0$ and again $E_{x_np_1}>0$. Only when x_1 and x_n are used in constant proportions $E_{x_np_1}=0$. So the increase in the price of any nonlabor input, even when complementary with labor, increases the use of labor (except for constant proportions).

(iii) *The sign of $E_{x_2p_1}$ in (26a), x_2 being an output or an input.* Unless specific assumptions are made about the interrelationships among the variables involved, the result is indeterminate.

* * *

A comment on the effects of the magnitude of R on $E_{x_1p_1}$ and $E_{x_np_1}$ in (24a) and (25a). Since $p_n=v$, we obtain from (1a)

$$p_nx_n = -\left(\sum_1^{n-1} p_ix_i - R\right). \tag{33a}$$

Differentiating both sides of this expression in respect to p_1 and taking advantage of

$$\sum_1^n p_i\cdot\frac{\partial x_i}{\partial p_1} = 0$$

in equilibrium [3, p. 615], we find that

$$\frac{\partial p_n}{\partial p_1} = -\frac{x_1}{x_n}, \tag{34a}$$

and therefore

$$\frac{\partial p_n}{\partial p_1}\cdot\frac{p_1}{p_n} = \frac{p_1x_1}{\sum_1^{n-1} p_ix_i - R}. \tag{35a}$$

Thus a larger R makes p_n more sensitive to a rise in p_1 and hence causes a greater reduction in labor input in response to a given rise in p_1. So $|E_{x_np_1}|$ is increased. But on $E_{x_1p_1}$ in (24a) R does not have a clear effect, because a large R implies a small payroll for the twin and a reduction in the importance of labor cost. It is possible that the magnitude of R has no effect on $E_{x_1p_1}$ at all, and this is indeed the case with a Cobb-Douglas production function consisting of one output and of several inputs, and subject to decreasing returns to scale.

REFERENCES

1. F. ABRAMOV, "Vokrug da okolo," *Neva*, No. 1, 1963, 109–37. Translated into English under the title of *One Day in the "New Life."* New York 1963.

2. R. G. D. ALLEN, *Mathematical Analysis for Economists*, New York 1939, pp. 350–83, 461–65, 495–520.
3. ———, *Mathematical Economics*, London 1956, pp. 472–79, 613–17.
4. H. AVERCH AND L. JOHNSON, "The Firm under Regulatory Constraint," *Am. Econ. Rev.*, Dec. 1962, *52*, 1052–69.
5. W. J. BAUMOL, *Business Behavior, Value and Growth*. New York 1959.
6. J. R. HICKS, *Value and Capital*, 2nd ed., Oxford 1946, pp. 96–98.
7. F. LUTZ AND V. LUTZ, *The Theory of Investment of the Firm*. Princeton 1951.
8. J. R. MILLAR, *Income and Price Formation in the Soviet Collective Farm Sector since 1953*. Unpublished doctoral dissertation, Cornell Univ., 1965.
9. B. P. ROZHIN, *Nekotorye voprosy pod"ema ekonomiki slabykh kolkhozov*. Moscow 1961.
10. T. SCITOVSKY, "A Note on Profit Maximization and Its Implications," *Rev. Econ. Stud.*, Winter 1943, *11*, 57–60; reprinted in *The Readings in Price Theory*, Am. Econ. Assoc., Homewood, Ill., 1952, pp. 352–60.
11. M. K. SHERMENEVA, *Finansy i kreditovanie sel'skokhoziaistvenykh predpriiatii*, Moscow 1963, pp. 172–79.
11a. M. I. TUGAN-BARANOVSKY, *Sotsial'nyia osnovy kooperatsii*, Berlin 1921, pp. 237–56.
12. B. WARD, "The Firm in Illyria: Market Syndicalism," *Am. Econ. Rev.*, Sept. 1958, *48*, 566–89.
13. F. M. WESTFIELD, "Regulation and Conspiracy," *Am. Econ. Rev.*, June 1965, *55*, 424–43.
14. AKADEMIIA NAUK SSSR, INSTITUT EKONOMIKI, *Material'noe stimulirovanie razvitiia kolkhoznovo proizvodstva*, Moscow 1963, pp. 54–56.
15. "The Calculation of Collective Farm Production Costs," *Kommunist*, Sept. 1962, No. 13, 46–59, as reported in *The Current Digest of the Soviet Press*, Oct. 10, 1962, *14*, 3–11.
16. *Izvestiia*, March 27, 1965, pp. 1–3.
17. ———, April 11, 1965, p. 1.
18. ———, April 13, 1965, pp. 1–2.
19. NAUCHNO-ISSLEDOVATEL'SKII FINANSOVYI INSTITUT, *Denezhnye dokhody kolkhozov i differentsial'naia renta*. Moscow 1963.
20. *The New York Times*, March 28, 1965, *114*, 1 and 6.
21. *Pravda*, Nov. 7, 1965, p. 2.
22. ———, Dec. 26, 1965, pp. 1–2.
23. ———, Jan. 5, 1966, p. 2.
24. ———, Feb. 20, 1966, p. 3.
25. TSENTRAL'NOE STATISTICHESKOE UPRAVLENIE PRI SOVETE MINISTROV RSFSR, *Narodnoe khoziaistvo RSFSR v 1963 godu. Statisticheskii ezhegodnik*, pp. 316–18.
26. TSENTRAL'NOE STATISTICHESKOE UPRAVLENIE PRI SOVETE MINISTROV SSSR, *Narodnoe khoziaistvo SSSR v 1963 godu. Statisticheskii ezhegodnik*, p. 347.

MEASUREMENT OF POTENTIAL OUTPUT

By EDWIN KUH*

Ever since the *General Theory* first appeared, economists and policy-
ıakers have been concerned with measuring full employment by ad-
ısting official labor force statistics to a norm that would satisfy theo-
:tical and practical notions of full employment.

A twin concept, potential (aggregate) output attainable at full em-
loyment, is related to the fully employed labor force through a produc-
on function. While the greater complexity of the potential output
ɔncept has restricted most discussion to hypothetical textbook exam-
les, it has recently become a part of the Council of Economic Advisers'
5] [7] aggregative analysis. Current interest in growth models has also
·d to measurement of potential output [12] [19] [25].

In this paper a structural method for estimating potential output is
resented and compared with two alternative techniques; the main
ements of this subsystem include a production function, a labor force
articipation equation, and a policy restriction which stipulates that
nemployment shall be, for example, 4 per cent (or 3 per cent, or . . .).[1]
abor participation rates increase when employment opportunities are
ɔod and decline when employment drops, according to studies by
owen and Finegan [3], Lebergott [17], Mincer [18], Strand and
'ernburg [26], and Tella [27].

I. *Measuring Potential Output with Cyclical Data*

. *Dynamic Production Functions*

Estimates of production functions from data subject to cyclical fluc-
ıations pose severe practical problems. It has been widely observed
:] [5] [9] [11] [13] [14] [22] that average labor productivity falls
hen output declines and rises when output rises, contrary to the usual

* The author is professor of economics and finance at Massachusetts Institute of Technology.
:lpful comments on an earlier draft were made by A. Okun, R. M. Solow, and M. Spiro. Re-
.rch support by the Ford Foundation to the Sloan School of Management and the Economics
partment is gratefully acknowledged. The author is also indebted to the National Science
undation's grant to the Economics Department. Peter Hughes provided valuable research
istance.

The general analytic approach is similar to that of Locke Anderson and Jarvis Babcock
Their emphasis, which is on closing "the" gap, is different from mine. There are clear
ferences in their formulation of the behavior equations which lead to significantly different
icy implications. W. A. W. Godley and J. R. Shepherd [8] have applied a related approach
the British economy. Their treatment of labor supply is more elaborate in some important
ɔects, but slurs over direct estimation of participation rates by relating potential output
unemployment.

short-run theory of diminishing returns (average or marginal, but certainly the latter). While various explanations can be adduced to explain this behavior, one important cause is the lagged response of employment to short-run variations in output. This phenomenon is especially evident in nonproduction-worker employment, since much of this "overhead" personnel is associated with management, supervision, and other tasks which will not be rapidly adjusted to what are deemed to be only temporary fluctuations in labor requirements. Production cost changes in the spirit of production, inventory, and employment-smoothing suggested by optimizing linear decision rules [10] [23] similarly imply lagged adjustments of employment to output changes. A second significant possibility is that of increasing returns to scale over broad ranges of output. While it would be a mistake to rule out this possibility altogether (and several others discussed elsewhere [14] [15]), empirical evidence points to the lagged adjustment of employment to output as the single most important short-run element.

One simple dynamic production function that reflects these employment lags can be represented in the following way:

$$X_t = \beta_0 + \beta_1 X_{t-1} + \beta_2 E_t + \beta_3 E_{t-1} + \beta_4 t + \beta_5 K_{t-1} + \epsilon_t, \tag{1}$$

where X is output, E is employment, K is capital stock, t is a time trend, and ϵ the regression-error term.

The steady-state solution of (1) (where a bar represents a fixed value) is:

$$\overline{X} = \frac{\beta_0}{1 - \beta_1} + \frac{\beta_2 + \beta_3}{1 - \beta_1}\overline{E} + \frac{\beta_4}{1 - \beta_1}\bar{t} + \frac{\beta_5}{1 - \beta_1}\overline{K}. \tag{2}$$

This is a linear approximation to some unknown production relation in which output is a function of the usual inputs, capital and labor, plus a trend term to represent disembodied technical change.[2] The initial effect of an increase in employment reflected in β_2 will naturally be positive. When the employment response is rapid, the coefficient of lagged employment, β_3, will be negative; on this account the intermediate and longer-run elasticities of output (two or more quarters) with respect to labor input will be lower than the impact, one-quarter elasticity.

One modification of equation (1) will be made in the empirical estimates. The capital stock term has been dropped from the estimated equations that are presented, because of collinearity with trend and the inferior quality of measured aggregate capital stock. Since investment is partly the vehicle for technical change [19] [21] [25], this deficiency should be remedied for *ex ante* policy calculations. However, this neglect does not preclude the calculation of historical gaps or the demon-

[2] When data are logarithms, we once more have the Cobb-Douglas function.

stration of the main methodological points. Furthermore, Solow [24] has shown that if production were to take place under a regime of (*ex post*) fixed proportions, and if there is more than enough capital to match with the existing labor supply, technical change represented by trend should appear in the production function, but capital would not.

B. *Empirical Estimates*

The largest output aggregate that seems appropriate for production-function estimation, called Gross Business Product, is measured by constant-dollar GNP, exclusive of outputs that are not related to inputs in the usual spirit of a production function. One obvious example of such exceptions would be imputed rents on owner-occupied dwellings. Similarly, the government, households and institutions, and rest-of-world sectors have been excluded. Agriculture has also been left out because labor input and labor supply measurement problems are complicated by the extent of secondary family workers. Gross Business Product comprises about 73 per cent of GNP.

A logarithmic version of equation (1) has two advantages over the arithmetic linear approximation: an exponential trend would imply a more rapid increase in potential output toward the end of the period than the linear relation and, in addition, it will be consistent with the view that labor's marginal productivity is an increasing function of time. Both propositions are theoretically preferable.[3]

$$\begin{aligned}(3)\quad \log X_t &= \underset{(.00038)}{.00158}\,\text{TIME} + \underset{(.262)}{1.808}\log E_t - \underset{(.208)}{1.534}\log E_{t-1} \\ &\quad + \underset{(.115)}{.667}\log X_{t-1} + \underset{(.406)}{.814} \\ \bar{R}^2 &= .987 \qquad \bar{S}_e = .0140 \qquad \text{D.W.} = 2.044\end{aligned}$$

Fitted to quarterly data 1949-II to 1962-IV.

The corresponding steady-state solution:

$$(4)\qquad \log X_t^* = .0190\ \text{TIME} + .823 \log E_t^* + 2.442.$$

One other theoretical and econometric aspect, the structural source of the distributed lag, needs further consideration. In full generality, distributed lags in both output and employment could be "caused" by employment lagging output, output lagging employment, or both. This is a matter of some relevance, since which variable will be considered as dependent, and which the forcing function, will be determined by the choice.[4]

[3] To conserve space, linear estimates are not presented. Where the results differed, the logarithmic version seemed most reasonable.

[4] Estimates with employment the dependent variable were made, and the resultant steady-state solution solved for output as the left-hand side variable. Potential output estimates were

Industries which have a substantial reservoir of underutilized labor will tend to have employment lag behind output, so that if output increases, employment levels will be gradually adjusted to the new higher level. If, on the other hand, there is only negligible slack in the labor supply, output changes must lag behind changes in employment, at least when output is increasing. Finally, where in-process inventory is large relative to the production rate, the more likely it is that output will lag behind employment. Shipbuilding provides a clear-cut example of this latter phenomenon. Because this matter cannot be settled readily at an aggregate level, requiring as it does a detailed understanding of industry structure, I shall only report results for production functions estimated with output as the dependent variable.

The response of output to labor input in equation (3) for various periods of time has been summarized in Table 1. The impact elasticity of output with respect to employment is about 1.80, a magnitude that will reappear later on when comparing this approach with several others. The cumulative two-quarter elasticity is much closer to the long-run steady-state elasticity. When the output-employment adjustment is completed, the elasticity declines sharply to .82, a magnitude that is clearly consistent with constant returns, but which does not imply this regime.[5] This illustrates the obvious pitfall of using cyclical data which do not explicitly include an appropriate lag structure. My own analysis for measuring the output gap uses only the steady-state solution of equation (4) to measure potential output.

II. *Labor Participation Equations*

Within the past five years, the sensitivity of the labor force to employment opportunities has become widely recognized; it seems that for about each additional worker fired during a slump, two workers leave the labor force. Thus unemployment increases by half as much as the decrease in employment. Women, young men, and older men are the most sensitive to labor market opportunities in the manner suggested, which has come to be known as the "discouraged worker" hypothesis.

An alternative hypothesis asserts a contrary tendency, on the grounds that secondary family workers will enter the labor market when primary workers lose their jobs, to offset the income losses thus incurred. The dominant gross relation clearly supports the "discouraged worker"

typically about \$5–\$10 billion higher in this form. With few exceptions, potential output gaps (for a given participation equation) were smallest for output dependent variable linear form and then, in ascending order, output dependent variable, log form, employment dependent variable linear form, employment dependent variable, log form.

[5] A more complete treatment and discussion of these phenomena in their most relevant context can be found in E. Kuh [14]. Strong possibilities of increasing returns to scale are implied by the analogous estimates for the manufacturing sector.

TABLE 1—CUMULATIVE ELASTICITY OF OUTPUT WITH RESPECT TO EMPLOYMENT[a]

Period	Current	1st Quarter	2nd Quarter	Long Run
Elasticity	1.81	1.48	1.27	.82

[a] These elasticities show the cumulative per cent output response to a steadily maintained 1 per cent increase in the level of employment.

hypothesis according to the Bowen and Finegan [3] cross-section study, and the most immediately relevant time-series estimates by Lebergott [17], Tella [27], and Strand and Dernberg [26]. I have chosen variants of the Tella relation over the preferred relation of Strand and Dernberg, although the different authors are in general agreement on much of the basic analysis.[6]

The participation equations chosen for subsequent analysis relate the labor force participation rate primarily to employment divided by population. In addition, either the reciprocal of population (a trend-like variable) or the lagged dependent variable has been included. The latter variable should reflect the lagged response of participation to variations in the employment-population ratio.

$$\left(\frac{L}{P}\right)_t = \alpha_0 + \alpha_1\left(\frac{E}{P}\right)_t + \epsilon_{1t}, \tag{5a}$$

$$\left(\frac{L}{P}\right)_t = \alpha_0' + \alpha_1'\left(\frac{E}{P}\right)_t + \alpha_2'\left(\frac{L}{P}\right)_{t-1} + \epsilon_{2t}, \tag{5b}$$

$$\left(\frac{L}{P}\right)_t = \alpha_0'' + \alpha_1''\left(\frac{E}{P}\right)_t + \alpha_2''\frac{1}{P_t} + \epsilon_{3t}. \tag{5c}$$

E is employment for individuals 14 years and over in the business sector; P is civilian noninstitutional population 14 years and over; L, the labor force associated with the business sector, includes the unemployed with previous experience in the business sector as well as workers with no previous experience. The variables appear in ratio form because collinearity among the explanatory variables prevents sensible estimates in undeflated form. Since the deflator is approximately linearly and homo-

[6] Strand and Dernberg [22] have also included a variable intended to measure the secondary worker hypothesis. This variable is the ratio of unemployment benefit exhaustions to unemployment for the period two months in the future beyond the current-period participation rate dependent variable. This ingenious representation substantially increases the amount of variation explained and is highly significant statistically, both for the aggregate and in separate regressions for different age-sex groups (in a presently unpublished paper). It seems reasonable to avoid a strong commitment to this formulation because of its proxy nature, despite the impressive statistical performance. This construction has been sharply criticized by Mincer [18].

geneously related to the labor force and employment over periods of the length considered, significance tests will not be biased on this account [16].[7]

Separate participation responses by various age-sex categories represent a clear improvement over these global estimates [28], but the basic objective of this paper is well enough served by the aggregate equations.

Variable	Definition	Mean
E/P	employment to population ratio	.3464
L/P	labor force to population ratio	.3719
$1/P$	inverse of population	.0082

$$\left(\frac{L}{P}\right)_t = \underset{(.031)}{.283} + \underset{(.090)}{.257}\left(\frac{E}{P}\right)_t \tag{6a}$$

$$\bar{R}^2 = .158 \quad \bar{S}_e = .0037 \quad \text{D.W.} = .347$$

$$\left(\frac{L}{P}\right)_t = \underset{(.035)}{.047} + \underset{(.057)}{.123}\left(\frac{E}{P}\right)_t + \underset{(.094)}{.760}\left(\frac{L}{P}\right)_{t-1} \tag{6b}$$

$$\bar{R}^2 = .693 \quad \bar{S}_e = .0022 \quad \text{D.W.} = 1.35$$

$$\left(\frac{L}{P}\right)_t = \underset{(.027)}{.292} + \underset{(.086)}{.385}\left(\frac{E}{P}\right)_t - \underset{(1.811)}{6.524}\left(\frac{1}{P}\right)_t \tag{6c}$$

$$\bar{R}^2 = .364 \quad \bar{S}_e = .0032 \quad \text{D.W.} = 0.47.$$

Period of fit for three equations is 1953-III through 1962-IV.

Estimates of the participation response to employment vary considerably, depending upon which formulation is chosen. The slope of .26 in equation (6a) contrasts sharply with that of .38 in equation (6c), which includes the trend representation of $1/P_t$, and the steady-state response of .50 from equation (6b). Statistical properties of the last two equations are superior to those of the first, although much variance remains unexplained and first-order serial correlation is substantial, suggesting perhaps that the short-run dynamics have not been properly specified. Readers are referred to the basic research cited, to which full justice has not been done here. Nevertheless, it seems that further investigation is required before much confidence can be placed in any particular param-

[7] Anderson and Babcock [1], unlike Strand and Dernberg, and my own treatment, included armed forces in labor force and employment. I have not done so, since participation rates behaved in an unusual manner during Korea and there is no reason to expect similar behavior to prevail in time of peace. Therefore, the participation regressions were fitted from 1953-II until 1962-IV. Inclusion of armed forces after 1953 would have little effect on the estimated slope parameters.

eter estimate. The policy implications of the alternative participation equation forms for potential output differ significantly. This does not impede their usefulness in illustrating the methods developed in this paper. Mincer [18] suggests that an upward bias might exist in the time-series estimates. The smallest participation response, that recorded in (6a), could be viewed as least subject to bias when results are evaluated. Because this estimate neglects lagged responses, however, one might just as readily conceive that this opposite bias dominates.[8]

III. *The Full-Employment Gap*

Having estimated a production function and several participation equations, all that remains is to combine them in a relevant manner. The full-employment-policy restriction, equation (8), and one participation equation (for example, the linearized version of equation (5a) represented here as equation (7)), are solved simultaneously:

$$L_t^* = \beta_1 P_t + \beta_2 E_t^* + \nu, \tag{7}$$

$$E_t^* = \lambda L_t^*. \tag{8}$$

Substituting equation (7) into equation (8) yields equation (9):

[8] Partial coverage of the labor force (the government sector, agriculture, and some services are excluded) might prove to be a serious shortcoming of the entire analysis. Several circumstances mitigate this defect. First, the estimated parameters when adjusted for coverage, are not greatly different from those obtained for the entire labor force. Second, coverage is a substantial 69 per cent of the total, so that intrasectoral movements are likely to dominate intersectoral movements. As a particular sector becomes a smaller fraction of the total labor force, however, calculations of this character become increasingly suspect.

Using seasonally adjusted quarterly data and Civilian Labor Force data instead of the subsector labor force yielded the following results that can be compared with those in the text, and the nearly equivalent formulation of Strand and Dernberg.

$$\text{(a)} \quad \left(\frac{L}{P}\right)_t = \underset{(.039)}{.335} + \underset{(.074)}{.421}\left(\frac{E}{P}\right)_t$$

$$\bar{R}^2 = .387 \quad \bar{S}_e = .0046 \quad \text{D.W.} = .38$$

$$\text{(b)} \quad \left(\frac{L}{P}\right)_t = \underset{(.044)}{.075} + \underset{(.059)}{.195}\left(\frac{E}{P}\right)_t + \underset{(.092)}{.680}\left(\frac{L}{P}\right)_{t-1}$$

$$\bar{R}^2 = .722 \quad \bar{S}_e = .0031 \quad \text{D.W.} = 1.82$$

$$\text{(c)} \quad \left(\frac{L}{P}\right)_t = \underset{(.043)}{.310} + \underset{(.099)}{.523}\left(\frac{E}{P}\right)_t - \underset{(2.259)}{3.431}\frac{1}{P_t}$$

$$\bar{R}^2 = .414 \quad \bar{S}_e = .0046 \quad \text{D.W.} = .39$$

Period of Fit: 1954-I to 1959-IV.

An upward bias arising from common sampling errors in labor force and employment data should also be mentioned. Its magnitude is likely to be small (about .02, with a maximum of .045) at existing levels of sampling error, so that further adjustment on this account seems needless.

$$E_t^* = \frac{\lambda\beta_1}{1 - \lambda\beta_2} P_t. \tag{9}$$

This full-employment labor quantity is then substituted into the steady-state production function, equation (10),

$$\log X^* = \alpha_1 \log E^* + \alpha_2 t + \alpha_3, \tag{10}$$

which provides an estimate of full employment or potential output.

The full-employment parameter λ requires comment. It has been chosen by the Council of Economic Advisers to be .96 as a provisional policy objective:

> The Council's estimate of Potential GNP reflects the belief that the economy could operate at a 4 per cent unemployment rate today without substantial strains on either labor supplies or plant capacity [7, p. 83].

The Council's choice of $\lambda = .96$ is stated to be "an interim target" [7, p. 81]. Evidently, this concept of full employment depends, as in all good sense it must, on considerations other than maximum attainable output. In particular, it appears to embody a widely held political and economic judgment that with existing wage-price behavior relations and balance-of-payments problems, undesirable inflation might ensue if unemployment were to drop below 4 per cent for an extended period. I shall adhere to this 4 per cent convention in order to make comparisons with the Council's analysis and other similar calculations.

Because the business sector data are not directly comparable with the Council's GNP gap figures, a rough attempt has been made to remedy this deficiency. The procedure is decidedly crude, so that the resulting comparisons should be treated with appropriate reserve. Since most slack occurs in the business sector (government, domestic service, and agriculture, in particular, not being prone to cyclical disturbances), the strong assumption was made that *all* slack exists in the business sector, so that actual nonbusiness sector output is assumed equal to its potential output. On this basis, columns (4)–(6) of Table 2 were derived from the data underlying columns (1)–(3). Chart 1 portrays derived GNP output gaps for columns 4 through 6.

The more conservative output gaps are based on participation equations (6a) and (6c), while participation equation (6b), including the lagged dependent variable, indicates slightly larger gaps than the others. Generally speaking, the business sector gaps are larger than the Council gaps, shown in column (7), a partly fictitious result that to some extent has been counteracted by the Derived GNP gap calculations. In the 1954 recession the Derived GNP gaps were similar to the Council gap. During the period 1955-I/1962-II the CEA gap is consistently larger

TABLE 2—OUTPUT GAPS AS PER CENT OF POTENTIAL OUTPUT

Quarter-Year	Business Sector Gap			Derived GNP Gap			
	A (1)	B (2)	C (3)	A (4)	B (5)	C (6)	CEA (7)
2.49	12.4	13.1	10.8	9.2	9.7	8.0	
3.49	13.5	14.4	11.9	9.8	10.4	8.6	
4.49	14.4	15.4	12.9	10.5	11.3	9.4	
1.50	11.2	12.4	9.8	8.2	9.1	7.2	
2.50	6.1	7.0	4.9	4.6	5.2	3.6	
3.50	−.1	.4	−1.2	−.1	.3	−1.0	
4.50	.8	−.9	−.4	.6	−.7	−.3	
1.51	−2.0	.6	−3.0	−1.5	.5	−2.3	
2.51	−1.1	−.5	−2.0	−.8	−.4	−1.6	
3.51	.7	1.4	−.3	.5	1.0	−.2	
4.51	1.5	2.3	.6	1.1	1.7	.4	
1.52	1.3	2.0	.5	1.0	1.5	.4	
2.52	2.7	3.3	1.9	1.9	2.4	1.4	
3.52	2.4	3.2	1.7	1.8	2.4	1.2	
4.52	−1.5	−.6	−2.1	−1.1	−.5	−1.6	
1.53	−1.5	2.0	−2.0	−1.1	1.5	−1.5	−2.6
2.53	−1.0	−.5	−1.4	−.7	−.4	−1.1	−2.9
3.53	.4	1.1	0.0	.3	.8	.0	−1.3
4.53	4.4	5.1	4.0	3.2	3.7	2.9	1.3
1.54	7.0	7.6	6.6	5.0	5.5	4.8	3.3
2.54	7.3	7.7	7.0	5.2	5.6	5.0	4.6
3.54	7.3	7.7	7.0	5.2	5.6	5.0	4.7
4.54	5.3	6.1	5.1	3.9	4.4	3.7	3.7
1.55	1.9	2.5	1.8	1.4	1.9	1.3	1.5
2.55	−.7	−.1	−.8	−.5	−.1	−.6	.5
3.55	−1.4	−.8	−1.5	−1.1	−.6	−1.1	−.5
4.55	−2.2	−1.3	−2.2	−1.6	−1.0	−1.6	−.6
1.56	−1.2	−.5	−1.1	−.9	−.3	−.8	.8
2.56	−.8	0.0	−.7	−.6	0.0	−.5	1.6
3.56	.7	1.8	.9	.5	1.3	.7	2.1
4.56	−.5	.8	−.2	−.3	.6	−.1	1.6
1.57	−.7	.4	−.4	−.5	.3	−.3	1.5
2.57	.5	1.8	.9	.3	1.3	.6	2.2
3.57	1.2	2.7	1.7	.9	1.9	1.2	2.9
4.57	4.7	5.8	5.3	3.4	4.2	3.9	5.3
1.58	9.9	10.7	10.5	7.1	7.7	7.6	8.4
2.58	11.1	12.0	11.8	7.9	8.5	8.4	8.6
3.58	7.6	8.4	8.4	5.5	6.1	6.1	7.6
4.58	5.2	6.2	6.0	3.8	4.5	4.3	6.0

TABLE 2—(*Continued*)

Quarter-Year	Business Sector Gap			Derived GNP Gap			
	A (1)	B (2)	C (3)	A (4)	B (5)	C (6)	CEA (7)
1.59	2.8	3.6	3.6	2.1	2.7	2.7	5.0
2.59	0.0	.8	.8	0.0	.6	.6	3.2
3.59	2.7	3.6	3.7	2.0	2.7	2.7	5.4
4.59	4.0	4.9	5.0	2.9	3.6	3.6	5.6
1.60	1.0	1.6	2.0	.7	1.2	1.5	4.5
2.60	2.3	3.0	3.4	1.7	2.2	2.5	4.9
3.60	4.2	5.5	5.3	3.0	4.0	3.9	6.0
4.60	6.9	9.4	8.3	5.0	6.7	5.9	7.4
1.61	9.2	11.6	10.6	6.6	8.3	7.6	8.8
2.61	7.4	9.5	9.0	5.3	6.8	6.4	7.5
3.61	5.9	7.9	7.5	4.2	5.7	5.4	6.9
4.61	4.3	6.0	6.0	3.1	4.3	4.3	5.3
1.62	3.8	5.4	5.6	2.8	3.9	4.0	4.8
2.62	2.9	4.2	4.7	2.1	3.0	3.4	4.4
3.62	2.8	4.5	4.6	2.0	3.3	3.4	4.6
4.62	2.9	4.5	4.9	2.1	3.3	3.5	4.5

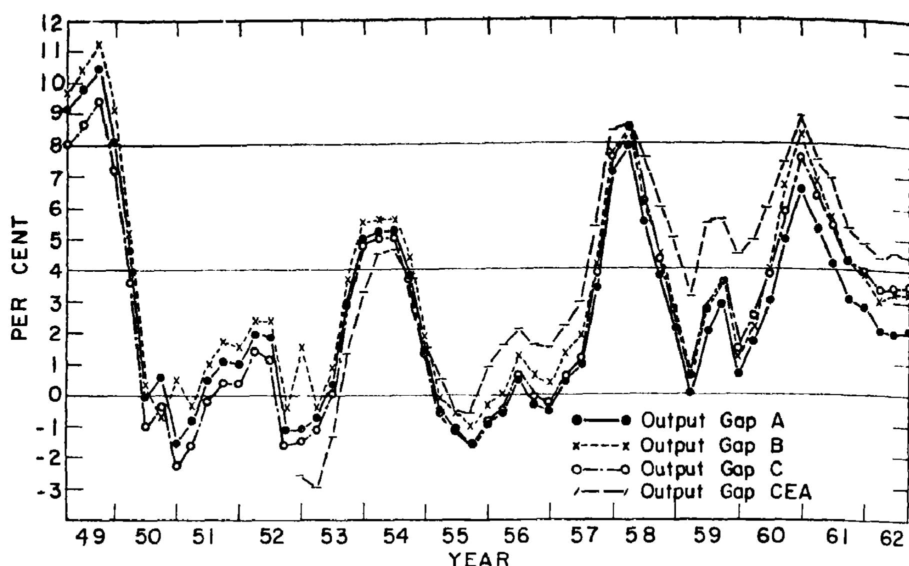
than the more conservative Derived gaps. The paths during the 1958 recession are highly similar although, in the 1958–59 recovery phase, my results would indicate that the gap had been largely eliminated while the Council gap never dropped below 3 per cent. When production functions using employment as the dependent variable are used to estimate Derived gaps (see footnote 4), the difference between Council gaps and Derived gaps disappeared. Since my own estimates are open to numerous valid objections, a definitive resolution of these differences must await later research results. Less highly aggregated production functions will surely be an indispensable element of more refined estimates.

IV. *Different Measures of Output and Employment Gaps*

A. *The Council of Economic Advisers*

While the Council of Economic Advisers uses economic analysis of production relations, they have also relied on an estimated exponential trend as a smoothing device to provide a quantitative summary of the main underlying economic determinants. An important element in the underlying calculation is the empirical generalization that "each percentage point of progress toward 4 per cent in the unemployment rate has meant a gain of roughly 3 per cent in total output in postwar periods of expansion" [6, p. 50]. About two-fifths of the output gap would be closed by lower unemployment and one-tenth by a larger labor force, in recognition of the participation response already discussed.

CHART 1. OUTPUT GAPS AS PER CENT OF POTENTIAL OUTPUT



The remaining half of the gap would have been closed, according to an early Council calculation [6], two-fifths by an improvement in productivity and one-tenth by longer hours per worker at higher utilization rates. The assumed short-run elasticity of output with respect to employment is about three. This accurately reflects short-run responses that are so important in assessing alternative paths to full employment, but apart from some permanent economy of scale gains, much of the productivity gain imputed to higher utilization would have disappeared in the new, steady-state full-employment equilibrium.[9]

B. *Okun Formulation*

The pathbreaking article of Okun [20] sought to take account of all the influences that have been mentioned by directly linking output to unemployment:

> Ideally, the measurement of potential output would appraise the various possible influences of high employment on labor input and productivity and evaluate the influences step-by-step, developing quantitative estimates for each adjustment to produce the desired measure of potential. While I shall discuss the steps individually below, the basic technique I am reporting consists of a leap from the unemployment rate to potential output rather than a series of steps involving the sev-

[9] Without investigating the point here, I suspect that some component of the longer hours per worker would vanish in a new higher employment steady state, since variations in hours are a dominant method of adjusting labor input to temporary variations in output. This aspect of hour behavior has substantial econometric support [15].

eral underlying factors. Strictly speaking, the leap requires the assumption that, whatever the influence of slack economic activity on average hours, labor force participation and manhour productivity, the magnitudes of all these effects are related to the unemployment rate. With this assumption, the unemployment rate can be viewed as a proxy variable for all the ways in which output is affected by idle resources [20].

Three alternative methods of relating unemployment to output lea Okun to the view that "the uniformity which emerged from these vari ous techniques was the approximate 3-to-1 link between output and th unemployment rate."

It also turned out that a graph of potential GNP embodying thi relation leads to "a curve that wiggles from quarter-to-quarter, eve dipping at times." Okun prefers a $3\frac{1}{2}$ per cent per year growth tren since he finds it "difficult to accept the verdict that potential output ha actually contracted at times, as the unsmoothed unemployment mea sure implies."

Finally, Okun states that "the record clearly shows that manhou productivity is depressed by low levels of utilization and that periods c movement toward full employment yield considerably above-averag productivity gains." This leads to a correct inference that "the positiv relationship between output and labor productivity suggests that muc of labor input is essentially a fixed cost for fairly substantial periods.

It seems that the short-term irregularities (which Okun quite properl rejects as an accurate reflection of potential output) and the high short run elasticity of output with respect to unemployment are both refle tions of the short-run distributed-lag relation between output and em ployment, which can be eliminated efficiently at the source by placin direct reliance on the steady state solution of the cyclical productio function recommended in the preceding section of the paper.

C. *Labor Gaps*

It is a matter of some interest to estimate unemployment by subtract ing actual employment from potential (rather than actual) labor force Table 3 contains unemployment gaps for each of the three participatio equations for the business sector and the Strand-Dernberg calculatio for total civilian labor force, after adjusting actual to potential labo force at a 4 per cent unemployment rate. Chart 2 illustrates potentia gaps for employment and output based on the most conservative labo participation equations (6a).[10] In each recovery the output gap decline

[10] The Strand-Dernberg estimates are well below my most "conservative" participatio equations (6a) and (6b) throughout most of the period, in part at least because of differe coverage, until 1961-IV when their estimate just exceeds the (6a) level. Their calculation d pends only on an estimate of population growth, and an additional equation to explain the additional variable related to the secondary worker hypothesis. A production function by-passed completely.

TABLE 3—UNEMPLOYMENT PERCENTAGE GAPS

Year-Quarter	Business Sector Labor Force			Total Civilian Labor Force*
	Gap A	Gap B	Gap C	Strand-Dernberg Gap
	Participation Equation			
	(8a)	(8b)	(8c)	
1949 II	12.11	12.95	10.17	
III	12.41	13.52	10.54	
IV	12.65	13.89	10.88	
1950 I	12.22	13.69	10.53	
II	10.32	11.43	8.75	
III	7.99	8.68	6.51	
IV	6.29	4.04	4.77	
1951 I	6.02	9.40	4.67	
II	5.98	6.67	4.70	
III	6.48	7.40	5.25	
IV	7.20	8.26	6.05	
1952 I	6.45	7.28	5.37	
II	6.69	7.49	5.67	
III	6.90	7.92	5.96	
IV	5.60	6.69	4.74	
1953 I	4.48	9.02	3.83	
II	5.31	5.96	4.72	
III	6.11	7.02	5.58	
IV	6.72	7.64	6.26	4.77
1954 I	8.72	9.50	8.31	
II	10.46	11.07	10.09	
III	11.01	11.63	10.70	
IV	11.20	12.25	10.97	6.67
1955 I	10.64	11.43	10.46	
II	8.61	9.43	8.50	
III	6.73	7.52	6.69	
IV	6.35	7.47	6.38	4.03
1956 I	5.60	6.51	5.70	
II	5.35	6.33	5.52	
III	5.32	6.62	5.56	
IV	4.61	6.19	4.95	3.91
1957 I	5.06	6.47	5.48	
II	6.02	7.72	6.54	
III	6.68	8.59	7.31	
IV	7.28	8.60	7.99	5.98

* From [22, Table 3, p. 388] for 4 per cent unemployment rate.

TABLE 3—(*Continued*)

Year-Quarter		Business Sector Labor Force			Total Civilian Labor Force*
		Gap A	Gap B	Gap C	Strand-Dernberg Gap
		Participation Equation			
		(8a)	(8b)	(8c)	
1958	I	9.60	10.60	10.39	
	II	11.37	12.42	12.24	
	III	11.45	12.40	12.39	
	IV	10.05	11.32	11.05	7.75
1959	I	9.36	10.42	10.43	
	II	8.73	9.76	9.86	
	III	8.07	9.20	9.27	
	IV	8.43	9.57	9.71	6.93
1960	I	7.00	7.83	8.32	
	II	6.51	7.35	7.88	
	III	7.89	9.53	9.36	
	IV	9.35	12.38	10.98	7.63
1961	I	10.08	13.07	11.86	
	II	9.43	11.99	11.31	
	III	9.70	12.20	11.71	
	IV	10.49	12.72	12.62	9.17
1962	I	10.42	12.42	12.64	
	II	9.69	11.26	11.98	
	III	9.05	11.38	11.45	
	IV	8.88	10.88	11.36	9.45

before the employment gap, reflecting the employment-output lag which has been repeatedly emphasized throughout this paper. It furthermore appears that at times actual output exceeds long-run steady-state potential output even when the potential unemployment gap is in excess of 4 per cent. Since the parameters are sufficiently uncertain, and the capacity concept itself is somewhat ambiguous, literal insistence on inadequate capacity would be unwarranted.

In order to focus more closely on differences among the participation equations, the three potential unemployment gaps have been plotted together on Chart 3. Equation (6b) has the highest gap throughout, although from 1958 on it is virtually indistinguishable from (6c). Since this formulation is the only one which begins to measure lagged responses, however simply, it does not appear wise to dismiss it out of hand. On theoretical and statistical grounds, equations (6b) or (6c) seem superior to (6a), but a final choice does not seem possible on the basis of the evidence presented here.

CHART 2. POTENTIAL UNEMPLOYMENT AND POTENTIAL OUTPUT GAP PERCENTAGES
[Based on participation equation (6a)]

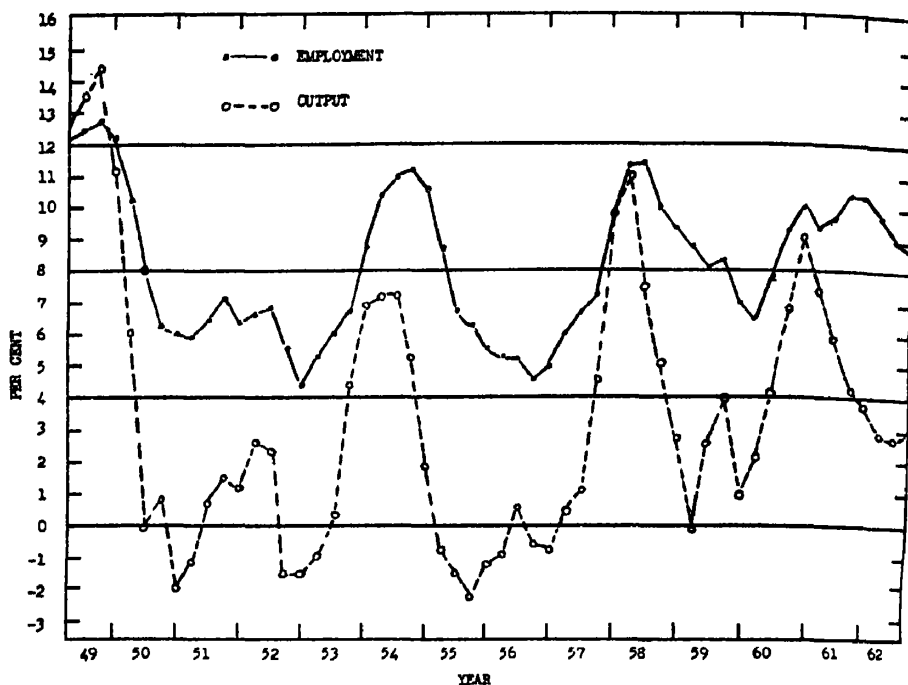
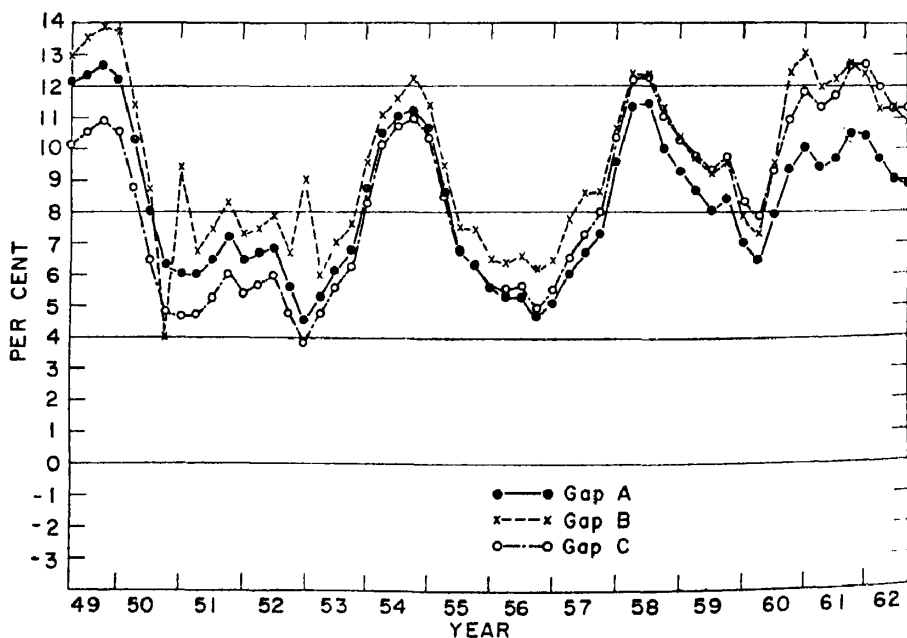


CHART 3. ALTERNATIVE EMPLOYMENT GAPS



V. *Summary and Qualifications*

This paper provides an outline of operational procedures for measuring potential output, a useful conception for short- and intermediate-run fiscal and monetary policy measures. Predominant emphasis has been on the measurement of potential output, rather than the corollary endeavor of using this equation system to trace out paths from underutilization toward potential output.[11]

The empirical bases of these parameters are shaky enough so that excessive confidence ought not to be placed on the present set of point estimates which, however, seem solidly enough based to illustrate an appropriate set of procedures. While existing research on cyclical participation rates has offered valuable new insights, continued efforts seem required in order to narrow the range of plausible estimates of potential labor force.[12] Furthermore, the highly aggregated cyclical production function ought to be replaced by a more disaggregated set of relations. These should include capital stock and reflect different dynamic reactions among industries and possibly among different types of labor inputs. Explicit consideration of cyclical variations in hours worked should also be included in future calculations.

Various methods have recently been proposed to estimate output that could be produced at a given level of unemployment. These methods include quite mechanical (though pragmatically useful) trend extrapolation and relating current unemployment to current output. The method suggested here combines recent econometric developments in the estimation of labor participation rates and cyclical production functions to portray the major elements that should appear in a proper structural formulation.

DATA APPENDIX

Gross Product Originating in the Business Sector in 1954 Dollars: By Quarters at Annual Rates

The original data were presented in an article by M. L. Marimont, "GNP

[11] The latter problem is in essence an exercise in simulation, since: (a) the initial conditions relating to output and employment, and (b) subsequent feasible and/or desired levels of output (obtainable through fiscal and monetary policy), in principle are consistent with an indefinitely large number of intermediate-output, employment configurations. Optimal macroeconomic decision rules might readily be applied to provide a more restricted (possibly unique) set of choices. At a future date, it should be worthwhile to carry out some of the more interesting combinations, within the subsystem and in the context of a complete equation system such as the Brookings-SSRC econometric model [4].

[12] Tella [28] and Strand and Dernberg have done additional work (published in this *Review*, March 1966) on age-sex participation rate responses, which lend support to their more aggregative results previously cited. Two important aspects of the problem not yet solved are, first, a more precise characterization of the lag structure; and second, the cross-elasticities of supply among the different age-sex groups.

by Major Industries," *Survey of Current Business*, October 1962, pages 6–18. Comprehensive annual data by originating sector were here presented for the first time. These data have been interpolated on a quarterly basis with relevant quantity indices (including components of the Federal Reserve Board Index of Industrial Production for Manufacturing) by Gary Fromm and James Craig for the Brookings-SSRC Econometric Model Project.

The gross product originating series was arrived at (to within the statistical discrepancy published by Marimont) by aggregating the business industries and adding actual deflated residential rents[13] (thus excluding imputed rent on owner occupied dwellings). Quarterly data on the business industries, including manufacturing, construction, trade, transportation, public utilities and mining, had already been constructed by the Brookings-SSRC Econometric Model Project. Annual real residential rents were constructed using a quadratic interpolation formula and then added to the business industries to form the business sector gross product originating.

Employment and Labor Force by Quarters in the Business Sector

The data for employment and labor force associated with the business sector have been computed and seasonally adjusted by the Bureau of Labor Statistics. Both the household and establishment surveys of labor force and employment have been used in preparing these estimates. Employment in the business sector is approximated by subtracting the number of wage and salary workers in government from the nonagricultural employment excluding domestics.[14] To obtain the labor force associated with the business sector, those unemployed with previous experience in the business industries, and those unemployed with no previous experience are added to the employment totals previously described.

Civilian Population by Quarters

Monthly statistics on the civilian noninstitutional population over the age of 14 series are regularly published by the Bureau of Labor Statistics. Quarterly data were compiled by averaging the three constituent midmonth figures, and then seasonally adjusting this series using standard constant seasonal adjustment factor methods. The 1949 and 1951 quarterly data (used only in extrapolation backwards of equations (6a) and (6c)) were adjusted upwards to be comparable with data based upon the 1960 census.

REFERENCES

1. W. H. L. ANDERSON AND J. BABCOCK, "Measuring Potential Gross National Product," paper delivered at December 1964 Meeting of the Econometric Society.
2. F. BRECHLING, "The Relationship Between Output and Employment in

[13] Farm rents paid to nonfarm landlords have been subtracted.

[14] A minor inconsistency arises since landlords are not in this business sector labor force, even though rent has been included in output.

British Manufacturing Industry," *Rev. Econ. Stud.*, July 1965, *32*, 187–216.

3. W. G. Bowen and T. A. Finegan, "Labor Force Participation and Unemployment," in Arthur M. Ross, ed., *Employment Policy and the Labor Market*, Berkeley, Calif. 1965.
4. J. S. Duesenberry, G. Fromm, L. Klein, and E. Kuh, eds., *Brookings Quarterly Econometric Model of the United States*. Chicago 1965.
5. O. Eckstein and T. Wilson, "Short-Run Productivity Behavior in U.S. Manufacturing," *Rev. Econ. Stat.*, Feb. 1964, *46*, 41–54.
6. *Economic Report of the President*, January 1962, pp. 49–53.
7. ———, January 1963, pp. 81–85.
8. W. A. W. Godley and J. R. Shepherd, "Long-Term Growth and Short-Term Policy," *Nat. Inst. Econ. Rev.*, Aug. 1964, 26–38.
9. T. Hultgren, *Change in Labor Cost During Cycles in Production and Business*, Nat. Bur. Econ. Research Occas. Paper 74. New York 1960.
10. C. C. Holt, F. Modigliani, J. Muth, and H. Simon, *Planning Production, Inventories and Work Force*. Englewood Cliffs, N.J. 1960.
11. J. W. Kendrick, *Productivity Trends in the United States*. A Study of the National Bureau of Economic Research. Princeton 1961.
12. J. Knowles and C. B. Warden, Jr., *The Potential Economic Growth of the United States*. Study Paper 20, Joint Economic Committee Study of Employment, Growth and Price Levels, Washington, D. C., January 1960.
13. E. Kuh, *Profits, Profit Markups and Productivity: An Examination of Corporate Behavior Since 1947*. Study Paper 15, Joint Economic Committee Study of Employment, Growth and Price Levels, Washington, D.C., January 1960.
14. ———, "Cyclical and Secular Labor Productivity in U.S. Manufacturing," *Rev. Econ. Stat.*, Feb. 1965, *47*, 1–12.
15. ———, "Income Distribution and Employment over the Business Cycle," Chapter 8 in [4].
16. ——— and J. R. Meyer, "Correlation and Regression Estimates When the Data Are Ratios," *Econometrica*, Oct. 1955, *23*, 400–416.
17. S. Lebergott, "The Labor Force and Marriages as Endogenous Factors," Chapter 10 in [4].
18. J. Mincer, "Labor Force Participation and Unemployment," (mimeo., unpublished) paper delivered to Universities-National Bureau of Economic Research Conference, spring 1965.
19. R. Nelson, "Aggregate Production Functions and Medium-Range Growth Projections," *Am. Econ. Rev.*, Sept. 1964, *54*, 575–606.
20. A. Okun, "Potential GNP: Its Measurement and Significance," *Am. Stat. Assoc., Proc.*, 1962, 98–104.
21. E. Phelps, "The New View of Investment: A Neoclassical Analysis," *Quart. Jour. Econ.*, Nov. 1962, *76*, 548–67.
22. R. Soligo, *The Short Run Relationship Between Employment and Output*. Unpublished doctoral dissertation, Yale Univ., 1963.
23. R. M. Solow, "Distribution in the Long and Short Run," to appear in a

forthcoming volume on income distribution sponsored by the International Economic Association.

24. ———, Presidential Address to the Econometric Society, delivered in December 1964 (unpublished).
25. ———, "Technical Progress, Capital Formation, and Economic Growth," *Am. Econ. Rev., Proc.*, May 1962, *52*, 76–86.
26. K. Strand and T. Dernberg, "Cyclical Variation in Civilian Labor Force Participation," *Rev. Econ. Stat.*, Nov. 1964, *46*, 378–91.
27. A. Tella, "The Relation of Labor Force to Employment," *Indus. Lab. Rel. Rev.*, April 1964, *17*, 454–69.
28. ———, "Labor Force Sensitivity to Employment by Age, Sex," *Indus. Rel.*, Feb. 1965. *4*, 67–83.

OWNERSHIP AND CONTROL IN THE 200 LARGEST NONFINANCIAL CORPORATIONS, 1929 AND 1963

By ROBERT J. LARNER*

In 1932, Adolf Berle and Gardiner Means published their classic study, *The Modern Corporation and Private Property*, a major thesis of which was that "Ownership of wealth without appreciable control and control of wealth without appreciable ownership appear to be the logical outcome of corporate development" [4, p. 69]. Since then, the existence of management control among giant corporations and its increasing extent over time have been generally accepted in the literature as part of the "conventional wisdom" [2, pp. 70–74] [3, p. 30] [5] [7] [13, p. 53]. Yet, with the exception of the 1939 study which the Securities and Exchange Commission prepared for the Temporary National Economic Committee [18], no attempt seems to have been made to determine the extent of management control in the years since the Berle and Means study.

This article attempts to measure systematically the extent to which management control actually exists among the 200 largest nonfinancial corporations in the first half of the 1960's. The article is divided into two parts: the first part describes the method followed in the study, and the second summarizes the results of the study and compares them with the findings of the 1929 Berle and Means study.

I. *The Method of This Study*

To assure as direct a comparison as possible between the 1929 and the 1963 findings, this paper will follow very closely the definitions, procedures, and classifications used in the Berle and Means study. Since direction over the activities of a corporation is legally and theoretically exercised by its board of directors, Berle and Means defined control as the "... actual power to select the board of directors (or its majority)" [4, p. 69]. Although the power to control and the actual exercise of that power can conceivably reside in different individuals, there are nevertheless cogent reasons for accepting Berle and Means's definition of control. First, even if the owner of a majority or substantial minority of a corporation's voting stock were to surrender control to the manage-

* The author is a graduate student in economics at the University of Wisconsin. He wishes to thank Professor Leonard Weiss of that university for the encouragement, advice, and overall supervision he has so kindly given in the preparation of this article, an earlier draft of which was written for his seminar in industrial organization. The author, however, is solely responsible for all errors. Part of the research was financed by the National Science Foundation.

ment, he would still retain the legal power to vote an unsatisfactory board of directors out of office, and even such a dormant power can be a strong influence. Secondly, to prevent biased results which are solely dependent on arbitrary judgements, it seems wise to use objective criteria which are easily observed and capable of precise measurement in determining the type of corporate control.

Berle and Means distinguished between "ultimate control" and "immediate control." This distinction occurred where one corporation controlled another through a dominant minority stock interest.[1] In this case, the controlled corporation was always classified as immediately controlled by either minority or joint-minority interests. If the controlling corporation itself was management-controlled, then the controlled corporation was also classified as ultimately controlled by management. If the controlling corporation was not management-controlled, then the controlled corporation was said to be ultimately controlled through pyramiding. The present study also uses this distinction.

Following Berle and Means, our list of the 200 largest nonfinancial corporations is composed of firms primarily engaged in manufacturing, mining, merchandising, transportation, and electric, gas, and pipeline utilities. Banks, insurance companies, and investment companies are excluded. Size is measured in terms of book assets, a procedure which overstates the size of the transportation and utility companies relative to the size of firms in other industries. The use of sales to measure size, however, would introduce an equally serious opposite bias.[2] Moreover, since Berle and Means, the SEC study [18], and R. A. Gordon [6] all used assets to measure size, this paper will follow the same procedure to maintain comparability.[3]

Berle and Means classified the firms in their study according to the following five types of corporate control: (1) privately owned, (2) con-

[1] In the Berle and Means study, a corporation which was majority-controlled by another corporation was classified as a subsidiary of the latter and was disregarded, except where an important element of pyramiding entered in. The present study follows the same procedure, except where the controlling corporation is a smaller firm not included among the "200 largest." In this case the controlled corporation is retained as if it were an independent company: e.g. Hughes' Tool Co. is ignored and TWA is treated as majority-controlled. If the controlling corporation is a foreign firm, no attempt is made to determine if the foreign firm is management-controlled, and the controlled corporation is assigned either to ultimate minority control or to ultimate control through pyramiding. This is the same procedure which Berle and Means followed.

[2] Perhaps the best measure of size is value added, since it compares the value of the factors of production controlled by each firm [1], but data on value added by firm are not generally available.

[3] The names and assets of the 200 largest nonfinancial corporations in 1963 appear in the appendix to this article, which may be obtained from the author on request. Requests can be addressed in care of the Department of Economics, University of Wisconsin, Madison, Wisconsin 53706.

trolled through the ownership of a majority of the voting stock, (3) controlled through the ownership of a dominant minority of the voting stock, (4) controlled by means of a legal device,[4] and (5) management-controlled.[5] This study uses the same categories.

A firm is considered to be privately owned if an individual, a family, or a group of business associates holds 80 per cent or more of its voting stock. For majority ownership, the individual, family, or group of business associates must own between 50 and 80 per cent of the voting stock. In the Berle and Means study, stock ownership of between 20 and 50 per cent was generally necessary for minority control, although in several specific instances a smaller holding was credited with the power of control. In view of the greater size of the 200 largest nonfinancial corporations in 1963 and the wider dispersion of their stock, this lower limit to minority control seems too high. In the present study a firm is classified as immediately controlled by minority stock ownership if 10 per cent or more of its voting stock is held by an individual, family, corporation, or group of business associates.[6]

Berle and Means assigned corporations in which no base of control in stock ownership could be found to management control on the belief that no group of stockholders would be able under ordinary circumstances to muster enough votes to challenge the rule of management. This study follows the same procedure.[7]

When all of the above criteria are applied to the 1963 data, it is generally not difficult to distinguish management control from the other types of control, but several errors or distortions occur when ownership control is further broken down into privately owned, majority-controlled, and minority-controlled corporations, and those controlled through a legal device.[8]

[4] Berle and Means recognized four kinds of legal devices by which corporate control might be obtained: (1) pyramiding, (2) nonvoting common stock, (3) stock with disproportionate voting power, and (4) the voting trust. Only the first and the fourth devices are found today.

[5] For a more detailed description of these five categories and of the criteria for each, see Chapter 5 in *The Modern Corporation and Private Property* [4].

[6] In two cases, this rule has been disregarded. The Transcontinental Gas Pipe Line Company is classified as management-controlled even though the Stone & Webster Company holds of record an 11 per cent stock interest, since the latter is not represented on Transcontinental's board of directors. On the other hand, May Department Stores Company is classified as minority-controlled even though the May family has only a 3.9 per cent stock interest, since members of the May family hold the offices of chairman, vice-chairman, and president. and occupy five seats on the board of directors.

[7] In addition, Berle and Means found 16 companies to be controlled jointly, either by two or more minority interests or by a minority interest and management. In the latter case, they divided the corporation into two "half companies," each possessing one-half of the assets of the original company. One of these "half companies" was then classified as management-controlled and the other as minority-controlled. This joint minority-management control category is not used in the present study.

II. *Summary and Comparison of the Results*

Each of the 200 largest nonfinancial corporations in 1963 is listed in the appendix, together with its size and rank in assets, its type of control, immediate and ultimate, in both 1963 and 1929, and the source and basis of its classification in 1963.[9]

Tables 1 and 2 provide a summary of the type of control, by number of corporations and by assets in 1963 and 1929, for the 200 corporations as a whole and for each of the three major industrial groups (industrials, public utilities, and transportation companies). A significant finding of this study is that management control[10] has substantially increased among the 200 largest nonfinancial corporations since 1929. As Table 1 illustrates, 44 per cent of the 200 largest nonfinancial corporations in 1929 and 58 per cent of their assets were management-controlled. In 1963, however, 84.5 per cent of the "200 largest" of that year and 85 per cent of their assets were so controlled. Management control increased substantially within each of the three industrial groups and became the overwhelmingly predominant type of control within each group.

As shown in Table 1, private ownership had *completely disappeared* among the 200 largest nonfinancial corporations by 1963. Of the 12 privately owned firms on the 1929 list, six had dropped out of the "200 largest" by 1963. Of the remainder, one (A & P) was majority-controlled, four (Alcoa, Ford, Gulf Oil, and National Steel) were minority-controlled, and one (Jones & Laughlin) was apparently management-controlled. Only five companies (A & P, Duke Power Co., Kaiser Industries, Sun Oil, and TWA) were found to be majority-controlled in 1963, and in the case of TWA actual control was, at least temporarily, in the hands of trustees.

[8] Following the Berle and Means definitions, the author classified Tidewater Oil, Shell Oil, and the Coca-Cola Company as ultimately controlled by pyramiding because of their peculiar organizational structures, even though the evidence suggests that Tidewater and Shell are effectively majority-controlled and Coca-Cola effectively management-controlled. The Berle and Means definitions were followed exactly in order to keep the two studies as comparable as possible. Similarly, although Trans World Airlines is classified as majority-controlled, an equally convincing argument might be made for classifying it as controlled by a legal device, since Howard Hughes's 78 per cent stock interest (through the Hughes Tool Company) in TWA was, at least temporarily, being held in trust by a group of insurance companies and banks in 1963. Hughes's stock was sold to the general public in May, 1966. The TWA management was reported to be "anxious that no one should gain effective control—which might be done with as little as 10 per cent of the stock" [19, p. 145].

[9] The principal sources used in determining type of corporate control were the definitive proxy statements filed with the SEC by all of the 200 corporations, the annual reports filed with the Interstate Commerce Commission by each railroad, and the annual reports filed with the Federal Power Commission by the utilities which it regulates. A more complete description of these sources and references to other sources used can be found in the appendix.

[10] "Management control" without qualification should be understood to mean *ultimate* control by management. The same applies, *mutatis mutandis*, to the other types of corporate control.

TABLE 1—SUMMARY ACCORDING TO THE TYPE OF ULTIMATE CONTROL OF THE 200 LARGEST NONFINANCIAL CORPORATIONS, 1963 AND 1929

Part 1: Number of Corporations

Type of Control	Number of Corporations				Proportion of Companies by Industrial Groups			
	Total	Industrials	Public Utilities	Transportation Cos.	Total	Industrials	Public Utilities	Transportation Cos.
1963					%	%	%	%
Private Ownership	0	0	0	0	0	0	0	0
Majority Ownership	5	3	1	1	2.5	3	2	4
Minority Control	18	18	0	0	9	15	0	0
Legal Device	8	5	0	3	4	4	0	13
Management Control	169	91	58	20	84.5	78	98	83
	200	117	59	24	100	100	100	100
1929								
Private Ownership	12	8	2	2	6	8	4	5
Majority Ownership	10	6	3	1	5	6	6	2
Minority Control	46½	34½	7½	4½	23	32	14	11
Legal Device	41	14½	19	7½	21	14	36	18
Management Control	88½	43	19½	26	44	40	38	62
In Receivership	2	—	1	1	1	0	2	2
Total	200	106	52	42	100	100	100	100

Sources: 1963—Appendix (see footnote 3); 1929—Berle and Means, *The Modern Corporation and Private Property* [4, p. 115].

TABLE 1—(*Continued*)

Part 2: Assets of Corporations

Type of Control	Assets (In Millions of Dollars)				Proportion of Assets by Industrial Groups			
	Total	Industrials	Public Utilities	Transportation Cos.	Total	Industrials	Public Utilities	Transportation Cos.
1963					%	%	%	%
Private Ownership	0	0	0	0	0	0	0	0
Majority Ownership	3,307	2,098	697	512	1	1	1	2
Minority Control	28,248	28,248	0	0	11	19	0	0
Legal Device	8,765	4,959	0	3,806	3	3	0	15
Management Control	224,377	117,732	85,300	21,345	85	77	99	83
Total	264,697	153,037	85,997	25,663	100	100	100	100
1929								
Private Ownership	3,366	2,869	221	276	4	9	1	1
Majority Ownership	1,542	779	480	283	2	3	2	1
Minority Control	11,223	9,258	1,261	704	14	31	5	3
Legal Device	17,565	4,307	9,406	3,852	22	14	37	15
Management Control	47,108	13,142	14,291	19,675	58	43	55	79
In Receivership	269	0	108	161	—*	0	—*	1
Total	81,073	30,355	25,767	24,951	100	100	100	100

* Less than 1 per cent.

Sources: 1963—Appendix (see footnote 3); 1929—Berle and Means, *The Modern Corporation and Private Property* [4, p. 115].

TABLE 2—SUMMARY ACCORDING TO THE TYPE OF IMMEDIATE CONTROL OF THE 200 LARGEST NONFINANCIAL CORPORATIONS, 1963 AND 1929

Part 1: 1963

Type of Control	Total		Industrials		Public Utilities		Transportation Cos.		Distribution of Total	
	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	By Company	By Assets
Private Ownership	0	0	0	0	0	0	0	0	0%	0%
Majority Ownership	9	8,387	5	5,218	2	1,480	2	1,689	4	3
Minority Control	28	37,252	23	31,641	0	0	5	5,611	14	14
Legal Device	0	0	0	0	0	0	0	0	0	0
Management Control	160	216,818	87	114,792	57	84,517	16	17,509	80	82
Joint Minority Control*	3	2,240	2	1,386	0	0	1	854	2	1
Total	200	264,697	117	153,037	59	85,997	24	25,663	100	100

* Includes corporations jointly controlled by two or more minority interests.
Source: Appendix (see footnote 3).

TABLE 2—(*Continued*)

Part 2: 1929

Type of Control	Total		Industrials		Public Utilities		Transportation Cos.		Distribution of Total	
	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	Number of Companies	Assets ($000,000)	By Company	By Assets
Private Ownership	12	3,367	8	2,870	2	221	2	276	6%	4%
Majority Ownership	10	1,542	6	779	3	480	1	283	5	2
Minority Control	73	25,593	38	11,179	22	10,105	13	4,309	36.5	32
Legal Device	21	9,232	10	2,260	10	5,372	1	1,600	10.5	12
Management Control	65	35,802	41	12,736	10	8,040	14	15,026	32.5	44
Joint Control*	16	5,164	3	532	4	1,441	9	3,191	8	6
Special Situations	3	374	0	0	1	108	2	266	1.5	—
Total	200	81,074	106	30,356	52	25,767	42	24,951	100	100

* Includes corporations jointly controlled by two or more minority interests or jointly controlled by a minority interest and management.
Source: Berle and Means, *The Modern Corporation and Private Property* [4, p. 116].

Only 18 firms were found to be controlled by minority stockholders in 1963, roughly a third of the 46½ firms which Berle and Means classified as minority-controlled in 1929. Control through legal devices decreased even more sharply—from 41 in 1929 to 8 in 1963. On the other hand, the number of management-controlled firms almost doubled, from 88½ in 1929 to 169 in 1963.

Five companies on the 1963 list which are classified as management-controlled appear to be controlled, or at least very strongly influenced, by a single family within their management. Yet these families owned only a very small fraction of the outstanding voting stock. The five companies and their controlling families are: IBM (Watson), Inland Steel (Block), Weyerhaeuser (Weyerhaeuser), Federated Department Stores (Lazarus), and J. P. Stevens (Stevens). Federated Department Stores is the best illustration of this. In 1963 its chairman of the board, its president, and five of its 19 directors were members of the Lazarus family, even though the combined stock interest of the entire family was only 1.32 per cent. Since the present basis of control by these families appears to be their strategic position in management and the traditional identification of the corporation with the family rather than any appreciable amount of stock ownership, these companies are classified as management-controlled.

Management control was distributed rather evenly among the three industrial groups in 1963. Its highest incidence was among the utilities where it accounted for all but one of the 59 firms. Yet the public utilities had the lowest incidence of management-controlled firms in 1929 (38 per cent). This drastic change can be explained by the "death sentence" provision of the Public Utility Holding Company Act of 1935, which proscribes pyramiding beyond the second degree among public utility holding companies. Management control was the predominant type of control for the industrials and the transportation companies as well accounting for 78 per cent of the former and 83 per cent of the latter.

A significant difference between 1963 and 1929 is that the proportion of the 200 largest nonfinancial corporations that were management-controlled in the later year was about the same as the proportion of assets so controlled, while in 1929 the proportion of assets that were management-controlled was a good deal larger than the proportion of companies so controlled. This indicates that management control, which was concentrated among the larger firms on the 1929 list, has since reached down to relatively smaller (though absolutely larger) firms than it touched in 1929 and has become rather evenly distributed among the "200 largest." This contrast is illustrated by Table 3, which divides the "200 largest" of each year by rank into five groups of 40 firms each and lists the number of management-controlled firms in each group.

TABLE 3—"200 LARGEST" DIVIDED BY RANK INTO 5 GROUPS AND NUMBER OF MANAGEMENT-CONTROLLED FIRMS IN EACH GROUP

Firms Ranking	Number of Management-Controlled Firms	
	1963	1929
1 through 40	34	27
41 through 80	33	21
81 through 120	32	15⅓
121 through 160	37	15½
161 through 200	33	9½
Total	169	88⅓

Source: Appendix (see footnote 3)

This evidence suggests that a firm may reach a size so great that, with a few exceptions, its control is beyond the financial means of any individual or group. This point appears to have been reached only by the larger firms on the 1929 list, but by all of the firms on the 1963 list. The smallest corporation among the top 200 in 1963 had assets of 423 million current dollars or, deflating by the GNP deflator, 204 million 1929 dollars. A corporation of this size would have ranked 111th on the Berle and Means list. Of the 110 firms which would have ranked ahead of it, 55 per cent were management-controlled, compared with only 31 per cent of the remaining 90 firms. Moreover, many of the remaining 45 per cent of the top 110 firms in 1929 were either public utilities controlled by a kind of pyramiding which is now illegal or industrial firms still owned and controlled by their founder.

The present study may classify some firms incorrectly because of the limited information available to outsiders about the control of the 200 largest nonfinancial corporations. Generally, such errors would involve failure to locate an existing center of ownership (especially minority) control, so that the company is mistakenly classified as management-controlled. This would result, of course, in our overstating the extent of management control in 1963. Berle and Means, however, would seem to be in greater danger of overstating the extent of management control because of the less authoritative and less systematic sources of data upon which they had to rely and because of the larger minimum stockholding which they required as sufficient evidence of minority control. It follows that, even though the findings of the present study may overstate somewhat the *extent* of management control in 1963, they are also likely to understate the *change* in the extent of management control from 1929 to 1963 when compared with the findings of the Berle and Means study.

In summary, it would appear that Berle and Means in 1929 were observing a "managerial revolution" in process. Now, 30 years later,

that revolution seems close to complete, at least within the range of the 200 largest nonfinancial corporations.

REFERENCES

1. M. A. ADELMAN, "The Measurement of Industrial Concentration," *Rev. Econ. Stat.*, Nov. 1951, *33*, 269–96; reprinted in R. B. Heflebower and G. W. Stocking, eds., *Readings in Industrial Organization and Public Policy*, Homewood 1958, pp. 3–45.
2. A. A. BERLE, *Power Without Property*. New York 1959.
3. ———, *The 20th Century Capitalist Revolution*. New York 1954.
4. ——— and G. C. MEANS, *The Modern Corporation and Private Property*. New York 1932.
5. ———, *et al.*, "Symposium on the Impact of the Corporation on Classical Economic Theory," *Quart. Jour. Econ.*, Feb. 1965, *79*, 1–51.
6. R. A. GORDON, *Business Leadership in the Large Corporation*. Washington 1945.
7. R. J. MONSEN, JR. AND A. DOWNS, "A Theory of Large Managerial Firms," *Jour. Pol. Econ.*, June 1965, *73*, 221–36.
8. *Moody's Industrial Manual, 1964*. New York 1964.
9. *Moody's Public Utility Manual, 1964*. New York 1964.
10. *Moody's Transportation Manual, 1964*. New York 1964.
11. M. J. PECK, *Competition in the Aluminum Industry, 1945–1958*. Cambridge, Mass. 1961.
12. T. K. QUINN, *Giant Business: Threat to Democracy*. New York 1953.
13. E. V. ROSTOW, "To Whom and for What Ends Is Corporate Management Responsible?", in E. S. Mason, ed., *The Corporation in Modern Society*, Cambridge, Mass. 1959, pp. 46–71.
14. Standard and Poor's, *Standard Listed Stock Reports*. Ephrata, Pa.
15. R. B. TENNANT, *The American Cigarette Industry*. New Haven 1950.
16. "*Fortune*'s Directory of the 500 Largest Industrial Corporations," *Fortune*, July 1964, *70*, 179–98.
17. "*Fortune*'s Directory: Part II," *Fortune*, Aug. 1964, *70*, 151–62.
18. TEMPORARY NATIONAL ECONOMIC COMMITTEE, *Distribution of Ownership in the 200 Largest Nonfinancial Corporations*, "Monograph No. 29." Washington 1940.
19. "What's behind the Big TWA Sale?," *Business Week*, April 16, 1966, 145–50.

THE GAIN FROM INTERNATIONAL TRADE AND INVESTMENT: A NEO-HECKSCHER-OHLIN APPROACH

By MURRAY C. KEMP*

1. That it may be to a single country's advantage to depress the volume of its foreign trade below the free trade level was known to Torrens, J. S. Mill, and Sidgwick;[1] and the more precise notion of an optimal degree of trade restriction (by means of an optimal tariff on imports or exports) goes back at least to Edgeworth and Bickerdike.[2] Rather later, it was recognized that a country may also gain by curbing its net international borrowing or lending,[3] and that there may exist an optimal degree of restriction (by means of an optimal tax on foreign interest and profit receipts or payments).[4]

2. In this earlier literature, the gain from trade and the gain from international investment were implicitly assumed to be mutually independent and separately calculable. In conformity with this assumption, the calculation of the optimal degree of trade restriction and of the optimal curb on investment were treated as unrelated problems.

It is clear, however, that by changing the conditions of supply and the level of net income, both in the lending and in the borrowing country, international investment also may modify both the gain from trade and

* The author is research professor of economics in the University of New South Wales. He gratefully acknowledges the sustained assistance of Professor Henry Y. Wan, Jr., and of Professor Ronald Jones, especially with the more delicate arguments of Section II. He hopes that the results of Professor Jones's own investigations will soon be published. The author acknowledges also the useful comments of Professors Takashi Negishi and John Chipman on the penultimate draft.

[1] Torrens [36, pp. 281–82] and [37, Postscript to Letter IX, especially pp. 329–38]; Mill [19, pp. 21 ff.] and [20, Book V, Ch. 4, Sec. 6]; Sidgwick [32, pp. 493 ff.].

The notion that a tariff may turn the terms of trade in favor of the tariff-imposing country has a confusing and little-known early history. The principle was clearly stated by Torrens in 1824, in his *Essay on the Production of Wealth*, though the scope of the principle was unnecessarily restricted to circumstances in which the tariff-imposing country has "a natural monopoly, either partial or complete." The principle was reasserted by Torrens in an 1832 parliamentary speech (for details, see Robbins [23, pp. 192–94]) and, for the special case in which all import demands are of unit elasticity, was elaborated with great precision in *The Budget* of 1844 (the important Postscript to Letter IX was written in 1843). In order of writing, though not of publication, Torrens' 1844 exposition was preceded by that of J. S. Mill. In the preface to *Essays on Some Unsettled Questions of Political Economy* (1844) Mill mentions that they were written in 1829–30 and were being published in response to Torrens' *The Budget*.

[2] Edgeworth [6, pp. 13–16, 38–39]; Bickerdike [3] [4].

[3] Keynes [14] [16]; Iversen [8]; Cairncross [5]; Singer [34]; Nurkse [22]; Schonfield [30]. See also, however, Smith [35, Book II, Ch. 5].

[4] MacDougall [18]; Rowan and Pearce [24].

the optimal degree of trade restriction. On the other hand, the restriction of trade changes the terms of trade and the relative profitabilities of investment at home and abroad; it therefore may affect the optimal degree of restriction of international investment. "Tariff factories" provide a simple and familiar example: the imposition of a tariff gives rise to an inflow of capital to finance the construction of branch plants behind the tariff wall; as a result, the terms-of-trade effect of the tariff is weakened, perhaps reversed. To treat trade and investment policies as separable and independent can be justified therefore only in a rough first approximation.

3. It is my purpose to formulate the problem of gain in a manner which will make clear the elements of interdependence, and to derive expressions for the optimal tariff and optimal tax.

In two earlier papers[5] this has been attempted for the simple case in which each country produces a single commodity. In the present paper I shall instead work with a two-countries, two-commodities, two-factors model which is distinguished from the familiar Heckscher-Ohlin model, however, in that technology is allowed to differ between countries. The two factors recognized are labor and capital, the latter assumed to be homogeneous, perfectly durable, and smoothly substitutable for labor.[6] Both factors are assumed to be in fixed supply: that is, the population of each country is stationary, and in each country net saving is zero. It is assumed that markets are perfectly competitive, all participants being well informed about world trading and investing opportunities. In particular, both factor markets are perfectly competitive, so that full employment is always assured, whatever commercial and fiscal policies are adopted. In keeping with the assumption of perfect competition, returns to scale are assumed to be constant. Finally, no account is taken of the possibility that one country may retaliate against the tariffs and taxes imposed by the other.

It will emerge that when account is taken of the elements of interdependence already referred to, either the optimal tariff or the optimal tax on foreign earnings, or both, may be negative.

I. *Geometric Statement of the Problem*

4. Putting aside net new borrowing, grants, indemnities, and remittances, every country is subject to a budget constraint of the form

(Quantity of Exports) *minus* (Quantity of Imports) × (Terms of Trade)
minus (Net International Indebtedness) ×
(Average Earnings on Debt)
= 0

[5] Kemp [10] [11]; also [12, Chs. 13 and 14].

[6] Whatever its shortcomings in more general contexts, this assumption seems appropriate when stationary equilibria only are being considered. Cf. Samuelson [26].

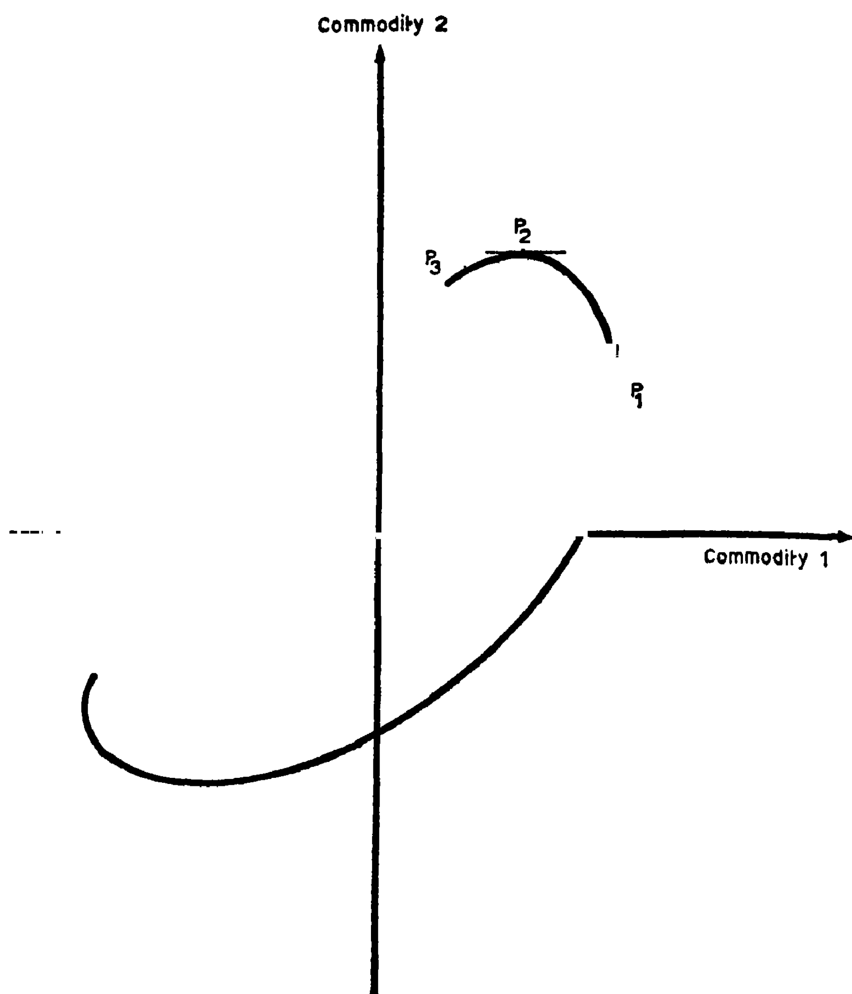


FIGURE 1. GENERALIZED OFFER CURVE

The desired imports and exports of a country depend both on the terms of trade and on its net indebtedness. Suppose the latter is given. Then for each hypothetical value of the terms of trade we can plot desired imports and exports. By varying the terms of trade we can trace a locus of import-export combinations, each of which represents a potential international equilibrium. (Cf. Figure 1.) This locus is a straightforward generalization of the familiar foreign trade offer curve. Note, however, that only when net indebtedness is zero does the curve pass through the origin and that, except in that special case, it is not possible to read off the terms of trade corresponding to a particular point on the locus by simply connecting that point by a straight line to the origin—the capital

item in our equation complicates things. Note also that an equilibrium is possible in which the debtor country imports neither commodity; such an equilibrium would be located in the fourth quadrant of Figure 1.

Now take the foreign country's offer curve thus defined and slide it round the home country's production-possibilities curve in the manner made familiar by Baldwin [1] and Samuelson [27]. In this way is generated an envelope—the home country's consumption-possibilities curve (*cc'* in Figure 2). Throughout this exercise, however, the net indebtedness of each of the two countries has been held constant. Suppose now that the home country invests a little more or less in the foreign country and that this is accomplished by shipping equipment from one country to the other. Evidently both the home country's production frontier and the foreign country's (generalized) offer curve will shift, and there will emerge a new envelope. Consider the envelope of all such envelopes (*CC'* in Figure 2). This truly describes the consumption possibilities facing the home country.

The problem is to find that combination of import duty (or subsidy)

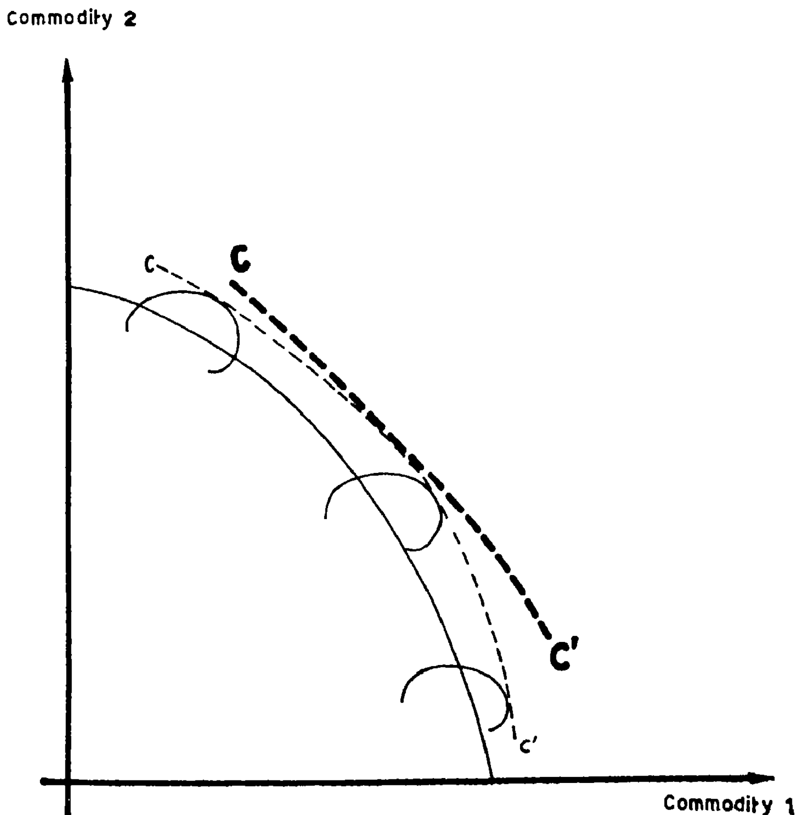


Figure 2. Consumption Possibilities Curve

and income tax (or subsidy) which will move the home country to the favored point on its generalized consumption-possibilities curve.

II. *Solution of the Problem: Incomplete Foreign Specialization*

5. Until further notice it will be assumed that, if it is a net creditor, the home or policy-making country either exports the second commodity or exports nothing; and that, if it is a net debtor, the home country exports the second commodity or exports both commodities. The second commodity is, in this sense, the "natural" export of the home country.

For the time being it will be assumed also that each country produces something of both commodities.

Let X_i stand for the home output of the ith commodity and D_i for the home consumption of the ith commodity. $E_i(\equiv D_i - X_i)$ is then the home excess (or import) demand for the ith commodity. Let p stand for the price of the second commodity in terms of the first, r_1 for the return on one unit of capital (the marginal product of capital in terms of the first commodity), and K for the amount of capital invested abroad by the home country. (If K is negative, it stands for the amount of capital invested in the home country by the foreign country.) Asterisks relate the variables to the foreign country. Thus D_i^* stands for the foreign country's demand for the ith commodity and p^* for the world terms of trade. Finally, U is a concave Scitovsky[7] index of the home country's welfare, a function of the aggregate amounts consumed by the home country of each of the two commodities, and of nothing else:

$$U = U(D_1; D_2) = U(X_1 + E_1; X_2 + E_2).$$

The foreign country, however, is subject to the budget constraint

$$E_1^* + p^* E_2^* + r_1^* K = 0; \tag{1}$$

moreover, in market equilibrium

$$E_i + E_i^* = 0 \qquad i = 1, 2.$$

Hence the index may be written as

$$U(X_1 + p^* E_2^* + r_1^* K; X_2 - E_2^*).$$

Now foreign excess demand depends on the world terms of trade, on real income, and on the amount of capital invested abroad by the home country. However, real income is itself related to the terms of trade and to the level of net international indebtedness; hence we may simply write $E_2^* = E_2^*(p^*, K)$. We note also that, since the foreign country is assumed to be incompletely specialized, r_1^* depends on p^*, but not on

[7] Scitovsky [31]. See also Samuelson [28].

K.[8] We therefore may write $r_1^* = r_1^*(p^*)$, with $\partial r_1^*/\partial p^* \gtreqless 0$ if, respectively, the second commodity is relatively capital-intensive, the two commodities are equally capital-intensive, or the first commodity is relatively capital-intensive. Finally, we note that the home output of the second commodity depends on the output of the first commodity and on the amount of capital invested abroad; that is, $X_2 = \varphi(X_1, K)$. [$\partial\varphi/\partial X_1$ is the slope of the production possibilities curve; $-\partial\varphi/\partial K$ is the marginal productivity of capital in terms of the second commodity, r_1/p.] It follows from all this that the welfare index can be written as a function of the three parameters X_1, p^*, and K:

$$U[X_1 + p^*E_2^*(p^*, K) + r_1^*(p^*)K; \varphi(X_1, K) - E_2^*(p^*, K)].$$

Each of the three parameters can be varied as an act of policy. The problem is to find those values of X_1, p^*, and K which maximize U.

It is worth noting, perhaps, that in this formulation of the problem the proceeds of the tariff and income tax are not explicitly recognized. This does not mean, of course, that matters of public finance are overlooked. The disposition of revenue is implicit in the selection of a social welfare function.

6. The first-order conditions of an interior maximum[9] may be written:[10]

$$\text{(2a)} \qquad U_2/U_1 = -1 \Big/ \frac{\partial\varphi}{\partial X_1}$$

$$\text{(2b)} \qquad U_2/U_1 = \frac{p^*}{\eta_2^*}\left(1 + \eta_2^* + \frac{K}{E_2^*}\cdot\frac{\partial X_2^*}{\partial K}\right)$$

$$\text{(2c)} \qquad U_2/U_1 = p^*\left(\frac{\partial X_2^*}{\partial K} - \frac{r_1^*}{p^*}\right) \Big/ \left(\frac{\partial X_2^*}{\partial K} - \frac{r_1}{p}\right)$$

where $U_i \equiv \partial U/\partial D_i$, $\eta_2^* \equiv (p^*/E_2^*)(\partial E_2^*/\partial p^*)$ is the total price elasticity of foreign import demand, and $\partial X_2^*/\partial K$ is the rate at which the foreign output of the second commodity responds to a unit change in the capital endowment, given the price ratio but assuming equilibrating adjustments in both industries.

[8] With constant-returns technology, a given price ratio, and incomplete specialization, changes in the factor endowment give rise to changes in outputs but not to changes in factor proportions and (therefore) not to changes in marginal products.

[9] Those boundary cases in which the *home* country is completely specialized or in which K lies at one of the upper and lower limits determined by the initial endowments will be considered in paragraph 12.

[10] In writing equation (2b) use has been made of the fact that $\partial X_2/\partial K = dr_1^*/dp^*$. This remarkable equality is proved in the Appendix. In writing equation (2c) use has been made of the fact that, given p^*, changes in K have no effect on net foreign real income and therefore no effect on foreign demand. Changes in K therefore affect $E_2^*(=D_2^* - X_2^*)$ solely through X_2^*: $\partial E_2^*/\partial K = -\partial X_2^*/\partial K$. Use has been made also of the relation: $-\partial\varphi/\partial K = r_1/p$.

7. The first of these three conditions states simply that the marginal social rate of substitution between the two commodities must equal the slope of the production frontier. This condition will be met under competitive conditions, when both are equal to the home price ratio.

8. The second condition may be interpreted as defining the optimal rate of duty. As we have just noted, $U_2/U_1=p$; also, $p=p^*/(1+\tau_1)$, where τ_1 is the *ad valorem* rate of import (or export[11]) duty. The second condition may be written, therefore, as

$$1/(1+\tau_1) = (1+\eta_2^*+\epsilon_2^*)/\eta_2^*$$

where $\epsilon_2^* \equiv (K/E_2^*)(\partial X_2^*/\partial K)$ is the foreign elasticity of import production with respect to borrowed capital. It follows that the optimal rate of duty is

$$\tau_1 = -(1+\epsilon_2^*)/(1+\eta_2^*+\epsilon_2^*). \tag{3}$$

Now $\partial X_2^*/\partial K$, and therefore ϵ_2^*, may be of either sign: if the second commodity is relatively capital-intensive abroad, $\partial X_2^*/\partial K$ is positive; if the second commodity is relatively labor-intensive, $\partial X_2^*/\partial K$ is negative.[12] It follows that the optimal tariff may be of either sign, or zero.

Of special interest is the zero-indebtedness case, in which $K=0$. For then $\epsilon_2^*=0$ and the optimal rate of duty takes its familiar value:

$$\tau_1 = -1/(1+\eta_2^*). \tag{3'}$$

It is not surprising perhaps that free trade is optimal if the foreign import demand is perfectly elastic ($\eta_2^*=\infty$) or, when the home country is a net creditor and imports *both* commodities, if the foreign supply of the second commodity is perfectly elastic ($\eta_2^*=+\infty$).

9. The interpretation of the third condition, contained in eq. (2c), is a little more intricate. Suppose that $K>0$, that is, that the home country is a net creditor. Earnings per unit of capital, expressed in terms of the first commodity, are r_1^*. Suppose further that foreign earnings are subjected to a special income tax at a rate of $100t_1$ per cent. In competitive equilibrium the rate of return on capital will be the same in both countries; that is,[13]

[11] If, in the optimum, each country is an exporter, the tariff can be applied indifferently to the home country's imports of the first commodity or to its exports of the second commodity; the assignment of subscripts to τ is of no consequence. If, however, the home country is a net debtor and imports neither commodity, τ_1 must be interpreted as an *export* duty (levied on the *second* commodity). If, at the other extreme, the home country is a net creditor and exports neither commodity, the only possible interpretation of τ_1 is as an import duty (levied on the *first* commodity).

[12] Rybczynski [25].

[13] It is assumed here and later that no duty is levied on repatriated dividends. In the contrary case, considered in my earlier articles [10] [12], $r_1=r_1^*(1-t)/(1+\tau)$. The difficulty with the alternative assumption is that conclusions based on it do not carry over to a monetary economy.

(4) $$r_1 = r_1^*(1 - t_1).$$

From equations (2b), (2c), and (3), on the other hand,

(5) $$r_1 = r_1^*[1 + \epsilon_2^*(1 + \epsilon_2^*)/\mu_2\eta_2^*]$$

where $\mu_2 \equiv r_1^* K/p^* E_2^*$ is the ratio of the earnings on capital invested abroad to the value of exports. It follows from equations (4) and (5) that the optimal rate of tax on foreign earnings is

(6) $$t_1 = -\epsilon_2^*(1 + \epsilon_2^*)/\mu_2\eta_2^*.$$

If $K<0$, that is, the home country is a net debtor, it is the earnings on *foreign* capital which must be taxed. In competitive equilibrium

(7) $$r_1(1 - t_1^*) = r_1^*$$

where t_1^* is the rate of tax. It follows from equations (5) and (7) that the optimal rate of tax is

(8) $$t_1^* = \epsilon_2^*(1 + \epsilon_2^*)/[\mu_2\eta_2^* + \epsilon_2^*(1 + \epsilon_2^*)].$$

The tax rates t_1 and t_1^* are the counterparts, on capital account, of the more familiar optimal tariff. Like τ_1, they may be of either sign. Note that when $|\eta_2^*| = \infty$, so that free trade is optimal, it also is optimal to refrain from tinkering with the international movement of capital. When $K=0$, so that $\mu_2 = \epsilon_2^* = 0$, the optimal tax rates are, as one might have expected, indeterminate.

10. Inherent in equations (3), (6), and (8) is a quite confusing variety of possibilities. These are displayed in Figures 3a and 3b. Figure 3a covers the case in which the home country is a net creditor and Figure 3b describes the possibilities when the home country is a net debtor. (Note, however, that Figure 3a does double duty: when E_2^* is negative all t_1-signs are reversed.) Depending on the values assumed by η_2^* and ϵ_2^*, it seems that any sign combination of rate of duty and tax rate is possible, that in particular both may be negative. But not all of these possibilities are plausible; indeed some regions may be ruled out altogether. This will now be shown.

First the elasticity of foreign import demand is decomposed into its pure substitution and income components. Thus

(9) $$\begin{aligned}\eta_2^* &= \frac{p^*}{E_2^*}\cdot\frac{\partial E_2^*}{\partial p^*}\\ &= \frac{p^*}{E_2^*}\left\{\frac{\partial D_2^*}{\partial p^*}\bigg|_{y_1^*\ \text{const.}} + \frac{m_2^*}{p^*}\cdot\frac{\partial y_1^*}{\partial p^*} - \frac{\partial X_2^*}{\partial p^*}\right\}\\ &= \bar{\eta}_2^* + \frac{m_2^*}{E_2^*}\cdot\frac{\partial y_1^*}{\partial p^*}\end{aligned}$$

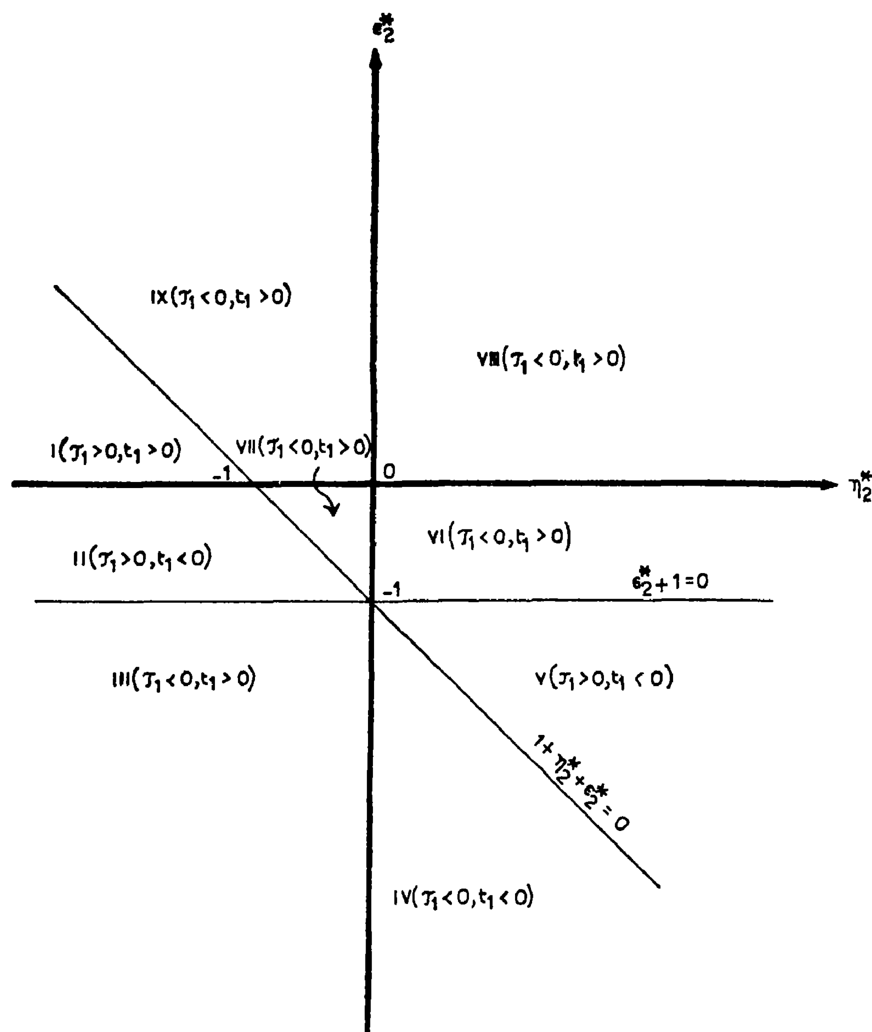


FIGURE 3a. SIGN COMBINATION OF τ_1 AND t_1

where

$$\bar{\eta}_2^* \equiv \frac{p^*}{E_2^*}\left\{\frac{\partial D_2^*}{\partial p^*}\bigg|_{y_1^* \text{ const.}} - \frac{\partial X_2^*}{\partial p^*}\right\}$$

is the pure elasticity of substitution, $m_2^* \equiv p^*(\partial D_2^*/\partial y_1^*)$ is the foreign marginal propensity to consume the second commodity, and y_1^* is the real income of the foreign country (that is, the cost, in terms of the first commodity, of the initial quantities consumed). Now

$$(10)\qquad \frac{\partial y_1^*}{\partial p^*} = \frac{\partial D_1^*}{\partial p^*} + p^* \frac{\partial D_2^*}{\partial p^*} \qquad \text{by definition}$$

$$= -\,E_2^*(1 + \epsilon_2^*)$$

from the budget restraint (1) and the duality relation of the Appendix.

Hence, substituting in equation (9),

$$(11)\qquad \eta_2^* = \bar{\eta}_2^* - m_2^*(1 + \epsilon_2^*).$$

Adding $(1+\epsilon_2^*)$ to both sides we have also

$$(12)\qquad 1 + \eta_2^* + \epsilon_2^* = \bar{\eta}_2^* + m_1^*(1 + \epsilon_2^*).$$

Since $\bar{\eta}_2^*$ is the sum of the two pure substitution elasticities, of demand and production, it must be negative when E_2^* is positive and positive

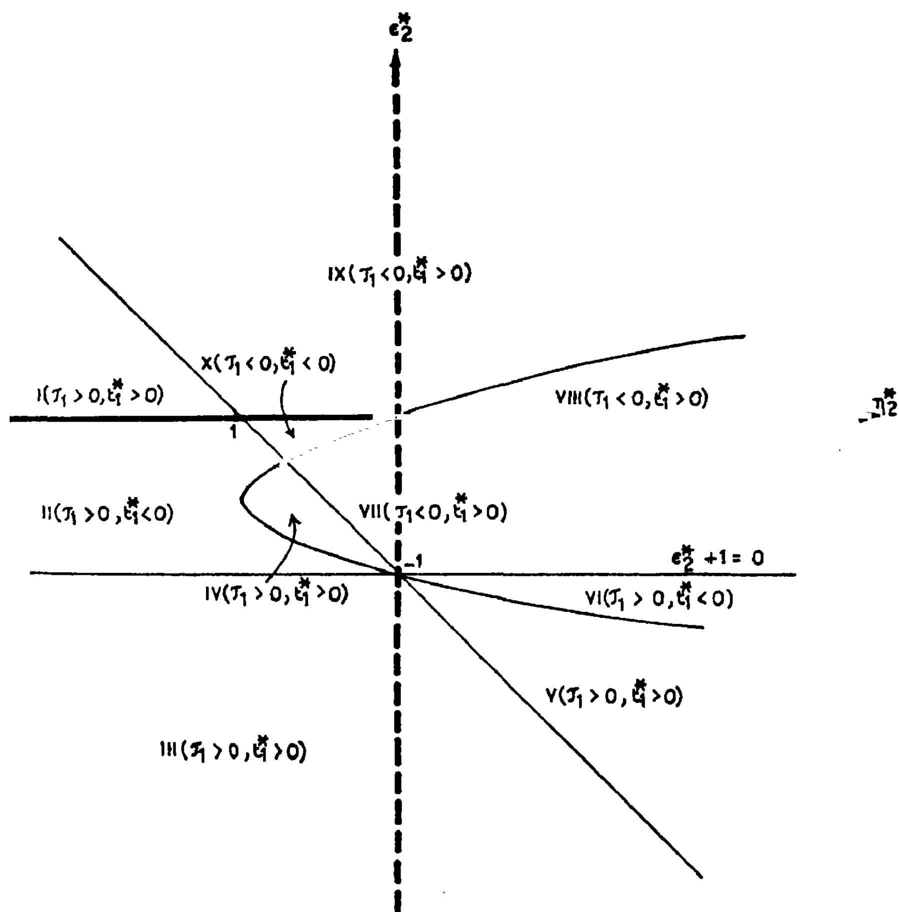


FIGURE 3b. SIGN COMBINATIONS OF τ_1 AND t_1^*

when E_2^* is negative. It then follows, from equation (11), (a) that when E_2^* is positive, positive η_2^* requires negative $(1+\epsilon_2^*)$ and therefore, from equation (12), negative $(1+\eta_2^*+\epsilon_2^*)$, and (b) that when E_2^* is negative, negative η_2^* requires positive $(1+\epsilon_2^*)$ and therefore, from equation (12), positive $(1+\eta_2^*+\epsilon_2^*)$:

$$\text{(13a)} \qquad \text{if } E_2^* > 0 \quad \text{then} \quad \eta_2^* > 0 \quad \textit{implies} \quad 1 + \eta_2^* + \epsilon_2^* < 0$$

$$\text{(13b)} \qquad \text{if } E_2^* < 0 \quad \text{then} \quad \eta_2^* < 0 \quad \textit{implies} \quad 1 + \eta_2^* + \epsilon_2^* > 0.$$

Further restrictions may be inferred from the requirements (i) that import and export subsidies must fall short of 100 per cent (if they did not, competition would force the price of the subsidized commodity to zero or below) and (ii) that the rate of income tax must fall short of 100 per cent. Thus, from equation (3),[14]

$$\text{(14)} \qquad -1 < \tau_1 = -(1 + \epsilon_2^*)/(1 + \eta_2^* + \epsilon_2^*)$$

and, from equations (6) and (8), respectively,

$$\text{(15a)} \qquad 1 > t_1 = -\epsilon_2^*(1 + \epsilon_2^*)/\mu_2\eta_2^*$$

and

$$\text{(15b)} \qquad 1 > t_1^* = \epsilon_2^*(1 + \epsilon_2^*)/[\mu_2\eta_2^* + \epsilon_2^*(1 + \epsilon_2^*)].$$

It is possible that further restrictions on η_2^* and ϵ_2^* can be teased out of the higher order conditions for a maximum, but I have been unable to do so.

When all restrictions are taken into account we are left with the possibilities displayed in Figures 4a–4c. Figure 4a illustrates the case in which, in the optimum, the home country is a net creditor and continues to export the second commodity (so that $K>0$, $\mu_2>0$); Figure 4b illustrates the case in which the home country is a net creditor but imports both commodities (so that $K>0$, $\mu_2<-1$, and the optimum occurs in the fourth quadrant of Figure 1); Figure 4c illustrates the case in which, in the optimum, the home country is a net debtor and exports the second commodity only (so that $K<0$, $-1<\mu_2<0$); and Figure 4d illustrates the remaining case in which the home country is a net debtor and exports both commodities (so that $K<0$, $\mu_2<-1$, and the optimum occurs in the second quadrant of Figure 1).

The most striking feature of Figures 4a and 4b, which between them cover the case in which the home country is a net creditor, is the possibility that the optimal tariff and/or the optimal tax may be zero or negative. Both may be zero, so that free trade in goods and unrestricted

[14] Alternatively, inequality (14) may be obtained by noting that the concavity of U rules out negatively sloped sections of the foreign country's offer curve (cf. Figure 1). The detailed proof is omitted.

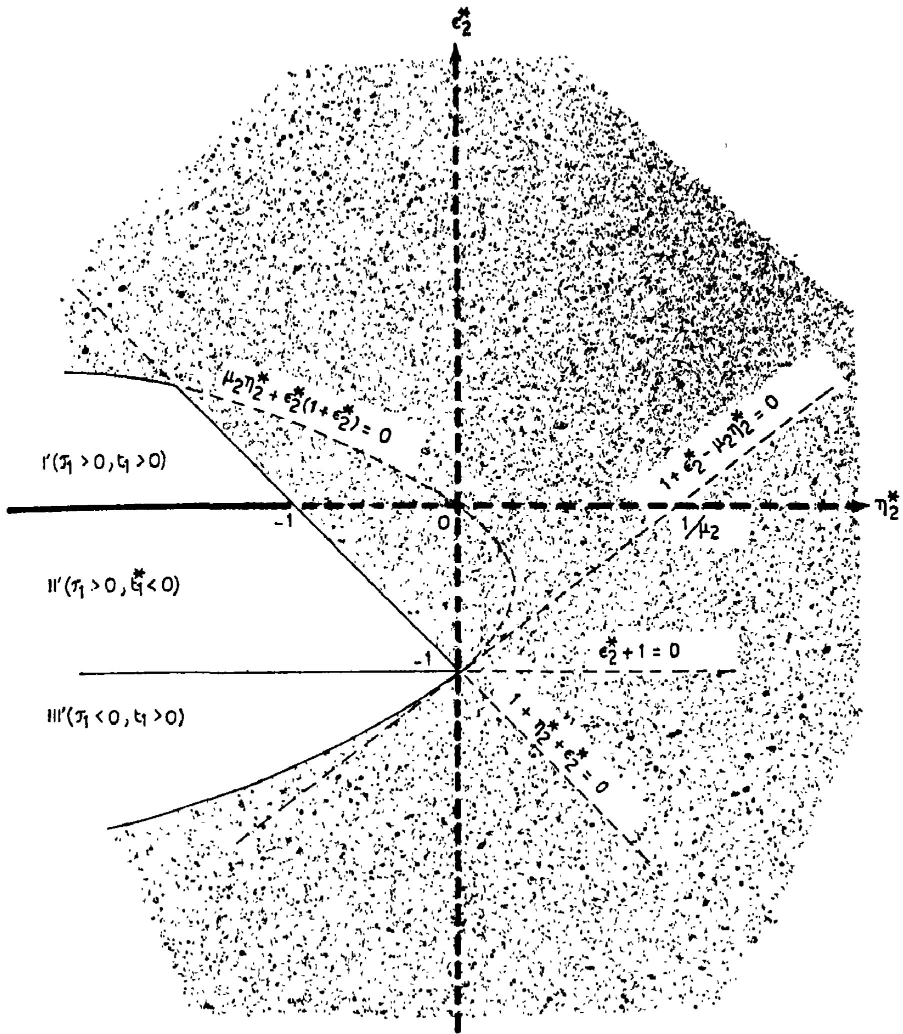


FIGURE 4a. $K>0$, $\mu_2>0$

capital mobility are optimal; and, if the home (or creditor) country imports both commodities, both may be negative. As Figures 4c and 4d make clear, however, there is not in the case of net home indebtedness quite the same richness of possibilities. In that case the optimal tariff or the optimal tax may be negative; but it is not possible for both to be negative, even when the home (or debtor) country exports both commodities.

It is noteworthy also that in none of the four cases distinguished is the elasticity of foreign import demand restricted to values algebraically less than minus one. This conclusion contrasts with a standard result of the

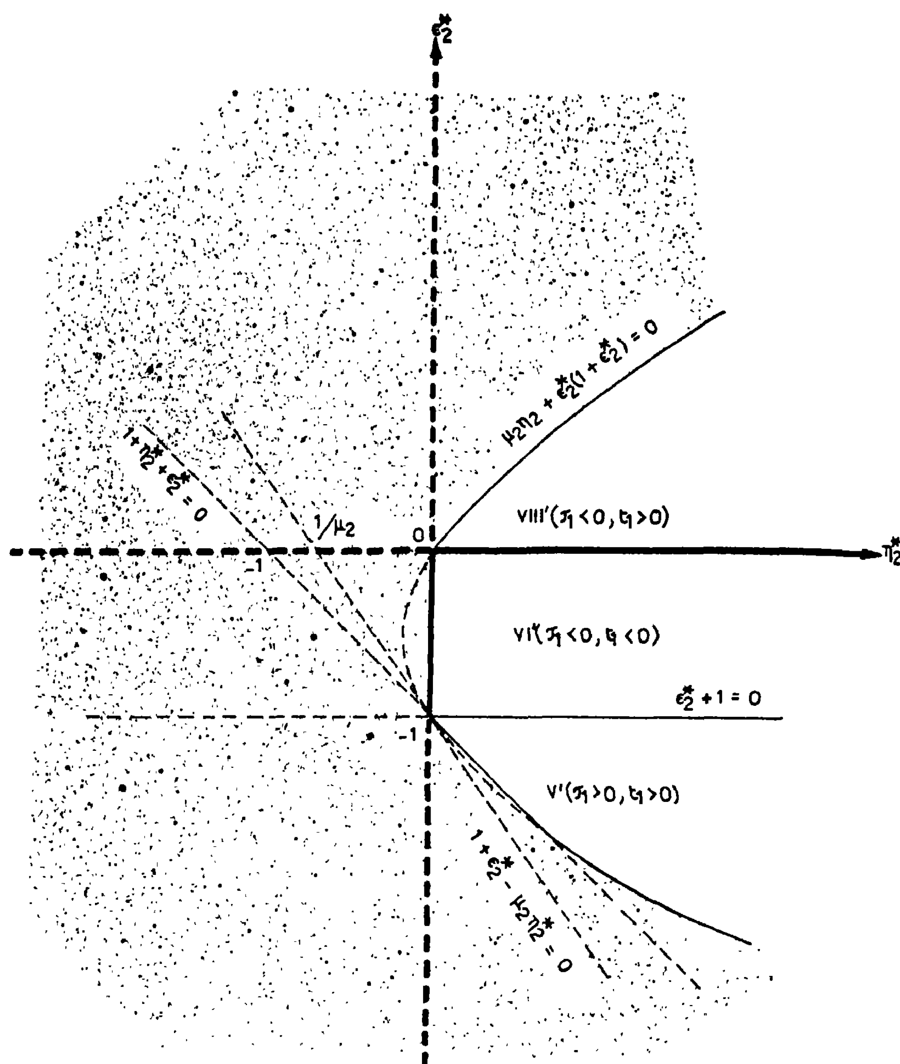


Figure 4b. $K>0$, $\mu_2<-1$

theory of optimal tariffs and reflects the fact that a variation of the terms of trade affects not only foreign import demand but also the rate of return on foreign investment. Thus if sufficient additional investment income is generated thereby, it will pay the home country to push the terms of trade below the point of zero marginal export income.

That an optimal tariff or tax may be negative is paradoxical. To help dispel the paradox, consider the following line of thought, valid for the case of capital export. When it parts with a unit of capital, the home country sacrifices production valued in terms of the first commodity at

r_1. This loss must be weighed against the additional goods made available by the foreign country after its economy has adjusted to the increment of capital. The net addition to the flow of goods available to the home country is, in terms of the first commodity, $-[(\partial E_1^*/\partial K)+p(\partial E_2^*/\partial K)+r_1]$ or, since net foreign real income, and therefore foreign demand, is unaffected by the capital movement, $(\partial X_1^*/\partial K)+p(\partial X_2^*/\partial K)-r_1$. Now the rate of return abroad is $r_1^*=(\partial X_1^*/\partial K)+p^*(\partial X_2^*/\partial K)$. Hence the net social return on the marginal investment is $[r_1^*-r_1+(p-p^*)(\partial X_2^*/\partial K)]$. In an optimum this expression must equal zero. It follows that the optimal tax, t_1, is positive if the home country places a lower valuation on the commodity the foreign production of which has increased than does the foreign country. This occurs if the home country's export good is capital-intensive abroad (so that $\partial X_2^*/\partial K>0$) and a tariff is levied (so that $p<p^*$): Region I′ of Figure 4a and Region V′ of Figure 4b; or if the export good is labor-intensive

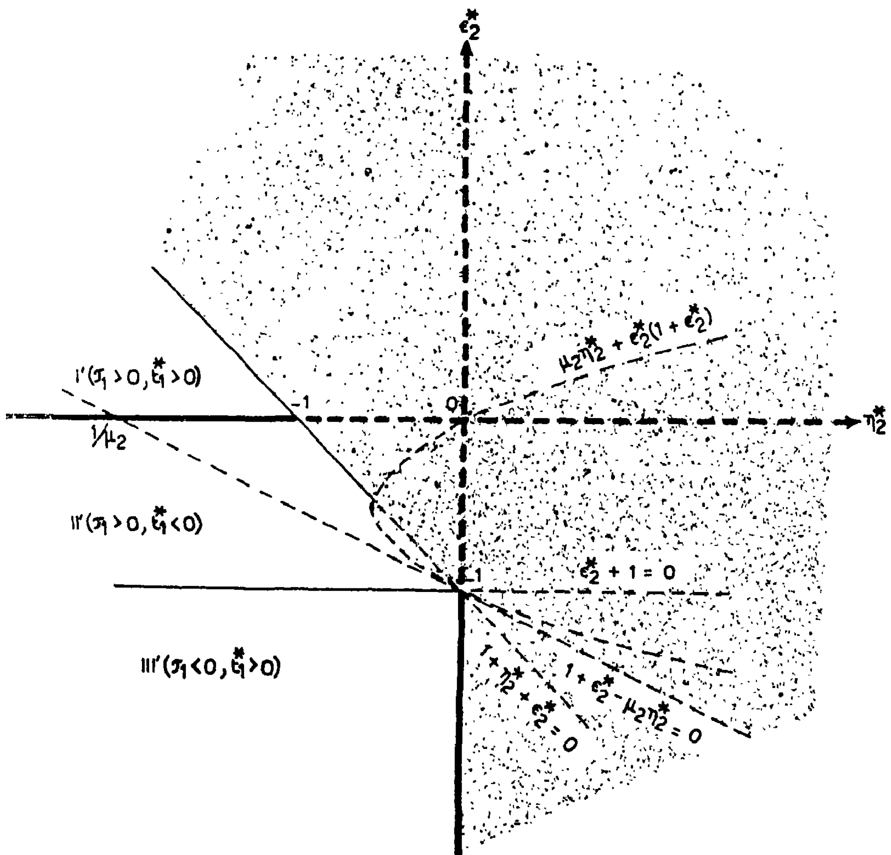


FIGURE 4c. $K<0$, $-1<\mu_2<0$

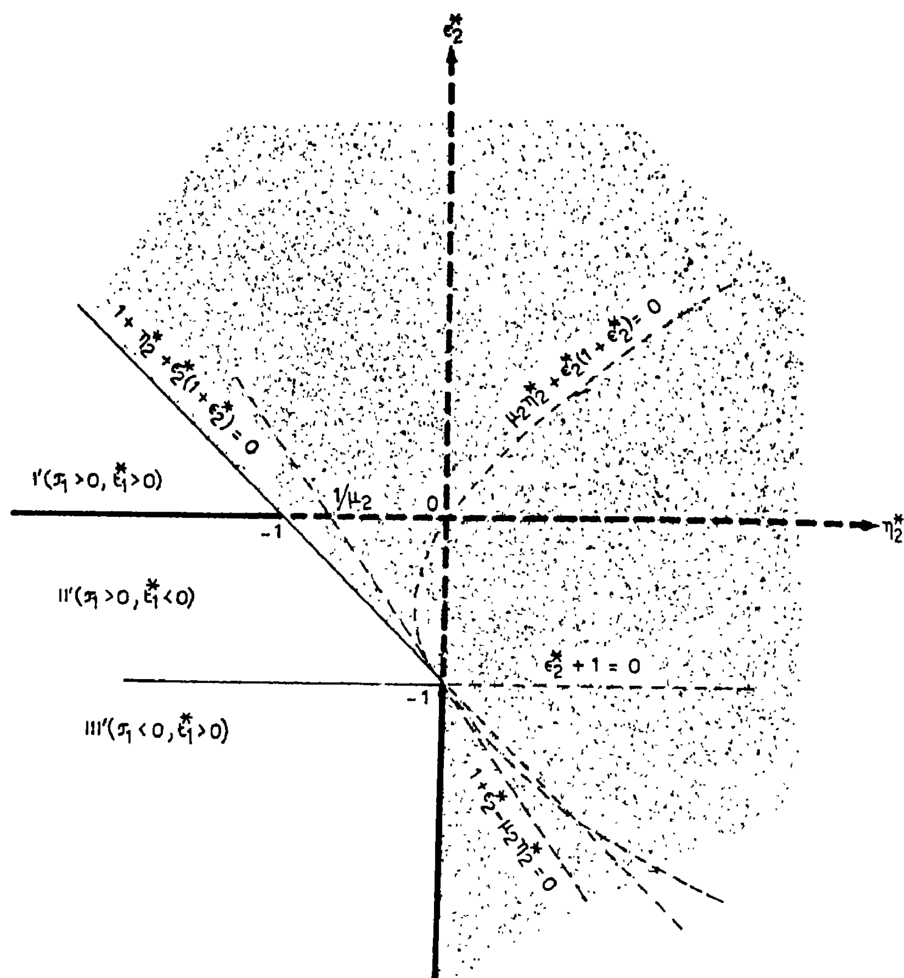


FIGURE 4d. $K<0$, $\mu_2<-1$

abroad (so that $\partial X_2^*/\partial K<0$) and an export subsidy is paid (so that $p>p^*$): Region III′ of Figure 4a and Region VIII′ of Figure 4b. Obversely, a subsidy on foreign earnings is optimal if the home country places a higher valuation on the commodity the foreign production of which has increased than does the foreign country. This occurs if the export good is labor-intensive abroad and an import duty is levied: Region II′ of Figure 4a; or if the export good is capital-intensive abroad and an export subsidy is paid: Region VI′ of Figure 4b.

A similar line of thought may be developed for the case of capital import. It remains true that in an optimum $[r_1^*-r_1+(p-p^*)(\partial X_2^*/\partial K)]$ must vanish. Hence the optimal tax, t_1^*, must be positive if the home country places a lower valuation on the commodity the foreign produc-

tion of which has increased than does the foreign country. This occurs if the home country's export good is labor-intensive abroad and a tariff is levied (so that $p < p^*$): Region I′ of Figures 4c and 4d; or if the home country's export good is capital-intensive abroad and an export subsidy is paid: Region III′ of Figures 4c and 4d. Obversely, a subsidy on foreign earnings is optimal if the home country places a lower valuation on the commodity the foreign production of which has decreased than does the foreign country. This occurs if the home country's export good is capital-intensive abroad and a tariff is levied: Region II′ of Figures 4c and 4d.

11. If both the optimal import duty and the optimal income tax are negative, net tax revenue also is negative. However, it is not in this case only that the operation of an optimal system of tariffs and taxes will leave the Treasury out of pocket. Whether the home country is on balance a creditor or on balance a debtor, net revenue is

$$p^* E_2^*(1 + \epsilon_2^*)(1 + \epsilon_2^* + \mu_2)/\eta_2^*. \tag{16}$$

From Figures 4a–4d it is clear that, in an optimum, $-p^* E_2^*/\eta_2^*$ is positive. Hence, if ϵ_2^* lies between -1 and $-(1+\mu_2)$, as it does in Region VI′ of Figure 4b, expression (16) is negative; otherwise, it is positive. When the home country is a net creditor and continues to export (Figure 4a), net revenue is necessarily positive if $\epsilon_2^* > 0$, that is, if the second commodity is relatively capital-intensive abroad; it is a necessary, but not sufficient, condition of negative net revenue that the second commodity be relatively labor-intensive abroad. When the home country is a net debtor and continues to import (Figure 4c), net revenue is necessarily positive if $\epsilon_2^* > 0$, that is, if the second commodity is relatively labor-intensive abroad; it is a necessary, but not sufficient, condition of negative net revenue that the second commodity be relatively *capital*-intensive abroad. In other cases (illustrated by Figures 4b and 4d) no statements of comparable simplicity seem possible.

12. So far we have confined our attention to interior optima. Specifically, we have put aside both the possibility that it is optimal for the home country to specialize completely and the possibility that the optimal K assumes one or other of the two extreme values determined by national capital endowments.

Fortunately our conclusions, with minor modifications, cover also the possibility of complete home specialization. For then $X_1 = 0$ and the welfare index becomes

$$U\{0 + p^* E_1^*(p^*, K) + r_1^*(p^*)K; \varphi(0, K) - E_2^*(p^*, K)\},$$

a function of two parameters only, p^* and K. The first-order conditions are identical with those contained in equations (2b) and (2c), hence the optimal tariff and tax are as set out in equations (3), (6), and (8). Equa-

tion (2a) no longer holds, and must be replaced by a suitable inequality.

Similarly, if the optimal K lies at its upper limit, set by the home country's capital endowment, equation (2c), and therefore equation (6) must be replaced by an inequality. Equation (6), for example, becomes

$$t_1 \geq -\epsilon_2^*(1+\epsilon_2^*)/\mu_2\eta_2^*. \tag{6'}$$

If, on the other hand, K lies at its lower limit, set by the foreign country's capital endowment, equation (8) becomes

$$t_1^* \geq \epsilon_2^*(1+\epsilon_2^*)/[\mu_2\eta_2^* + \epsilon_2^*(1+\epsilon_2^*)]. \tag{8'}$$

13. It has been assumed also that the home country exports the second commodity. The analysis of the alternative possibility is, however, symmetrical with that already provided. All results may be obtained by simply permuting the subscripts in equations (3), (6), and (8).

III. *Solution of the Problem: Complete Foreign Specialization*

14. The discussion of Sec. II was limited to the case of incomplete foreign specialization. The same general approach can be applied to the classical case in which the foreign country produces one commodity only. The cases differ, however, in two important respects. In the first place, the return on foreign investment, r_1^*, now depends on the level of net indebtedness, K, but not on the terms of trade, p^*; specifically $\partial r_1^*/\partial p^* = \partial X_2^*/\partial K = \epsilon_2^* = 0$, and $\partial r_1^*/\partial K < 0$. Second, it is no longer true that net foreign real income is, for given terms of trade, invariant under changes in K. Thus, if the foreign country is completely specialized in the production of the first commodity, we have

$$\begin{aligned}\frac{\partial E_2^*}{\partial K} = \frac{\partial D_2^*}{\partial K} &= \frac{\partial D_2^*}{\partial I_1^*}\cdot\frac{\partial I_1^*}{\partial K} \\ &= \frac{m_2^*}{p^*}\left(-K\frac{\partial r_1^*}{\partial K}\right) \\ &= -\frac{m_2^*}{p^*} r_1^*\delta_1\end{aligned}$$

where $\delta_1 \equiv (K/r_1^*)(\partial r_1^*/\partial K)$ is the elasticity of the foreign rate of return with respect to foreign investment, and I_1^* is the income of the foreign country in terms of the first commodity.

In view of these differences, equation (3) must be replaced by

$$\tau_1 = -1/(1+\eta_2^*), \tag{3'}$$

equation (2c) becomes

(2c′) $$U_2/U_1 = r_1^*(1 + \delta_1 m_1^*) \bigg/ \left[\frac{r_1}{p} - \frac{m_2^* r_1^* \delta_1}{p^*}\right],$$

equation (5) is replaced by

(5′) $$r_1 = r_1^*\{1 + \delta_1[m_1^* + m_2^*(1 + \eta_2^*)/\eta_2^*]\},$$

and equation (6) becomes

(6″) $$t_1 = -\delta_1[m_1^* + m_2^*(1 + \eta_2^*)/\eta_2^*].$$

Finally, (8) is replaced by

(8″) $$t_1^* = \frac{\delta_1[m_1^* + m_2^*(1 + \eta_2^*)/\eta_2^*]}{1 + \delta_1[m_1^* + m_2^*(1 + \eta_2^*)/\eta_2^*]}.$$

Equations (6″) and (8″) may be reached also by a line of reasoning similar to that pursued on pp. 799-803.

15. It is easy to show that in the present case, with complete specialization abroad, both the optimal tariff and the optimal tax must be positive. That is, it never pays to subsidize imports or exports, nor does it pay to subsidize borrowing or lending. To see that the optimal tariff must be positive, note first that if the foreign country is completely specialized, equation (10) reduces to

$$\frac{\partial y_1^*}{\partial p^*} = -E_2^*,$$

and that (11) therefore reduces to

(17) $$\eta_2^* = \bar{\eta}_2^* - m_2^* < 0;$$

equation (13), on the other hand, reduces to

(18) $$-1 < \tau_1 = -1/(1 + \eta_2^*).$$

From equations (17) and (18) it follows that $\eta_2^* < -1$.

To see that the optimal tax must be positive one need only note that $(1+\eta_2^*)/\eta_2^* = 1/(1+\tau_1) = p/p^* > 0$ and that $\delta_1 \gtrless 0$ as $K \lessgtr 0$.

That both the optimal tariff and the optimal tax must be positive is of considerable intrinsic interest for, at least since Ricardo, the case of complete specialization has possessed a special fascination for trade theorists. But the result is also interesting for the light it throws on some of our earlier conclusions; in particular, it helps us to see why a negative tax may be optimal when the foreign country produces something of both commodities. If the foreign country is completely specialized there is a direct link between K and r_1^*. An increase in the amount lent abroad inevitably depresses the rate of return, hence it can never pay to subsidize foreign lending. When foreign specialization is incomplete, however,

the direct link is broken, and with that link goes the inevitability that the tax is positive. The several cases described towards the end of paragraph 7 become possible.

IV. *Brief Comments on the Results*

16. It has been shown how one may solve for the optimal K, p^*, and X_1, and for the associated τ's and t's. It should be noted that the solutions depend on η_2^*, the elasticity of foreign import demand. Putting aside the special, constant-elasticity case, however, the value of η_2^* depends on the price at which it is calculated. That in turn depends on the choice of U-function. Thus, the optimal rates of duty and tax vary with the specification of U. That the optimal tariff is in this sense not unique is well known;[15] we now see that the same is true of the optimal tax on foreign earnings.

17. Equations (3), (6), and (8) contain the term ϵ_2^*, based on $\partial X_2^*/\partial K$ both of which are unfamiliar. They are, however, easy to translate into parameters of production functions. This is done in the Appendix.

18. The discussion has run in terms of comparative stationary states. Under stationary conditions the optimal tariff and tax are themselves stationary. It should be possible, however, to generalize the analysis to accommodate steadily growing or declining resource endowments and steadily improving technologies. If the assumptions of the present paper are retained, the results are bound to be messy. If, however, the capital good is identified with one of the consumer goods—a characteristic simplification of modern growth theories—a fairly elegant analysis should be possible.[16]

19. It has been assumed that in its choice of tariff and tax rate the policy-making country is constrained only by considerations of demand and technology. One can imagine, however, a second-best situation in which the policy-making country is further constrained by, for example a commitment to free trade or to maximum rates of duty or by an international tax convention. The interesting questions then concern the extent to which a country can achieve its over-all commercial-investment objectives with only the commercial (or investment) lever to manipulate or, more generally, with constraints on the extent to which the levers can be manipulated. The analysis can be extended to cope with such questions, though it might prove convenient to rewrite the U-function in terms of the alternative parameters X_1, τ_1, and t_1 (or t_1^*)

20. There are in our model not two but three traded goods, two consumption goods and the services of capital, and the optimal tax can be considered as the optimal tariff on trade in capital services. It is well known that if there are three traded consumption goods, it may pay to

[15] Cf. Scitovsky [31]; Graaff [7, pp. 136–37].

[16] A first step in this direction has been taken by Professor Takashi Negishi in [21].

subsidize trade in one of them (Graaff [7]). It now appears that the result can be extended by allowing one of the traded goods to be intermediate. On the other hand, it appears that the possibility of negative net revenue carries over to the case in which all traded goods are consumer goods.

21. If more than two countries are recognized the analysis becomes much more complicated, for then the pursuit of an optimum may require discrimination among the home country's trading partners. Thus, the optimal rate of duty on any particular good will depend on the amount invested in the supplying country; and the optimal tax on earnings derived from a particular foreign country will depend on the amount of trade carried on with that country and on the ϵ_1^* values of that country.

22. Finally, the reader is reminded of the long list of assumptions contained in paragraph 3, in particular of the assumptions of full employment and perfect competition. He might try his hand at a Keynesian (or neo-Mercantilist) reworking of the paper, or he might ask himself how many of our conclusions would be helpful to an oil sheik in his negotiations with the larger oil companies.

Appendix

23. In deriving equation (2b) use was made of the dual relationship $dr_1^*/dp^* = \partial X_2^*/\partial K$. This appendix contains a proof of that equality. Actually, the relationship was stated and proved some years ago by Paul Samuelson [29, p. 10]. But the proof was barely sketched and is, in any case embedded in an especially difficult article. The following straightforward proof may therefore be of interest.

24. If the foreign country is completely specialized in the production of its export commodity, the proof is trivial. For then, $\partial X_2^*/\partial K = 0$ and, since $r_1^* = \partial X_1^*/\partial K$, $dr_1^*/dp^* = 0$. In what follows, it will be assumed that the foreign country produces something of both commodities.

25. Given constant returns to scale, the two foreign production relationships may be expressed as

$$X_i^* = k_i f_i(\rho_i) \qquad i = 1, 2 \tag{A1}$$

where X_i^* is the output of the ith commodity, k_i is the amount of capital employed in the ith industry, and ρ_i is the labor:capital ratio in the ith industry. In a competitive equilibrium the following marginal equalities hold:

$$\begin{aligned} f_1' - p^* f_2' &= 0 \\ r_1^* = (f_1 - \rho_1 f_1') &= p^*(f_2 - \rho_2 f_2'), \end{aligned} \tag{A2}$$

[$f_i' \equiv df_i/d\rho_i$ is the marginal productivity of labor in the ith industry; and $(f_i - \rho_i f_i')$ is the marginal productivity of capital in the ith industry]. Finally we have two "full employment" conditions.

$$k_1 + k_2 = K + \text{const.}$$
$$k_1\rho_1 + k_2\rho_2 = \text{const.} \qquad \text{(A3)}$$

Treating K and p^* as parameters, and differentiating the entire system (A 1–3) with respect to K, we obtain

$$\partial X_2^*/\partial K = -\rho_1 f_2/(\rho_2 - \rho_1) \qquad Q.E.D.$$

26. By similar reasoning,

$$dr_2^*/d(1/p^*) = \partial X_1^*/\partial K$$

where r_2^* is the foreign marginal product of capital in terms of the second commodity.

References

1. R. E. Baldwin, "The New Welfare Economics and Gains in International Trade," *Quart. Jour. Econ.*, Feb. 1952, *66*, 91–101.
2. T. Balogh and P. P. Streeten, "Domestic Versus Foreign Investment," *Bull. Oxford Univ. Inst. Stat.*, Aug. 1960, *22*, 213–24; revised version in P. Streeten, *Economic Integration*, Leyden 1961, Ch. 4.
3. C. F. Bickerdike, "The Theory of Incipient Taxes," *Econ. Jour.*, Dec. 1906, *16*, 529–35.
4. ———, review of A. C. Pigou, *Preferential and Protective Import Duties*, in *Econ. Jour.*, March 1907, *27*, 98–102.
5. A. K. Cairncross, *Home and Foreign Investment 1807–1913*, Cambridge 1953, Ch. 9; reprinted from *Rev. Econ. Stud.*, Oct. 1935, *3*, 67–78.
6. F. Y. Edgeworth, *Papers Relating to Political Economy*, Vol. 2. London 1925.
7. J. de V. Graaff, *Theoretical Welfare Economics*. Cambridge 1957.
8. Carl Iversen, *Aspects of the Theory of International Capital Movements*, London 1936, pp. 160–70.
9. A. E. Jasay, "The Social Choice Between Home and Overseas Investment," *Econ. Jour.*, March 1960, *70*, 105–13.
10. Murray C. Kemp, "Foreign Investment and the National Advantage," *Econ. Record*, March 1962, *38*, 56–62.
11. ———, "The Benefits and Cost of Private Investment from Abroad: Comment," *Econ. Record*, March 1962, *38*, 108–10.
12. ———, *The Pure Theory of International Trade*. New York 1964.
13. ———, "A Guide to Negishi," *Econ. Record*, Dec. 1965, *41*, 632–33.
14. J. M. Keynes, "Foreign Investment and National Advantage," *The Nation and Athenaeum*, Aug. 9, 1924, *35*, 584–87.
15. ———, "Some Tests for Loans to Foreign and Colonial Governments," *The Nation and Athenaeum*, Jan. 17, 1925, *36*, 564–65.
16. ———, *Treatise on Money*, Vol. 1, London 1930, pp. 343–46.
17. Abba P. Lerner, "The Symmetry Between Import and Export Taxes," *Economica*, N. S., Aug. 1936, *3*, 306–13.
18. G. D. A. MacDougall, "The Benefits and Costs of Private Investment

from Abroad: A Theoretical Approach," *Econ. Record*, March 1960, *36*, 13–35; reprinted in *Bull. Oxford Univ. Inst. Stat.*, Aug. 1960, *22*, 187–212.

19. J. S. Mill, *Essays on Some Unsettled Questions of Political Economy.* London 1948.
20. ———, *Principles of Political Economy*, ed. Sir. W. J. Ashley. London 1909.
21. Takashi Negishi, "Foreign Investment and the Long-Run National Advantage," *Econ. Record*, Dec. 1965, *41*, 628–32.
22. Ragnar Nurkse, "The Problem of International Investment Today in the Light of Nineteenth-Century Experience," *Econ. Jour.*, Dec. 1954, *64*, 744–58.
23. Lionel Robbins, *Robert Torrens and the Evolution of Classical Economics.* London 1958.
24. D. C. Rowan and I. F. Pearce, "A Framework for Research into the Real Effects of International Capital Movements," to be published in a volume of essays in honor of Professor Marco Fanno.
25. T. N. Rybczynski, "Factor Endowments and Relative Commodity Prices," *Economica*, N.S., Nov. 1955, *22*, 336–41.
26. Paul A. Samuelson, "Parable and Realism in Capital Theory: The Surrogate Production Function," *Rev. Econ. Stud.*, June 1962, *29*, 193–206.
27. ———, "The Gains from International Trade Once Again," *Econ. Jour.*, Dec. 1962, *72*, 820–29.
28. ———, "Social Indifference Curves," *Quart. Jour. Econ.*, Feb. 1956, *70*, 1–22
29. ———, "Prices of Factors and Goods in General Equilibrium," *Rev. Econ. Stud.*, 1953, *21* (1), 1–20.
30. Andrew Schonfield, *British Economic Policy Since the War*, rev. ed., London 1959, pp. 108–22.
31. Tibor Scitovsky, "A Reconsideration of the Theory of Tariffs," *Rev. Econ. Stud.*, summer 1941, *9*, 89–110; reprinted in H. S. Ellis and L. A. Metzler, eds., *Readings in the Theory of International Trade*, Philadelphia 1949, pp. 359–89.
32. Henry Sidgwick, *The Principles of Political Economy*, 2nd ed. London 1887.
33. Paul B. Simpson, "Foreign Investment and the National Economic Advantage: A Theoretical Analysis," in R. F. Mikesell, ed., *U.S. Private and Government Investment Abroad*, Eugene, Oregon 1962, Ch. 18.
34. H. W. Singer, "The Distribution of Gains Between Investing and Borrowing Countries," *Am. Econ. Rev., Proc.*, May 1950, *40*, 473–85.
35. Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations.* London 1928.
36. R. Torrens, *An Essay on the Production of Wealth.* London 1921.
37. ———, *The Budget. On Commercial and Colonial Policy.* London 1944.

PEAK-LOAD PRICING AND OPTIMAL CAPACITY UNDER INDIVISIBILITY CONSTRAINTS

By OLIVER E. WILLIAMSON*

The problem of peak-load pricing has been "solved" at least four times in the postwar literature (by Marcel Boiteux [1], Hendrik Houthakker [4], Peter Steiner [7], and Jack Hirshleifer [3]), and thus an additional treatment of this subject requires some justification.[1] Ours is threefold. First, the welfare motivation of these analyses has been generally lacking,[2] and it is therefore difficult to evaluate the results obtained. Second, the geometric techniques that have been provided are awkward and unconventional, require that costs be redefined for each change in the number of subperiods, and cannot easily be generalized to handle the case of unequal-duration-subperiod loads. We attempt to remedy each of these deficiencies and, in addition, extend the analysis of peak-load pricing by replacing the usual assumption of fully divisible plant by one in which investment opportunities are discrete. This latter may be of little practical importance,[3] but it helps shed additional insights on the welfare attributes of the problem.

* The author is associate professor of economics at the University of Pennsylvania on leave as Special Economic Adviser to the Assistant Attorney General for Antitrust. The views expressed in this paper do not necessarily reflect those of the Department of Justice. Discussions with Jack Hirshleifer helped clarify some of the earlier work in the peak-load pricing area, and his comments on this paper are gratefully acknowledged. Research on this paper was supported by a grant to the author from the National Science Foundation and from the Lilly Foundation grant to the University of California, Los Angeles, for the study of the economics of property rights.

[1] The problem of peak-load pricing has also been treated recently by Arnold Harberger and N. Andreatta [2]. Their paper appears to be an application of the previous literature to the electricity-supply problems in India, with special attention to the question of optimal use of thermal and hydro capacity. Since they make no references to the earlier literature, their paper may represent still another "solution."

[2] Steiner sketches the welfare objective in a footnote [7, p. 587, n. 6] but fails to integrate the argument with the text beyond calling for a price-equal-marginal-cost result. Houthakker supplies a more comprehensive welfare motivation for his analysis, but he is concerned only with conditions that prevailed in Great Britain in the early 1950's. Since these conditions appear to be somewhat special, the applicability of the results is correspondingly circumscribed (which he explicitly points out [4, p. 1]). Our analysis, like those of Boiteux, Steiner, and Hirshleifer, is concerned with the general problem rather than with a particular set of time-place conditions. Our reference to "earlier treatments" in subsequent parts of this paper will therefore be restricted to these latter writers unless otherwise indicated.

[3] Houthakker claims that indivisibilities are unimportant in Great Britain since they operate in a fully interconnected system [4, p. 9]. Regions where interconnections are less extensive and economic generating units represent a nontrivial fraction of market size are presumably ones where indivisibilities could be significant. One caveat is, however, essential:

The welfare motivation for the analysis is supplied in Section I. Optimal pricing and the optimal adjustment of capacity to changes in demand, given a single uniform class of demands but indivisible plant, are investigated in Section II. The principal part of the paper is Section III, where a perfectly general geometric technique for combining periodic loads, given that costs are linear and subperiod demands are independent, is provided. Once costs are correctly specified, we show that the problem of optimally adjusting capacity in a periodic-load situation can always be solved by use of our "effective demand for capacity" construction. Criteria for optimal pricing to peak and off-peak loads are also derived. As contrasted with the optimal capacity problem, optimal pricing to periodic demands is not a weighting problem but involves efficient use of existing capacity to service each demand class taken separately.

I. *The Social Welfare Function*

We simplify our analysis by assuming that all of the optimum conditions of production and exchange are satisfied elsewhere in the economy. This is a strong assumption and may vitiate the analysis in the minds of some. However, some such assumption is necessary if we are to avoid "second best" digressions, and implicitly this assumption underlies the analyses of Boiteux, Steiner, and Hirshleifer. We are therefore merely making explicit what has previously been implicit in the peak-load-pricing literature. Moreover, since this assumption supplies us with conditions which permit us to employ the theoretical apparatus of partial welfare economics *to derive benchmark relations* if nothing else, and as there is no well-specified alternative (the claim that our assumptions are not obviously satisfied needs to be articulated before it becomes useful), we will proceed on this basis.[4]

Social benefit will therefore be given by total revenue plus consumers' surplus, and social cost (treated as opportunity cost and assuming no technological externalities) will be separable into total pecuniary cost less intramarginal rent. Assuming that all factors are available to the enterprise in completely elastic supply, intramarginal rents will be zero and the net welfare gain is:

---

although the presence of indivisibilities implies lumpiness in certain inputs, the lumps cannot be so large as to produce changes in relative outputs and prices over the whole economy. If this were the case, our partial equilibrium approach would have to be replaced by a general equilibrium analysis.

[4] That assumptions of this sort are enormously helpful in producing insights on complex economic questions is illustrated by the remarkably insightful treatment of the fishery regulation problem by Ralph Turvey [8]. The reason we believe he was able to get so far with the fishery problem is precisely because he was willing to make simplifying assumptions and thereby provide a fundamental welfare motivation for his analysis.

$$W = SB - SC \tag{1}$$
$$= TR + S - TC$$

where W = net welfare gain, SB = social benefit, SC = social cos
TR = total revenue, S = consumers' surplus, and TC = total cost.

Differentiating this expression with respect to output, we obtain a necessary[5] and sufficient conditions for a maximum the familiar rela tions:

$$\frac{dW}{dQ} = \frac{d}{dQ}(TR + S) - \frac{d}{dQ}(TC) = 0 \tag{2}$$

whence $P - MC = 0$

$$\frac{d^2W}{dQ^2} = \frac{dP}{dQ} - \frac{d^2}{dQ^2}(TC) < 0. \tag{3}$$

Assuming that we have completely divisible plant, that capacity cost (defined more precisely in Section II) are β per unit per period, and tha operating costs are b per unit per period, we have:

$$TC = (b + \beta)Q, \tag{4}$$

so that by (2) the optimal scale (for a given uniform class of demands will be that value of Q for which $P = b + \beta$.

If the physical plant is taken as given and the only decision is one o pricing optimally within the capacity constraint ($\overline{Q}$), we have:

$$\max W = (TR + S) - bQ \tag{5}$$
$$\text{s.t.} \quad Q \leq \overline{Q}.$$

Setting this up as a Lagrangian

$$\max L(Q, \lambda) = (TR + S) - bQ - \lambda(Q - \overline{Q}), \tag{6}$$

[5] That the derivative of the total revenue plus consumers' surplus term ($TR+S$) with respec to quantity is simply price can be seen from the fact that

$$TR + S = \int_0^Q P(Q')dQ', \quad \text{where } P(Q') \text{ is}$$

the demand curve. Differentiating this expression with respect to Q yields

$$\frac{d}{dQ}(TR + S) = \frac{d}{dQ}\int_0^Q P(Q')dQ'$$
$$= P(Q).$$

As pointed out below, we treat $P(Q')$ as the uncompensated demand curve and thus consumers surplus is given by the Marshallian triangle. It is not necessary, however, to define consumers surplus in this way to show that the welfare gain is maximized by equating price to margina cost. But clearly this specification leads to the $P = MC$ result, which is all that we require ir order to support the argument in the text.

and differentiating partially with respect to Q and λ and equating to zero, we have:

$$P = b + \lambda \tag{7}$$

$$Q \leq \bar{Q}. \tag{8}$$

By the Kuhn-Tucker Theorem [5], if the constraint is not binding and equation (8) is satisfied as an inequality, λ is zero, and the optimal price is where output equates price to short-run marginal cost, namely b. When the capacity constraint is reached, however, the value of λ becomes positive, and the resulting price necessarily exceeds b. If $b+\lambda > b+\beta$ (and demand is expected to continue at this level), an expansion of plant is signaled, whereas if $b+\lambda < b+\beta$, plant should be retired.[6]

The above are the principal optimality rules for a system in which plant is fully divisible and a single uniform class of demands exists. We turn now to consider the effects of relaxing these two assumptions. First, however, two characteristics of our welfare function not explicitly stated above are noted: (1) benefits and costs are weighted equally "to whomsoever they may accrue," and thus society is indifferent to the income redistribution effects under this formulation; (2) the welfare function can be rearranged in a form more convenient for the subsequent exposition by expressing it as

$$W = S + (TR - TC) \tag{1'}$$

where the first term is the consumers' surplus and the second term the producers' net revenue. For purposes of symmetry (although contrary to the standard usage, where the expression applies only to rents to intramarginal factors) we will refer to this net revenue term as "producers' surplus."

II. *Uniform Load and Indivisible Plant*

As already indicated, we distinguish between operating costs and capacity costs. Operating costs are mainly the energy costs of generation and transmission. Marginal operating costs (short-run marginal costs) are assumed to be constant, at a rate of b per unit per period, so long as output is less than capacity. When capacity is reached, however, marginal operating costs become effectively infinite. Thus a sharp kink develops at the existing capacity level.

Plant can be efficiently supplied only in integer multiples of output units of size E and cost Γ. The foregone alternative is an equivalent risk

[6] If $b+\lambda > b+\beta$, clearly $\lambda > \beta$ and the shadow price of the constraint exceeds the cost of installing an additional unit of capacity. Hence plant expansion is indicated. Where $b+\lambda < b+\beta$, so that $\lambda < \beta$, plant should be retired rather than renewed until an equality between λ and β is restored.

annuity which pays an amount γ per period over the useful life of the plant. Average capacity costs per period of a fully utilized efficiency unit of size E are thus γ/E plus the average maintenance cost per period, the sum of which we will call β. Subject to these indivisibility conditions, constant returns to scale prevail and long-run marginal costs are given by $b+\beta$.

P
V
U
SRMC 1
SRMC 2
P1 H
I
b+β1 G
N
J
K
LRMC
P2 E
M
L
D2
D1
b
O
Q* 1 = nE
Q* 1 + E
Q

FIGURE 1

We assume that the enterprise is initially in a fully adjusted equilibrium position in which short-run marginal costs, long-run marginal costs, and price are all equal. This is shown in Figure 1 where the demand curve is D_1 and the output $Q_1^* = nE$, where n is a positive integer. Price in these circumstances will be $b+\beta$ so that total revenue equals total cost. Producers' surplus is therefore zero and the net gain, W, is

given by the consumers' surplus region UNG (assuming constant marginal utility of money over the relevant range).[7]

Assume now we have a once-for-all shift in demand to D_2. Under what conditions does it become optimal to add a new capacity unit? Clearly if D_2 were to pass through K so that at a price of $b+\beta$ the quantity demanded would be Q_1^*+E, a new efficiency unit would be warranted. But what if D_2 passes through some intermediate position between N and K such as J? Is there any justification for adding a new efficiency unit under these circumstances, and if so what price should be charged? Given our welfare function, the answer is clearly affirmative and the condition under which it becomes desirable is when the area IJN just exceeds the area JKL. With the new capacity unit in place, the appropriate price is given by the intersection of $SRMC_2$ with D_2, namely P_2.

To see that this is the condition under which the new efficiency unit should be added suppose that, with demand at the level D_2, the area IJN is just equal to JKL and the enterprise continues to operate with Q_1^* units of capacity. A price of P_1 will be required to ration capacity output under these conditions. Thus producers' surplus (total revenue less total cost) will be given by $HING$ and consumers' surplus will be VIH. The welfare gain will be the sum, or $VING$. Should a new efficiency unit be added and price set at OF (where D_2 and $SRMC_2$ intersect), we find producers' surplus reduced in the amount $HING+GJLF+JKL$ while consumers' surplus increases by $HING+GJLF+IJN$. Thus the net gain is $IJN-JKL$, which by assumption is zero. Hence, from a welfare point of view, one is presumably indifferent between remaining at Q_1^* and moving to Q_1^*+E. But whenever $IJN>JKL$ the net welfare gain will be positive, in which case the new efficiency unit should be added and, for all positions intermediate between J and K, the enterprise will be operated at a loss.

Indeed we see that, in this system with indivisible plant, the fully adjusted long-run static equilibrium can be one in which either positive or negative net profits are realized despite (discontinuously) constant returns to scale. Only accidentally[8] will the enterprise earn zero profits at the welfare maximum.

If the assumption that the shift from D_1 to D_2 is a once-for-all change is replaced by one in which D_2 is merely a transitory position in a system where demand is continuously increasing, does our criterion for installa-

[7] This is another simplifying assumption, but one which, we suspect, is not likely to be seriously violated. See Alfred Marshall [6, pp. 132, 334–35, 842] for a similar view that the inaccuracy involved in employing this assumption is usually negligible. D. M. Winch has recently argued that, even where the marginal utility of money is not constant, the Marshallian triangle continues to be an accurate measure of what he refers to as "consumers' gain" so long as income distribution is a matter of indifference [9, pp. 395–407, 422].

[8] This will occur with probability zero if demand is a random variable with continuous distribution function.

tion of the additional capacity require modification? In some superficial sense it might appear that these dynamic conditions would warrant installation prior to the time at which the static criterion is satisfied. It is obvious, however, that if the new capacity is added before the area $IJN \geq JKL$, the welfare gain is less in those periods when $IJN < JKL$ than it otherwise would be, and no type of discounting procedure that expresses future gains in terms of present values will alter this result. Thus the criterion for installation remains the same: add the new capacity when IJN first exceeds JKL, but not before.[9]

The introduction of uncertainty requires that the analysis be adapted accordingly. In circumstances such as these where we are prescribing optimal social policy and the risk of *system* ruin due to the failure of any particular *enterprise* is negligible, and where we are assuming that the mix of projects is suitably diversified so as to secure the advantages of pooling, special allowance for risk would appear unnecessary. Instead, simple conversion to expected values would seem appropriate so that the counterpart of our previous criterion is to add capacity whenever $E(IJN) \geq E(JKL)$, where $E(\cdot)$ denotes expectation.

III. *Peak and Off-Peak Loads*

The analysis of periodic loads requires that costs and demands be specified with more than ordinary care. Specifications that conveniently solve a special case (such as the principal case examined by Boiteux, Steiner, and Hirshleifer in which there are two loads, each of identical length) may be difficult or awkward to generalize. Even more serious, the "apparent" generalization may lead to error. We therefore take *the entire demand cycle* as the natural unit against which to express costs. Assuming that the "period" referred to in Sections I and II is the same length as an entire cycle (typically a day), the specification of costs used

[9] If there is an installation lag, of course, it will be necessary to anticipate demands in order for capacity to be operative at the desired time. With a positive rate of interest, the capacity should be timed to come into operation after $IJN > JKL$. This correction is trivial, however, and also applies if there is an installation lag under once-for-all changes in demand.

If the assumption of continuously increasing demand is replaced by one of secularly increasing demand with cyclical variation about the trend, the criterion still remains largely intact. Thus, installation lags aside, installation never precedes but is always made subject to the condition that $IJN \geq JKL$. However, whereas this was the necessary and sufficient condition for adding capacity under our welfare criterion given the static or continuously increasing demand conditions, it is only a necessary condition when cyclical variation is present. Whether the installation should be made at the time that demand first satisfies this relation, or be deferred to the second or *n*th time that the inequality $IJN < JKL$ is reversed, depends on the phase and frequency of cyclical variation. More specifically, our objective is to time the installation so as to maximize the discounted value of net benefits, and this need not coincide with the first switch-over from $IJN < JKL$ to $IJN \geq JKL$. Thus if, for example, the first switch-over occurred at the very peak of a cycle, installation at the time of the switch-over would be quite without purpose for the downturn would set in immediately and the additional capacity would exceed optimal ($IJN < JKL$).

previously applies directly to our analysis of peak-load pricing. Thus under our formulation there is no difference between the conventional costing practices used in the analysis of uniform loads and those used to study the periodic-load problem, whereas this is not true in each of the papers referred to above. We therefore define short-run marginal costs as the operating costs of supplying the incremental unit of output (at levels of operation less than capacity) over an entire cycle, namely b per unit per cycle. Since incremental capacity costs continue at the rate of β per unit per cycle, long-run marginal costs are $b+\beta$ per unit per cycle.

In order for this specification of costs to be "appropriate" to the problem of peak-load pricing, it is also necessary that demands be specified in a consistent way. In particular, it is necessary to weight each demand by the fraction of the cycle over which it prevails. Thus, demand is expressed as $D_i^{(w_i)}$, where i refers to the subperiod in question and superscript (w_i) to the fraction of the cycle during which the demand in question prevails, with each demand curve showing the amount of output per cycle which would be demanded at every price were the demand in question to prevail over the entire cycle. This permits us to handle subperiods of unequal duration, whereas equal-length subperiods are all that are explicitly dealt with (and appear to be implicit where the length of period is not stated) in earlier analyses. Thus our formulation is more general in this respect than those which have been developed previously. In order to simplify the analysis we make the conventional assumption that each periodic demand is independent. Whether or not this is a reasonable assumption is, of course, an empirical question. In circumstances where time patterns of consumption are relatively inflexible, the independence assumption is presumably a close approximation.

We assume divisible plant initially and consider the two-period case with an off-peak load of length 8 hours and a peak load of 16 hours, so that demand is expressed as $D_1^{(1/3)}$ and $D_2^{(2/3)}$, respectively. These demand conditions, together with the cost relations specified above, are shown in Figure 2.

Solving for the optimum plant size geometrically requires that we develop a technique for combining the individual periodic-load curves to obtain an over-all "effective demand for capacity" relation. For this purpose we note that, taking each periodic load by itself and assuming that the plant is operated only for the one load and is shut down during the remainder of the cycle, the price charged in the interval during which the plant is operated must be $b+\beta/w_i$ (where w_i is the fraction of the cycle during which load i prevails) in order that enterprise net revenue be zero. That is, under the conditions described, total revenue from operations will be $P_iQ_iw_i$, total costs will be $bQ_iw_i+\beta Q_i$, and only if $P_i=b+\beta/w_i$ will total revenue less total costs yield the zero net revenue

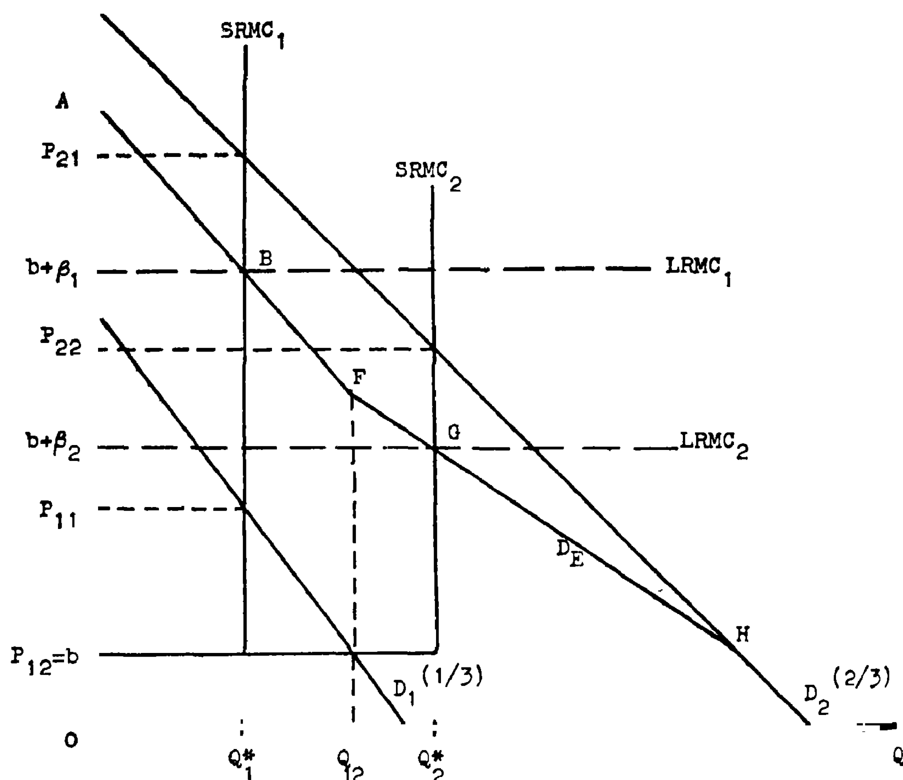


FIGURE 2

result. Since this latter condition necessarily holds in long-run equilibrium in a system with constant returns to scale and divisible plant, clearly the price of $b+\beta/w_i$ is correct. We want now to construct a demand-for-capacity curve for each periodic-load curve which has the property that it intersects the long-run marginal cost curve at the capacity level Q_i corresponding to the price $b+\beta/w_i$. If such a curve can be obtained by a simple transformation of the periodic-load curve, the appropriate capacity (and consequently price) can be found directly rather than through the indirect process described above.

A curve with these properties can be constructed by taking the vertical difference between the periodic-load curve and short-run marginal

costs (b), multiplying this difference by the fraction (w_i) of the cycle during which the periodic load in question prevails, and adding vertically this weighted demand-for-capacity curve to the short-run marginal cost curve. The resulting curve intersects $b+\beta$ at precisely the value of Q_i for which $P_i=b+\beta/w_i$ on the corresponding periodic-load curve. Extending the analysis so as to obtain an over-all demand-for-capacity curve requires that we combine the contributions to capacity made in every subperiod. This is accomplished by adding vertically to the short-run marginal cost curve the vertical summation of the individual weighted demand-for-capacity curves (obtained according to the procedure described above). The resulting effective demand-for-capacity curve for $D_1^{(1/3)}$ and $D_2^{(2/3)}$ is shown as D_E in Figure 2. It will be noted that D_E is kinked at F, which corresponds to output level Q_{12}, since for outputs that exceed Q_{12} the demand price along the off-peak-load curve is everywhere below $SRMC$. That this construction is correct and leads to a set of results that parallel those obtained for the uniform-load analysis in Section II can be seen by considering the solutions that obtain under two different values of capacity cost (β_1 and β_2) in Figure 2.

For $\beta=\beta_1$ the $LRMC_1$ intersects the demand-for-capacity curve D_E at B, so that the optimum scale is Q_1^*. $SRMC_1$ is horizontal at level b from O to Q_1^* and is vertical thereafter. Thus the appropriate output in each subperiod is Q_1^*, with the price being P_{11} in the off-peak-load period and P_{21} at peak load. Total revenue is given by $P_{11}Q_1^*(\frac{1}{3})+P_{21}(Q_1^*)(\frac{2}{3})$ while total cost is $(b+\beta_1)Q_1^*$. That these must be equal is seen by observing that for any value of β_1 for which the corresponding $LRMC$ curve intersects D_E anywhere in the range AF, and assuming that capacity is optimally adjusted and that P_{11} and P_{21} are chosen so as to equate $SRMC_1$ and demand price, the following relation will hold: $(b+\beta_1)-P_{11}=2[P_{21}-(b+\beta_1)]$. Thus, the amount by which revenues in the off-peak-load period fail to cover pro rata total costs is precisely offset by revenues in the peak-load period in excess of pro rata total costs. Put differently, net revenue is identically zero in this system with completely divisible plant so long as (1) the intersection of the demand-for-capacity curve and the long-run marginal cost curve is used to define plant capacity and (2) prices in each period are set at the level at which the short-run marginal cost curve intersects the respective periodic-load demand curve. Our construction of D_E guarantees this result.

Consider now a capacity cost of β_2 per unit per period. $LRMC_2$ now intersects D_E at G, so that the optimum scale is Q_2^*. $SRMC_2$ is horizontal at level b from O to Q_2^* and then moves vertically upward. The appropriate prices and outputs, therefore, are P_{12} $(=b)$ and Q_{12} in the off-peak period, and P_{22}, Q_2^* during peak load. The revenues and operating costs during the off-peak load are equal. Thus revenues during peak load must

be sufficient to cover both operating costs and the entire capital charge if zero net revenues are to be realized. Again, our construction of the effective demand-for-capacity curve, D_E, assures this result. To see this, we observe that, by virtue of this construction, P_{22} necessarily equals $b+(3/2)\ \beta_2$ at Q_2^*. Therefore revenue during peak load is $(b+3/2\beta_2)Q_2^*\cdot 2/3$, or $(2/3)(bQ_2^*+\beta Q_2^*$. Operating cost during peak load is $(2/3)(bQ_2^*)$. Hence the excess of revenue over operating cost during peak load is $\beta_2 Q_2^*$, which is the capacity cost per cycle, and zero net revenue results.

That positions B and G are optimal as claimed can also be seen by considering the effect on welfare of an incremental change in Q_1^* and Q_2^*, respectively. An increase in scale leads to a decrease in producers' surplus that exceeds the gain in consumers' surplus; a decrease in scale yields an increase in producers' surplus that is less than the loss of consumers' surplus. This can be judged by reference to either the D_E curve or the individual load curves taken separately.

The problem can also be formulated algebraically and identical results obtained. Using subscripts 1 and 2 to refer to off-peak and peak-load demands, respectively, and w_1 and w_2 as the corresponding fraction of a cycle accounted for by each (and $w_1+w_2\leq 1$, with the equality holding unless there is a third period in the cycle during which plant is shut down altogether), we have as our periodic-load counterpart of (1)

$$W = (TR_1 + S_1)w_1 + (TR_2 + S_2)w_2 - bQ_1w_1 - bQ_2w_2 - \beta Q_2. \tag{9}$$

The objective is to find the pricing rule in each subperiod and the optimum capacity, Q_2. If, as in case 1 described above, plant is utilized to capacity during both off-peak and peak-load periods, we have $Q_1=Q_2$. Letting $Q=Q_1=Q_2$, optimum plant size is obtained by differentiating

$$W = (TR_1 + S_1)w_1 + (TR_2 + S_2)w_2 - bQ(w_1 + w_2) - \beta_1 Q \tag{9'}$$

with respect to Q. Thus we obtain:

$$\frac{\partial W}{\partial Q} = P_{11}w_1 + P_{12}w_2 - b(w_1 + w_2) - \beta_1 = 0 \tag{10}$$

or

$$P_{11}w_1 + P_{12}w_2 = b(w_1 + w_2) + \beta_1.$$

Letting $w_1=1/3$ and $w_2=2/3$, this is precisely the result obtained from our geometry in Figure 2 for the case where $\beta=\beta_1$.

If instead plant is utilized to capacity only under peak-load demands, we have by differentiating (9) partially with respect to Q_1 and Q_2:

$$\frac{\partial W}{\partial Q_1} = P_{21}w_1 - bw_1 = 0 \tag{11}$$

or

$$P_{21} = b$$

$$\frac{\partial W}{\partial Q_2} = P_{22}w_2 - bw_2 - \beta_2 = 0 \tag{12}$$

or

$$P_{22} - b = \frac{\beta_2}{w_2}.$$

These are precisely the results obtained from our analysis of Figure 2 for $\beta = \beta_2$. Thus price in the off-peak interval is set equal to short-run marginal cost. The price during peak load is set at incremental operating cost (b) plus incremental capacity cost (β_2) divided by the fraction of cycle time accounted for by peak load (w_2).[10]

The multiperiod generalization of equations (9)–(12) is both easy and obvious. The welfare function in this instance becomes

$$W = \sum_{i=1}^{n} (TR_i + S_i)w_i - \sum_{i=1}^{n} bQ_iw_i - \beta Q^*,$$

where Q^* refers to optimum system capacity. Optimality requires that $P_i = b$ in those periods when plant is not utilized to capacity and, letting I be the subset of periods when capacity is fully utilized,

$$\sum_{i \epsilon I} (P_i - b)\, w_i = \beta$$

By way of summarizing the argument, we have as basic principles for dealing with periodic demands (assuming that costs and demands are defined as suggested above):

A. Optimal plant size (assuming fully divisible plant) is given by the intersection of the *LRMC* curve and the effective demand-for-capacity curve.
B. Optimal price in every subperiod is given by the intersection of the *SRMC* curve and the subperiod demand curve.

[10] If, as in Section I, we take capacity as given by the constraint $\overline{Q}$ and formulate the welfare-maximization problem as a Lagrangian, the parallel results to those obtained in equation (7) are:

(i) if $Q_1 = Q_2$ (the case where $\beta = \beta_1$)

$$P_{11}w_1 + P_{12}w_2 = b + \lambda_1,$$

(ii) if $Q_1 < Q_2$ (the case where $\beta = \beta_2$)

$$P_{12} = b$$

$$P_{22} = b + \frac{\lambda_2}{w_2}.$$

Again, λ_i is the shadow price associated with the constraint, and if $\lambda_i > \beta_i$ an expansion is signaled whereas if $\lambda_i < \beta_i$ a contraction is indicated.

C. In a fully adjusted, continuously utilized system with only two periodic loads:
   (a) Peak-load price always exceeds *LRMC*.
   (b) Off-peak-load price is always below *LRMC*.
   (c) Only when the off-peak load fails to use plant to capacity when priced at *SRMC* does the peak load bear the entire burden of the capacity costs.

Before extending the analysis to deal with indivisible plant, we consider first how our approach differs from those of Boiteux, Steiner, and Hirshleifer. With respect to Boiteux, the principal difference is that he uses only equal-length subperiods (both in his geometry and [apparently] in the appendix) and hence there is a lack of generality in his results. Although he solves the optimum pricing problem for the two-period case both geometrically and analytically, he lacks a device for aggregating periodic loads geometrically and thus solves the optimum capacity problem only analytically (and only for the special case of equal-length-subperiod loads).

Steiner and Hirshleifer each employs a geometric technique that correctly handles the peak-load capacity and pricing problem for the case of two equal-length-subperiod loads. Instead of using a weighted average of subperiod demands to obtain an effective demand-for-capacity curve in the way we have done, they sum the periodic loads vertically. This technique, together with the way in which they define their costs in order to support the technique, suggests that the periodic-load problem requires rather unconventional apparatus for its solution. It thus appears that the solution to the periodic-load problem is something other than a simple generalization of the usual uniform-load results. In addition, both Steiner's geometric treatment of the three-period case [7, pp. 590–91] and Hirshleifer's verbal generalization to the n—(equal length) period case [3, pp. 456–57] require that capacity costs be redefined for each condition. It is obvious, however, that capacity costs *per cycle* are independent of the number of subperiods included, and expressing costs in terms of the cycle, as we have, rather than for the subperiod, as they do, avoids this respecification problem. Moreover, since neither explicitly indicates the necessity for this respecification, uncritical implementation of their results easily leads to error.[11]

[11] Consider, for example, what seems to us the "natural" interpretation of Hirshleifer's argument. For the two-period case he observes that "If the demand price . . . for a single period alone did exceed $b+\beta$, an increase in capacity is obviously called for" [3, p. 456]. Generalizing to the n-period case he states that "more capacity should be added if . . . [in any of the n-periods] the demand [price] for *any* single period alone . . . exceeds $b+\beta$" [3, p. 457 (italics added)]. In the absence of explicit instructions to the contrary, the standard presumption is that the specification of β in both citations is identical. But, assuming that both statements are correct, this is clearly impossible. Thus suppose that demand price exceeded $b+\beta$ in only one of the subperiods in both the two-period and n-period cases and that during the remainder of the

Consider now the extension of our analysis to deal with indivisible plant. Replacing the assumption of fully divisible plant by one in which indivisibilities are present is made easy by our construction of an effective demand-for-capacity curve. Using this device, our analysis in Section II applies directly. Thus the criterion for shifting from Q_1^* to Q_1^*+E is identical to that shown in Figure 1: whenever the triangular area defined by Q_1^*, the effective demand-for-capacity curve, and the *LRMC* exceeds the area within Q_1^*+E, the D_E curve, and *LRMC*, the additional efficiency unit should be installed. The conditions under which one would be just indifferent between adding the capacity or not are shown in Figure 3.

Again we assume that the off-peak load prevails for 1/3 of the cycle and the peak load for 2/3, D_E being the weighted sum of $D_1^{(1/3)}$ and $D_2^{(2/3)}$. By construction, $IJN=JKL$, so that the benefits of installing the additional efficiency unit are just offset by the costs. In terms of the individual demand curves, the following relation holds: $2/3(NFG) - 2/3(GKH) - 1/3(MNKO) = 0$, where $2/3(NFG-GKH)$ is the amount by which the increase in consumers' surplus that is realized by adding the new capacity exceeds the loss in producers' surplus during peak-load operations (assuming that demand is supplied at *SRMC* prices both before and after), and $1/3$ *MNKO* is the amount by which the additional consumers' surplus falls short of the loss in producers' surplus during off-peak operations.

All of the implications that were developed in Section II concerning the effects of indivisibilities, including those that were obtained in connection with variable demand, likewise apply to the analysis of periodic demand. One additional comment, however, might be useful.

It will be noted that peak-load price in Figure 3 after installation of the new efficiency unit is less than *LRMC*. This appears to contradict our proposition C(a) given earlier. Actually, however, there is no inconsistency; the circumstances are different between Figures 2 and 3—the difference of course is that fully divisible plant is no longer assumed, whereas it was previously. Optimally adjusted plant under the assumptions of constant returns to scale and complete divisibility leads to zero

cycle capacity is never reached, so that service is supplied at the short-run marginal cost price of b. Thus the only contribution to capacity costs in each case occurs during the one subperiod when price exceeds b. Letting capacity be Q^*, the net revenue over operating costs is βQ^* (1/2) for the two-period case and βQ^* $(1/n)$ for the n-period case, assuming that each of the n periodic loads is of equal length. Obviously if capacity costs are covered for the two-period problem they can not be for the n-period case. Hence the presumption that β is the same in both-the two-period and n-period cases must be replaced by one in which, if β_2 is the capacity cost in the two-period case and β_n in the n-period case, $\beta_n = n/2\,\beta_2$. Given this respecification, Hirshleifer's optimality rules are correct, but not otherwise. Since both he and Steiner fail to indicate that such a respecification is necessary, the possibility that their prescriptions would lead to error can hardly be dismissed.

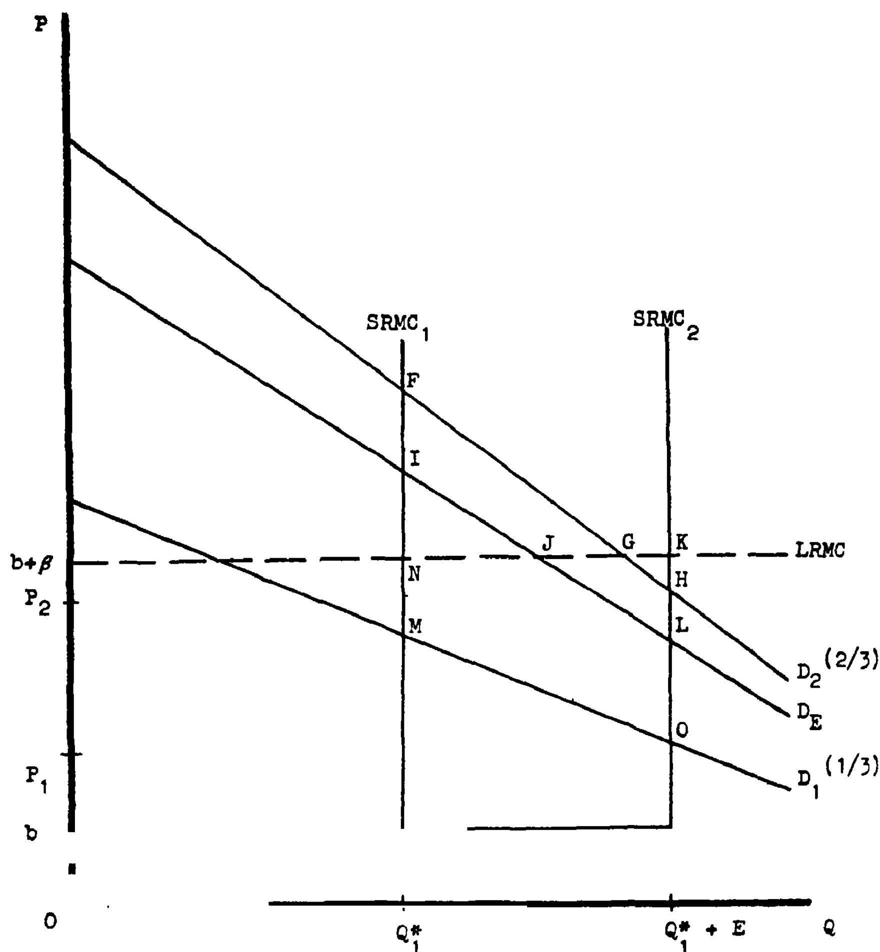


FIGURE 3

profits and, necessarily, peak-load price will exceed *LRMC*. The optimally adjusted plant when indivisibilities are present, however, will only accidentally lead to zero profits. Thus, if demand is $D_E+\epsilon$, the optimal plant size is Q_1^*+E, and although P_2 may exceed *LRMC*, it need not (and indeed does not in the circumstances shown in Figure 3).

IV. *Conclusions*

Although we have hardly disposed of all of the interesting questions that relate to the optimal pricing of periodic loads, we believe that the approach employed tends to clarify the issues in a useful way and readily lends itself both to specific application and to possible extensions. By

explicitly introducing a social welfare function at the outset we attempt to emphasize that the choice of optimal size plant and the pricing of services from plant of given capacity are not special problems at all but are merely particular applications of a perfectly general welfare formulation. Thus a good deal of the mystique which has tended to surround the analysis of peak-load pricing can be dispelled at the outset. This approach has the further advantage that we can interpret in welfare terms each of our results as they are derived and thereby provide a more convincing rationale for the results than merely arguing that they come from "straightforward maximization techniques."

That the analysis of periodic loads differs only in degree and not in kind from the more commonly investigated uniform-load problem is made particularly evident by our use of the "effective demand for capacity curve" device. By carefully defining our cost and demand relationships in advance, any periodic-load problem can be reduced to a demand-for-capacity analysis for purposes of selecting optimal plant size, and this holds whether we deal with completely divisible or indivisible plant. The optimal capacity criterion is identically the same in each case. Pricing to periodic loads and to uniform loads similarly follow the same pricing criterion: equate short-run marginal cost to demand price. Each periodic load must be priced separately, however; our weighted average demand curve is relevant only for long-run optimality (plant size) adjustments.

Possible extensions of our analysis would presumably include relaxing the assumption that demands in each period are independent. It is unlikely, however, that a useful geometry can be devised to deal with this more general case. Likewise it may be desirable to qualify the proposition that continuously equating short-run marginal cost to demand price is optimal. As Boiteux has pointed out, it may be useful for customer planning purposes to follow a more stable pricing policy [1, pp. 70–72]. However, Boiteux's proposal that "Whatever the capacity of existing plant, *the need to keep prices steady* generally leads prices to be fixed as if the plant were of optimum size" [1, p. 72] itself appears to be suboptimal. Rather, optimality requires that the benefits of pricing according to short-run marginal cost (the pricing rule that obtains under our welfare formulation) be weighed against the benefits of Boiteux's stable price proposal and an appropriate balance struck. For this purpose, a more broadly conceived social welfare function that makes explicit the gains to be secured through price stability is required. The trade-off between the short-run benefits of price flexibility and the long-run benefits of stability can then, presumably, be optimally arranged.

Generalizing the analysis to deal with nonlinear costs might also seem useful. This is easily accomplished analytically, but our geometry fails to

apply in this instance. Under the assumption of long-run constant returns to scale, so that the $LRMC$ curve remains horizontal and only the short-run cost curves display curvature, the nonlinear case yields results qualitatively similar to those obtained above.[12] Since the analysis of unequal-length periodic loads with nonlinear costs is somewhat more involved, does not lend itself to treatment geometrically, and fails to produce insights that differ appreciably from those obtained from the linear model, there are obvious advantages, for purposes of explicating the peak-load problem, in using the simpler linear form.

The relevance of the analysis to problems other than electricity generation might also be indicated. The optimality principles derived above regarding capacity and pricing of electricity apply to any public service which our welfare formulation and cost and demand specifications properly embrace. This would presumably include the investment in and operation of transportation facilities (airports, public surface transportation, bridges, etc.), recreation facilities, natural gas transmission, etc. Since the bridge problem is one of such long-standing interest, yet remains the source of continuing confusion, an application of the analysis to this question might be particularly useful. The major cause for misunderstanding here is the failure to distinguish between circumstances where capacity is a parameter from those in which it is a variable. Optimal pricing for a facility of *specified* capacity is given by the rules shown in equation (7) and footnote 10. As is obvious from inspection of these relations, if (for whatever reason) an oversized bridge is "inherited," so that the value of λ_i in these expressions is zero or negligible, and if operating costs are also so small that they can be disregarded, a zero use charge will be optimal. If, however, capacity is a variable rather than a

[12] The nonlinear cost problem can be formulated as: maximize,

$$W = \sum_{i=1}^{n} (TR_i + S_i)w_i - \sum_{i=1}^{n} (TVC_i)w_i - \beta Q^*.$$

Differentiating partially with respect to Q_i, we obtain n equations of the form

$$P_i = SRMC\big|_{Q_i},$$

and differentiating with respect to Q^*, we have

$$\beta = -\sum_{i=1}^{n} \frac{\partial}{\partial Q^*} (TVC_i)w_i.$$

Thus price in every period is set equal to short-run marginal cost, and capacity is extended until the weighted average marginal change in operating costs is equal to the marginal capacity cost, β.

It should be noted that Hirshleifer solves geometrically the case of nonlinear costs with constant returns to scale for the problem of identical-length-subperiod load by employing Steiner's vertical summation procedure [3, pp. 452–57). We also point out that, as anyone familiar with the Hirshleifer-Steiner dispute over the interpretation of marginal costs as they apply to the economics of peak-load pricing will recognize, we agree with Hirshleifer that the solution does not involve discriminatory prices.

parameter so that facility size can be adjusted (and indivisibility constraints aside), optimal pricing and optimal plant size are given by the relations shown in Figure 2 and equations (10)–(12). Even with zero operating costs, a positive use charge will be required here since, with capacity correctly selected, a positive price will be needed to ration capacity between those activities for which marginal benefits are high and those for which it is low. Assuming constant returns to scale, optimal pricing to this fully adjusted facility will yield precisely zero net revenues, and the bridge in these circumstances will be self-supporting. This latter, however, has no special normative significance. If there are indivisibilities, if increasing or decreasing returns to scale exist, or if capacity is given rather than subject to determination, optimal pricing will yield the zero net revenue result only accidentally if at all.

References

1. Marcel Boiteux, "La tarification des demandes en point: application de la théorie de la vente au coût marginal," *Revue Générale de l'Electricité*, Aug. 1949, *58*, 321–40; translated as "Peak-Load Pricing," *Jour. Business*, April 1960, *33*, 157–79; reprinted in James R. Nelson, ed., *Marginal Cost Pricing in Practice*, Englewood Cliffs, N.J. 1964, pp. 59–90.
2. A. Harberger and N. Andreatta, "A Note on the Economic Principles of Electricity Pricing," *Applied Econ. Papers*, March 1963, *3*, 37–54.
3. Jack Hirshleifer, "Peak Loads and Efficient Pricing: Comment," *Quart. Jour. Econ.*, Aug. 1958, *72*, 451–62.
4. H. S. Houthakker, "Electricity Tariffs in Theory and Practice," *Econ. Jour.*, March 1951, *61*, 1–25.
5. H. W. Kuhn and A. W. Tucker, "Nonlinear Programming," in J. Neyman, ed., *Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability*, Berkeley, Calif. 1951, pp. 481–92.
6. Alfred Marshall, *Principles of Economics*, 8th ed. New York 1948.
7. P. O. Steiner, "Peak Loads and Efficient Pricing," *Quart. Jour. Econ.*, Nov. 1957, *71*, 585–610.
8. Ralph Turvey, "Optimization and Suboptimization in Fishery Regulation," *Am. Econ. Rev.*, March 1964, *54*, 64–76.
9. D. M. Winch, "Consumer's Surplus and the Compensation Principle" *Am. Econ. Rev.*, June 1965, *55*, 395–423.

MULTIPERIOD FUTURE CONSUMPTION AS AN AGGREGATE

By NISSAN LIVIATAN*

The two-dimensional analysis of the optimal allocation of the consumer's wealth between present and future consumption in a two-period model is presented in most modern textbooks by means of the well-known diagram of Irving Fisher. However, when Fisher considered the possibility of extending his diagram to a multiperiod analysis he commented: "The truth seems to be that no complete visualization of this difficult problem is possible. The only complete symbolization which seems to be possible is in terms of mathematical formulas" [3, p. 287].[1]

While in general Fisher is right with respect to the *complete* visualization, the question arises as to how far one can go by using a simple two-dimensional diagram. After all we are often not interested in the determination of consumption in *every* future period but rather in the determination of *current* consumption and *current* capital accumulation. The purpose of this note is accordingly to analyze the conditions under which it is possible to break down the complicated problem of multiperiod planning into a problem of allocating consumer resources between present and future consumption where the latter is represented by a single quantity. When this is possible we may analyze the relevant problems by the ordinary two-dimensional indifference-map technique.

More specifically, the multiperiod consumer is supposed to maximize a utility function $U(C_0, C_1, \cdots C_n)$, where C_t denotes planned consumption for period t, subject to his wealth constraint.[2] The optimal values of all the C_t's obviously cannot be determined by a two-dimensional analysis. We may pose however the following question: under what conditions is it possible to represent the stream of future consumption $C_1, \cdots, C_n$ by a single "good" (say X) and to form a utility function [say $V(C_0, X)$] which can be maximized subject to a two-dimensional wealth constraint (involving only C_0 and X as endogenous variables) so as to yield the correct optimal value of C_0 (and, indirectly, of current capital accumulation). By "correct" I mean of course that value of C_0 which would result from maximizing the completely specified utility function $U(C_0, C_1, \cdots, C_n)$ subject to the relevant ($n+1$ dimensional) wealth constraint. A basic problem which arises in this connec-

* The author is senior lecturer in economics at the Hebrew University, Jerusalem.

[1] Cited by F. M. Westfield in [10, p. 1037].

[2] C_t is assumed to be, physically or economically, a single good which accounts for total consumption in period t.

tion is whether the two-dimensional system remains valid under changes in various exogenous parameters.

In our analysis special consideration will be given to two recently suggested measures which can essentially be interpreted as representing multiperiod future consumption by a single variable. One measure is that of Leontief [7, p. 105], who represents the "future" by the perpetual (equal) stream of future consumption, and the other measure is that of Dewey [1, p. 134], who represents it by the wealth which is planned for the next period. Although these measures have an intuitive appeal, it is by no means clear that we may use them under all circumstances. Indeed we shall show that under fairly common variations in external conditions these measures become economically meaningless. At the same time we shall point out some important cases where the foregoing measures *are* meaningful. In particular we shall show that it is possible to provide a rigorous theoretical justification for aggregation over future consumption in the models considered by Leontief and Dewey.

I. *Exogenous Interest Rates*

Consider an individual consumer in a market economy who has a given amount of wealth W which he plans to consume over $n+1$ periods. As before, denote the planned values of consumption by $C_0, C_1, \cdots C_n$. If necessary, C_n may be thought of as the terminal stock of wealth which the consumer plans to bequeath or to carry over beyond the planning horizon. It is assumed that the consumer is faced with fixed (exogenous) interest rates between any two consecutive periods, denoting the interest rate between period $t-1$ and t by r_t. We may then easily compute the "price" (or transformation rate), say P_t, of consumption in period t, in terms of current consumption C_0. Clearly we have:

$$P_0 = 1, P_1 = \frac{1}{1 + r_1}, P_2 = \frac{1}{(1 + r_1)} \frac{1}{(1 + r_2)}, \cdots, \tag{1}$$

$$P_n = \frac{1}{(1 + r_1)} \frac{1}{(1 + r_2)} \cdots \frac{1}{(1 + r_n)}.$$

(In the special case where all r_t's are equal (1) reduces to $P_t = 1/(1+r)^t$). Using this notation, the consumer's budget constraint is given by:

$$W = P_0C_0 + P_1C_1 + \cdots + P_nC_n. \tag{2}$$

The conditions under which we may aggregate over $C_1, \cdots, C_n$ are of two kinds: those which restrict the variation of the interest rates and those which restrict the form of the utility function. We begin with the former. Suppose that we consider situations where the r_t's, and hence also the P_t's, remain constant throughout the analysis. We shall indicate

these constant values by adding a zero superscript to r_t and P_t, writing r_t^0 and P_t^0. In this case we may apply Hicks's composite-good theorem [6, pp. 33–34 and 312–13] which states that a group of commodities with fixed relative prices can be treated as an ordinary individual commodity. We may then define

$$F = \sum_{t=1}^{n} P_t^0 C_t$$

as our composite good (the price of which is unity) and draw the usual type of indifference curves in the (C_0, F) plane, corresponding to a utility function $V(C_0, F)$, and ordinary budget lines, corresponding to $W = C_0 + F$. By Hicks's theorem this "reduced system" can be used to determine the correct optimal values of C_0 and of capital accumulation[3] $r_1^0 W - (1+r_1)C_0$. It is clear that the foregoing reduced system can be used to analyze the *income* effect[4] of a change in W on C_0. This is in fact the main theoretical use of the reduced system since, as we shall see, it cannot generally be used to study the effects of a change in the rate of interest.

It may be noted that

$$F = \sum_{1}^{n} P_t^0 C_t$$

is the "present value of future consumption"[5] which is the same thing as the "present value of planned wealth for the next period." Alternatively, we are free to define our composite good as

$$F^{(1)} = (1 + r_1^0)F = \frac{F}{P_1^0}$$

i.e., as "planned wealth for the next period" (valued as of that period) which coincides with Dewey's measure. The latter procedure is legitimate since we are allowed to multiply a Hicksian composite good by a constant $(1+r_1^0)$ provided, of course, we adjust its price accordingly.

[3] This is defined as the difference between W in the next period and in the present period, when consumption takes place in the *beginning* of any period. The value of consumption which leads to a zero accumulation, i.e., $r_1^0/(1+r_1^0)\, W$, can be properly called "permanent income."

[4] In some cases this is our primary interest. For example in the presentation of his consumption theory Friedman's main concern is to show how C_0 varies as a result of a change in the consumer's wealth. (See Friedman [4].) In view of our analysis there is no need in such cases to restrict oneself, as Friedman does, to a two-period graphical analysis [4, pp. 8–10].

[5] Note that

$$\sum_{1}^{n} P_t^0$$

is the present value of an annuity of one dollar for n periods, beginning with the next period.

Thus if we work with $F^{(1)}$ its "price" will be $1/(1+r^0)$, and the budget line will be

$$W = C_0 + \frac{1}{1 + r_1^0} F^{(1)}$$

When all r_t's are equal we may, by the foregoing reasoning, define a composite good $F^{(2)} = rF [= r(W - C_0)]$, which is the Leontief measure of the potential[6] constant stream of future consumption which can be maintained indefinitely starting from the next period. The budget line is then given by $W = C_0 + (1/r)F^{(2)}$. Thus Dewey's and Leontief's measures are legitimate for the present case.

Suppose now we wish to analyze the effect of a change in interest rates on C_0 or on capital accumulation within the framework of a reduced system including only C_0 and some composite good to represent future consumption. Suppose first that all r_t's are equal, so that $P_t = 1/(1+r)^t$. When r changes, every P_t will change in a different proportion. Alternatively, the relative price, say $P_{t-1}/P_t = 1+r$, must change. Since the relative prices of the C_t's $(t=1, \cdots, n)$ must change, the foregoing composite goods $(F, F^{(1)}, F^{(2)})$ become economically meaningless. A two-dimensional analysis must therefore be ruled out.

Let us return to the case where the r_t's may differ for various periods. Is it then possible to vary the r_t's in such a way as to leave the ratios of the P_t's $(t=1, \cdots, n)$ constant? When this is possible, we may still have a true composite good to represent the future, under appropriate variations in the r_t's. At first glance it may seem that there are many ways of varying the r_t's so as to leave the ratios of the P_t's $(t=1, \cdots, n)$ unchanged. However a closer examination will reveal that there is only a single way of accomplishing this, namely by changing only r_1, leaving all other r_t's $(t=2, \cdots, n)$ unchanged. This follows from the special form in which the P_t's are related to the r_t's by means of equation (1). Suppose, e.g., that we change r_2. Then $P_1/P_2 = 1+r_2$ changes, so that relative prices are no longer constant. This will remain true for a change in any r_t, or in a combination of r_t's, when $t=2, \cdots, n$. If, however, we change only r_1, then all the relative prices for any pair of P_t's $(t=1, 2, \cdots, n)$ will remain unaffected since $1/(1+r_1)$ is a common factor of all these P_t's

$$\left(P_t \text{ being given by } \frac{1}{1+r_1} \frac{1}{1+r_2} \cdots \frac{1}{1+r_t}\right).$$

[6] Note that this measure has nothing to do with the planned optimal *distribution* of consumption in future periods. Note also that although this measure refers to an infinite stream there is no need at all to assume that the actual planning horizon for consumption (i.e. the one on which F is based) is infinite too.

We may consider the foregoing case as one where the elasticity of expectations with regard to a change in r_1 is zero (since it does not affect the values of the future r_t's). For this case we may define our composite good as

$$F^* = \sum_{1}^{n} P_t^0 C_t,$$

where

$$P_t^0 = \frac{1}{1 + r_1^0} \frac{1}{1 + r_2^0} \cdots \frac{1}{1 + r_t^0}$$

and where the zero superscripts denote the initial set of parameters (r_t's and P_t's). This is equivalent to the valuation of a Hicksian composite good in terms of constant "base point" price. Since we permit changes in r_1 only, the "current" (variable) prices become

$$P_t = \frac{1}{1 + r_1} \frac{1}{1 + r_2^0} \cdots \frac{1}{1 + r_t^0}.$$

The price (P) of the composite good F^* is then

$$P = \frac{P_t}{P_t^0} = \frac{1 + r_1^0}{1 + r_1} \qquad (t = 1, \cdots, n),$$

which satisfies the "value identity"

$$PF^* = \sum_{1}^{n} P_t C_t$$

(noting that $P_t = PP_t^0$). The budget line is then given by $W = C_0 + PF^*$ and we may carry out the usual two-dimensional analysis with C_0 and F^* as our commodities. In this particular case we may analyze in two dimensions not only the effect of a change in W, but also a change in the rate of interest provided the latter is confined to a change in r_1 only.

This is as far as we can go when our restrictions relate to interest rates only. We may however consider restriction on the utility function itself. In particular suppose that the utility function $U(C_0, C_1, \cdots C_n)$ can be written as a "separable" function $U[C_0, H(C_1, \cdots, C_n)]$ where in turn $H(\ )$ is a linear homogeneous function in $C_1, \cdots, C_n$. It may be noted that the foregoing separability and homogeneity assumptions are perhaps not as unrealistic when applied to a utility function which deals with *aggregate* consumption in various periods in contrast with one which deals with *individual* categories of consumption (food, clothing, etc.). In this connection we may also note that the assumption that the

utility function is homogeneous in the C_t's (including C_0) is the basis of some well-known modern consumption theories.[7] Now, it has been shown [5, Ch. 4] that when the utility function is separable and homogeneous in the foregoing manner, then there exists an ideal price deflator[8] $\overline{P}(P_1, \cdots, P_n)$ for "money outlay"

$$\sum_1^n P_t C_t,$$

so that we may form a reduced utility function $Q(C_0, D)$ where

$$D = \frac{\sum_1^n P_t C_t}{\overline{P}(P_1, \cdots, P_n)} \equiv H(C_1, \cdots, C_n).$$

If we work with $Q(C_0, D)$ and with budget lines $W = C_0 + \overline{P}D$, then we shall be able to determine the correct optimal values of C_0 and D (which implies that we can determine correctly the optimal value of

$$\sum_1^n P_t C_t$$

and hence also of capital accumulation).

We must stress, however, that aggregation under the foregoing restrictions on the utility function is of very limited use. The difficulty stems from the fact that the slope coefficient of the budget line $\overline{P}(\ )$ depends not only on the P_t's (or r_t's) but also on the specific form of the function $H(C_1, \cdots, C_n)$ above (as can be seen from the example given in footnote 8). In other words, the slope of the budget line at some set of interest rates depends on the consumer's tastes. Consequently we cannot know in general what will be the effect of a change in r_t on $\overline{P}$ without knowing the consumer's tastes. This rules out effectively the possibility

[7] Namely Friedman's "Permanent Income Hypothesis" and Modigliani's "Life Cycle Hypothesis." See M. Friedman [4] and F. Modigliani and R. Brumberg [8].

[8] $\overline{P}(P_1, \cdots, P_n)$ is a linear homogeneous function in the P_t's. If, e.g., $H(C_1, \cdots, C_n)$ is of the form

$$C_1^{a_1} C_2^{a_2} \cdots C_n^{a_n},$$

then $\overline{P}(\ )$ will be of the form

$$\overline{P} = \left(\sum_1^n a_t\right) \left(\frac{P_1}{a_1}\right)^{\frac{a_1}{\sum a_t}} \left(\frac{P_2}{a_2}\right)^{\frac{a_2}{\sum a_t}} \cdots \left(\frac{P_n}{a_n}\right)^{\frac{a_n}{\sum a_t}}$$

where the summation is always from 1 to n. This example has been taken from an article by R. Solow on a different problem which can however be related to the present one. See R. Solow [9, p. 105].

of analyzing the effect of a change in r_t on C_0 and on capital accumulation. The only changes we can analyze in this case are changes in wealth (where the slope of the budget line remains unchanged). But as we have seen, in order to analyze changes in wealth we do not need to impose *any* restrictions on the utility function. Thus, while it is true that with a separable and homogeneous utility function we have perfect quantity and price indexes for aggregative future consumption, these indexes (and their underlying assumptions) are not required for the analysis of wealth effects, and are not sufficient for the general analysis of changes in the r_t's.

II. *The "Robinson Crusoe Economy"*

There exists a tendency to apply some version of Fisher's apparatus to deal with the economy as a whole on the assumption that we are permitted to use community indifference curves and some simple type of an aggregative production function. Thus, for example, both Leontief and Dewey were concerned with a model of the foregoing kind. Clearly, this set of assumptions amounts to considering a one-man ("Robinson Crusoe") economy. Let us now examine the possibility of aggregation over future consumption $(C_1, \cdots, C_n)$ within the framework of such an economy. First let us formulate the model more accurately.

On the consumption side we consider an individual ("Robinson Crusoe") whose preferences for alternative collections $C_0, C_1, \cdots C_n$ are ordered by a utility function $U(C_0, C_1, \cdots, C_n)$. Turning to the production side we assume that there exists a homogeneous good which can be used both as a present consumption good and as an input to produce next period's consumption. The quantity of this good at the beginning of any period (t) will be denoted by K_t and referred to as "capital in period t." We assume a simple Ramsey-type production function:[9]

$$K_{t+1} = f(K_t - C_t); \qquad t = 0, 1, \cdots, n-1, \tag{3}$$

which shows how the unconsumed part of capital at any period can be used to produce the (net)initial capital stock of the next period. Ordinarily we assume $f'(\ ) > 0$, $f''(\ ) < 0$, i.e., the marginal product of $K_t - C_t$ is positive and decreasing. It may be assumed, without affecting the argument, that there is a constant amount of labor input (for all t) cooperating with capital. It will be convenient to identify the terminal capital stock K_n with C_n since it is either consumed or "bequeathed." In any case K_n represents a "final use" within the framework of this model and has therefore to enter the utility function as do the other C_t's.

We may now derive the transformation function between the C_t's as

[9] This type of production function is implicit in Dewey's paper and is used often elsewhere. See, e.g., R. Dorfman and others [2, pp. 271-76].

follows. Write (3) in full as

(4) $K_1 = f(K_0 - C_0);\ K_2 = f(K_1 - C_1);\ \cdots;\ C_n = f(K_{n-1} - C_{n-1}).$

Substitute successively (working from left to right) the right-hand side of each equation in (4) into the following one. This will eliminate all K_t's except for K_0 and will lead to a transformation function

$$T(C_1, \cdots, C_n; K_0) = 0 \tag{5}$$

with K_0 as a parameter. This function shows all possible collections which can be produced starting with a given value of initial capital. If, for example, we set $n=3$ we have

$$T(C_0, C_1, C_2, C_3; K_0) \equiv C_3 - f\{f[f(K_0 - C_0) - C_1] - C_2\} = 0. \tag{6}$$

The difference between the present model and the one in Section I is that now the marginal rates of transformation (MRT) between the C_t's are no longer constant, but rather vary with their quantities. The MRT between C_0 and C_t can be calculated as follows. Given the initial K_0 and some specified set $C_0, C_1, \cdots, C_n$, we can determine via (4) the values of all $K_t (t=1, \cdots, n-1)$. It follows that all the arguments of the functions in (4) are determined. Suppose now we wish to calculate the MRT between C_0 and C_1. If the consumer gives up one unit of C_0 he may increase C_1 (holding all other C_t's constant) by the marginal product $f'(K_0-C_0)$. The negative of the latter is therefore the required MRT. Similarly a sacrifice of one unit of C_0 can increase C_2 (holding $C_1, C_3, \cdots, C_n$ constant) by $f'(F_0-C_0)f'(K_1-C_1)$, which is the MRT (in numerical value) between C_0 and C_2. In general,[10] the MRT between C_q and C_0 is given by the product

$$-f'(K_0 - C_0)f'(K_1 - C_1) \cdots f'(K_{q-1} - C_{q-1}).$$

[10] Alternatively, differentiate the system (4) w.r.t. C_0 and C_q holding all other C_t's (and K_0) constant. If $q<n$, then for $t=n$ this leads to

$$dC_n = f'(K_{n-1} - C_{n-1})dK_{n-1} = 0,$$

since $dC_n=0$ by assumption. However, since $f'>0$, this implies $dK_{n-1}=0$. Using this result and applying similar reasoning we find that $dK_t=0$ for $t=q+1, \cdots, n-1$. However $dK_{q+1} = f'(K_q-C_q)$. $(dK_q-dC_q)=0$ implies $dK_q=dC_q\neq 0$. Hence

$$dC_q = dK_q = f'(K_{q-1} - C_{q-1})dK_{q-1} \neq 0.$$

Similarly

$$dK_t = f'(K_{t-1} - C_{t-1})dK_{t-1} \neq 0$$

for $t=2, \cdots, q-1$. Finally,

$$dK_1 = -f'(K_0 - C_0)dC_0.$$

By successive substitution we eliminate the dK_t's and obtain (for $q=1, \cdots, n$):

$$\frac{dC_q}{dC_0} = -f'(K_{q-1} - C_{q-1})f'(K_{q-2} - C_{q-2}) \cdots f'(K_0 - C_0) = -\frac{\partial T(\ )}{\partial C_0} \div \frac{\partial T(\ )}{\partial C_q}.$$

The reciprocal of this product (in absolute value) is the "price" in terms of C_0 of producing an additional unit of $C_t(t=q)$. The latter "price" is in a sense the counterpart of the price P_t which we considered in Section I. Similarly $f'(K_t-C_t)-1$ is the counterpart of r_{t+1}.

The problem which we must investigate now is whether it is possible in the present model to determine the optimal values of C_0 and of capital accumulation by means of a two-dimensional utility analysis, where one variable is C_0 and the other variable is an index of future consumption possibilities. We shall now show that the answer to this question is positive. We shall also show how the reduced utility function is to be constructed from the original (complete) one.

Consider first the case $n=3$ described by (6). Note here that C_0 and K_0 enter $T(C_0, C_1, C_2, C_3; K_0)=0$ by means of the expression $f(K_0-C_0)$. However, since the latter equals K_1, we may write (6) as

$$C_3 - f[f(K_1 - C_1) - C_2] \equiv T^*(K_1, C_1, C_2, C_3) = 0$$

which defines a transformation function between K_1, C_1, C_2 and C_3. In general this function is $T^*(K_1, C_1, \cdots, C_n)=0$. Now the maximization problem when we work with the complete system is: maximize

$$U(C_0, C_1, \cdots, C_n) \quad \text{w.r.t.} \quad C_0, C_1, \cdots, C_n$$

subject to the production constraint $T(C_0, C_1, \cdots, C_n; K_0)=0$. Let us break this problem down into two stages. In the first stage maximize $U(C_0, C_1, \cdots, C_n)$ subject to $T^*(K_1, C_1, \cdots, C_n)=0$ for arbitrary values of C_0 and K_1. Now for given arbitrary values of C_0 and K_1 it is possible to determine an optimal set of $C_1, \cdots, C_n$ which (together with C_0) corresponds to a particular value of $U(\quad)$. This (conditionally) maximized value of U, to be denoted by Um, will of course vary for alternative sets of (C_0, K_1). Let us denote the dependence of Um on C_0 and K_1 by a function, say $Um=M(C_0, K_1)$.[11] Given any pair of collections (C_0, K_1), the consumer can determine directly by means of $M(C_0, K_1)$ which one he prefers. Hence $M(C_0, K_1)$ may serve the same analytic purposes as an ordinary utility function in the two-commodity case.

While the utility function is defined for arbitrary values of C_0 and K_1, it is clear that from the point of view of the production possibilities these variables are not independent. In fact, by (4), these variables are related by means of the transformation function $K_1-f(K_0-C_0)=0$. In order to determine the optimal values of C_0 and K_1, we must therefore go

[11] Alternatively, the optimal values of $C_1, \cdots, C_n$ which correspond to every set C_0, K_1 can be expressed by means of functions, say $C_t=g^{(t)}(C_0, K_1)$. Substituting these functions in U we obtain

$$U[C_0, g^{(1)}(C_0, K_1), \cdots, g^{(n)}(C_0, K_1)] = M(C_0, K_1).$$

through a second stage of maximization where we maximize $M(C_0, K_1)$ subject to $K_1 - f(K_0 - C_0) = 0$. We may now regard the second stage of maximization as the standard (two-period) problem of maximizing a utility function $M(C_0, K_1)$ subject to a production (or budget) constraint. This establishes the fact that in the present model K_1 can fully represent future consumption plans.

The equilibrium position is illustrated by the point E in Figure 1. The slope of the transformation curve at this point, i.e., $f'(K_0 - {}_0\overline{C})$, is the marginal rate of transformation between C_0 and C_1. This rate (minus 1) is also the relevant marginal rate of return on investment. The size of capital accumulation can be calculated by $\overline{K}_1 - K_0$. Finally note that by varying the initial amount of K_0 we can trace out the whole income consumption curve RR' from which we may derive the consumption function $C_0 = C(K_0)$. This is *the* demand function for C_0 in the present model since K_0 is the only exogenous factor.

We may note that K_1, which is "planned wealth for the next period," is the variable which Dewey uses in his two-dimensional analysis. It is easy to see that the foregoing analysis can be carried out alternatively in terms of Leontief's "perpetual consumption" measure. The constant value of future consumption, say C_f, can be obtained by solving the equation $K_1 = f(K_1 - C_f)$ for C_f. Thus if $C_1 = C_f$, then $K_2 = K_1$ so that C_f can be maintained forever. We now assume that $f'(K_1 - C_f) > 1$ for all values of K_1, i.e., at $K_1 - C_f$ the marginal *net* rate of return is positive (note that if there are no transfer costs between periods then f' can never fall below unity[12]). Under this assumption C_f is a monotonic increasing function of K_1, so that we may also express K_1 as a function of C_f, say $K_1 = g(C_f)$. Substituting $g(C_f)$ for K_1 in the utility function M we obtain $M[C_0, g(C_f)] \equiv M^*(C_0, C_f)$. Similarly substituting in the production function we obtain $g(C_f) - f(K_0 - C_0) = 0$. It can be verified by differentiating the latter transformation function w.r.t. C_0 and C_f, using the assumptions $f' > 1$ and $f'' < 0$, that it is of the usual type (i.e., negatively sloped[13] and concave). We now have a utility function and a production (or transformation) function in terms of C_0 and C_f so that we may carry out the two-dimensional utility analysis in the usual manner.

It may be noted that at alternative equilibrium situations (corresponding to alternative values of K_0) the MRT's between C_0 and C_1 will generally vary. It seems therefore somewhat surprising that in spite of this fact the two-dimensional analysis (as in Fig. 1) remains valid whereas the aggregation procedure in Section I failed when we varied

[12] This has been pointed out to me by D. Patinkin.

[13] More specifically

$$\frac{dC_f}{dC_0} = -\frac{f'(K_0 - C_0)}{f'(K_1 - C_f)}[f'(K_1 - C_f) - 1].$$

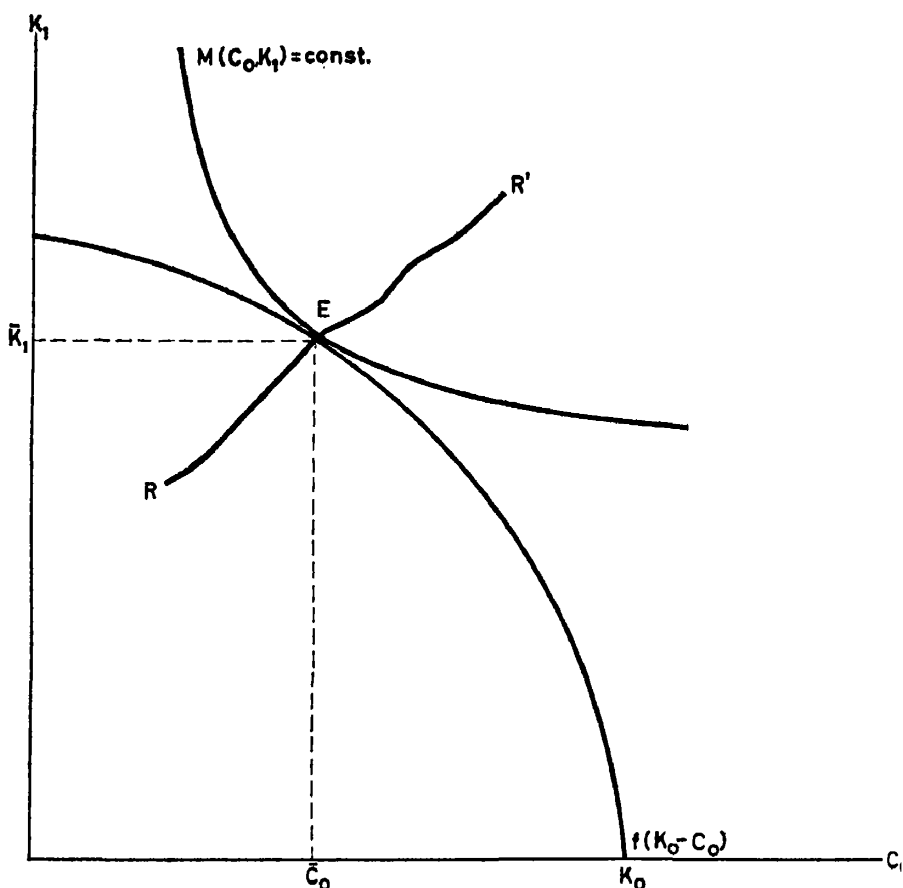


FIGURE 1

interest rates (and hence the transformation rates). The explanation is however simple and is related to the fact that in Section I the interest rates were *exogenous* and the corresponding P_t's were given parameters from the point of view of the consumer. Consequently when the P_t's changed in different proportions, we could argue by means of the Hicksian composite good theorem that the aggregation procedure is generally not valid. In the Robinson Crusoe model, however, the transformation rates are *endogenous* and cannot therefore be interpreted as exogenous prices. The only parameter which varies in the latter model is K_0, and this corresponds exactly to a change in initial wealth W, in Section I, with the r_t's remaining constant. It may be recalled that in this case we *could* define in Section I a true composite good to represent future consumption. The general result is therefore that, when we deal with "wealth effects" (of changes in W or K_0), we can always define a meaningful composite good of future multiperiod consumption.

One may introduce into the Robinson Crusoe economy something similar to *exogenous* interest rates by assuming some form of (expected) technical change. Suppose, e.g., that instead of (3) we have $K_{t+1}=bf(K_t-C_t)$ where $b(>1)$ is a parameter of technical change which is known to Mr. Crusoe. On the basis of this production function we may derive a utility function $M(C_0, K_1)$ as before. However as the (exogenous) b changes, the former indifference map will no longer be valid.

A similar statement can be made concerning the labor input which we have assumed to be constant. Suppose that in each period there is a certain quantity of exogenously given labor input. If this quantity varies among periods, then for each period we shall have a different production function, say $K_{t+1}=f^{(t)}(K_t-C_t)$. This does not prevent us from deriving a utility function $M(C_0, K_1)$ as before. However, the latter function will depend on the parameters of labor imput. For an alternative set of labor inputs the original $M(\quad)$ becomes meaningless.

III. *Conclusions*

For an individual consumer, who is faced with exogenous interest rates, it is possible to perform perfect aggregation over the stream of future consumption in order to carry out a two dimensional analysis of *current* consumption and of *current* capital accumulation under the following conditions: (a) When the interest rates are fixed throughout. In this case we can study the effect of a change in wealth on the foregoing variables. (b) When all interest rates are fixed except the one between the current period and the next one (r_1). In this case we may study, in addition to the effect of a change in wealth, the effect of a change in r_1. (c) When the utility function is restricted to the special form

$$U[C_0, H(C_1, \cdots, C_n)]$$

where $H(\quad)$ is a homogeneous function. It is then formally possible to describe the consumer's equilibrium in terms of C_0 and the value of H by means of a two-dimensional indifference-curves diagram. In this case, however, the two-dimensional analysis is of limited use since the slope of the budget line will depend not only on interest rates but also on the consumer's tastes, i.e. on the specific form of the funtcion H( ).

When we extend our analysis to deal with the economy as a whole the interest rates become endogenous variables and consequently our analysis must be confined to the effect of changes in initial capital. In particular for a Robinson Crusoe economy with a single homogeneous good and a simple Ramsey-type production function $K_{t+1}=f(K_t-C_t)$ it is possible to carry out a two-dimensional utility analysis of K_1 and C_0 for the purpose of studying the effects of a change in initial wealth [as in case (a) above].

REFERENCES

1. D. DEWEY, "The Geometry of Capital and Interest," *Am. Econ. Rev* March 1963, *53*, 134–39.
2. R. DORFMAN AND OTHERS, *Linear Programming and Economic Analysi* New York 1958.
3. I. FISHER, *The Theory of Interest*. New York 1930.
4. M. FRIEDMAN, *A Theory of the Consumption Function*. Princeton 195
5. J. GREEN, *Aggregation in Economic Analysis*. Princeton 1964.
6. J. R. HICKS, *Value and Capital*, 2nd ed. Oxford 1946.
7. W. W. LEONTIEF, "Theoretical Note on Time Preference, Productivit of Capital, Stagnation and Economic Growth," *Am. Econ. Rev.*, Marc 1958, *48*, 105–11.
8. F. MODIGLIANI AND R. BRUMBERG, "Utility Analysis and the Consump tion Function: An Interpretation of Cross-Section Data," in K. K Kurihara, ed., *Post-Keynesian Economics*, New Brunswick 1954.
9. R. SOLOW, "The Production Function and the Theory of Capital, *Rev. Econ. Stud.*, 1956, *23* (2), 101–8.
10. F. M. WESTFIELD, "Time Preference and Economic Growth: Com ment," *Am. Econ. Rev.*, December 1959, *49*, 1037–41.

COMMUNICATIONS

The Role of Capital-Goods Trade in the Theory of International Trade

In most treatises on the theory of international trade, the causes and nature of trade in capital goods receive little attention.[1] Typically, only when international movements of capital funds are introduced into the discussion is such trade even mentioned and then usually only in connection with the so-called "transfer" problem. Trade models that follow the traditional assumption of excluding international movements of capital funds invariably are set up entirely in static terms. A given stock of capital equipment is assumed to exist, and the possibility of any net saving is ruled out. Consequently, an exchange of currently produced consumption goods for part of a country's given stock of capital goods is not possible, since it would involve domestic saving by the nation importing the capital goods.[2]

This traditional approach is completely inadequate in these times, when economic growth is such an important goal, especially for the less developed nations. Capital-goods trade is a crucial part of the dynamic relationship between international trade and development. Part of the imports of investment goods into less developed countries is financed by international flows of capital funds, but a significant portion of this trade is financed by current foreign exchange earnings. International trade theory must cover both situations. The production of capital goods as well as consumer goods must be introduced into trade models, and the entire analysis integrated with domestic capital accumulation theory.

This paper will modify the static nature of existing trade theory by introducing explicitly the possibility of capital-goods trade that is financed from current export earnings. The factors that determine whether in fact at a particular time such trade will take place and, if so, how much and at what exchange ratio are analyzed as well as the elements affecting changes in the volume and terms of this trade as accumulation and growth proceed. Similarly,

[1] In his survey article on trade theory, J. Bhagwati comments upon the lack of analysis on this subject and also surveys what recent writings there are on the topic [1, pp. 51–53]. However, since Bhagwati's paper appeared (and also since this paper was written), H. Oniki and H. Uzawa have published an elegant mathematical analysis of trade in capital goods [9]. In contrast to the comparative statics approach here, Oniki and Uzawa frame their analysis in dynamic terms. The main difference in basic assumptions is that they postulate a constant average propensity to save throughout the accumulation process, whereas savings is made a function both of the interest rate and the level of income in the model used here.

[2] It is perhaps not out of place to remind ourselves that the traditional assumption of trade theory concerning the immobility of capital refers to abstract capital and not capital goods. See, for example [8, p. 77] [2, pp. 5-6] [3, p. 27].

Another method commonly followed to avoid the issue of capital-goods trade is to assume that labor and natural resources are the only productive factors. [7] and [4] are examples of writers using this approach.

the impact of capital-goods trade on the development process itself is analyzed. The case where capital funds as well as capital goods flow across international borders also is included and contrasted with the situation where only capital-goods trade occurs.

I

For explanatory purposes, it is useful first to consider the pattern of accumulation within a country in the absence of both trade in capital goods and international capital movements. Suppose there are two commodities, one of which is an infinitely durable capital good that, together with the other factor, labor, can be employed in producing either additional units of this capital good or the other consumption commodity. The actual quantity of the capital good produced in any time period is a function of the preferences of the population for the consumption good over their time horizons and the technological nature of the production functions for the two commodities.[3] In a world of perfect certainty in which future markets exist, a particular time pattern of investment would be selected by the community at the outset and followed during the entire accumulation process. However, if there is imperfect knowledge about the future, individuals will revise their accumulation plans as time passes and new information becomes available.

A simple model based upon the latter type of behavior, which was developed by Leontief [5] [6] [12], is represented in Figure 1. The indifference curves in the first quadrant, e.g., i and i', show the community's preferences between present and future consumption.[4] In selecting the current period's saving level, decision units in the economy are assumed to act as if they will not accumulate additional capital in any future periods. However, as each period passes, they revise these plans and undertake further saving until the marginal productivity of capital falls to the level at which it equals the community's marginal time preference rate between present and future consumption at the current period's income level.

The fourth quadrant indicates the current period's production-possibility curve between the consumption commodity and the capital good. This curve shifts outward over time as the productivity of the given labor force is raised by means of capital accumulation. The third quadrant shows the increase in potential consumption associated with the various quantities of the capital good that can be produced in the current period. This consumption-output curve for various sizes of the capital stock (and the full utilization of the fixed

[3] In the model and throughout this paper, production functions are assumed to be homogeneous of the first degree and to be identical for all trading partners. Factor-intensity reversals are also ruled out. In addition, it is assumed that there are no qualitative differences between the labor forces of different countries.

[4] Since this paper is not concerned with the welfare aspects of international trade, it is useful for expositional purposes to assume that each individual in a country is identical with respect to tastes and factor endowment. For simplicity, the community's preference for present versus future consumption also is taken to be such that at a given interest rate the fraction of income saved remains constant as income increases. However, the higher the interest rate, the higher this fraction becomes.

labor supply) is drawn such that in each period it begins on the x-axis at a point indicating the maximum amount of the consumption good that the economy can produce in the period. For example, initially it is supposed that OA (equals OG) of the consumption good can be produced if all available capital and labor are used for this purpose. If, however, AB of current potential consumption is foregone in order to produce BC of the capital good, potential consumption in future periods will increase by GI. To represent this possibility in the first quadrant, as well as all the other possibilities between present and future consumption (the curve RTL), a 45° line is drawn in the second quadrant from I to J. Thus if the economy saves RS (equals AB) of current income and accumulates BC (equals IH) of the capital good, it will be left with OB of the consumption good in the current period, but can produce BT(equals OI and OJ) of this commodity in each future period.

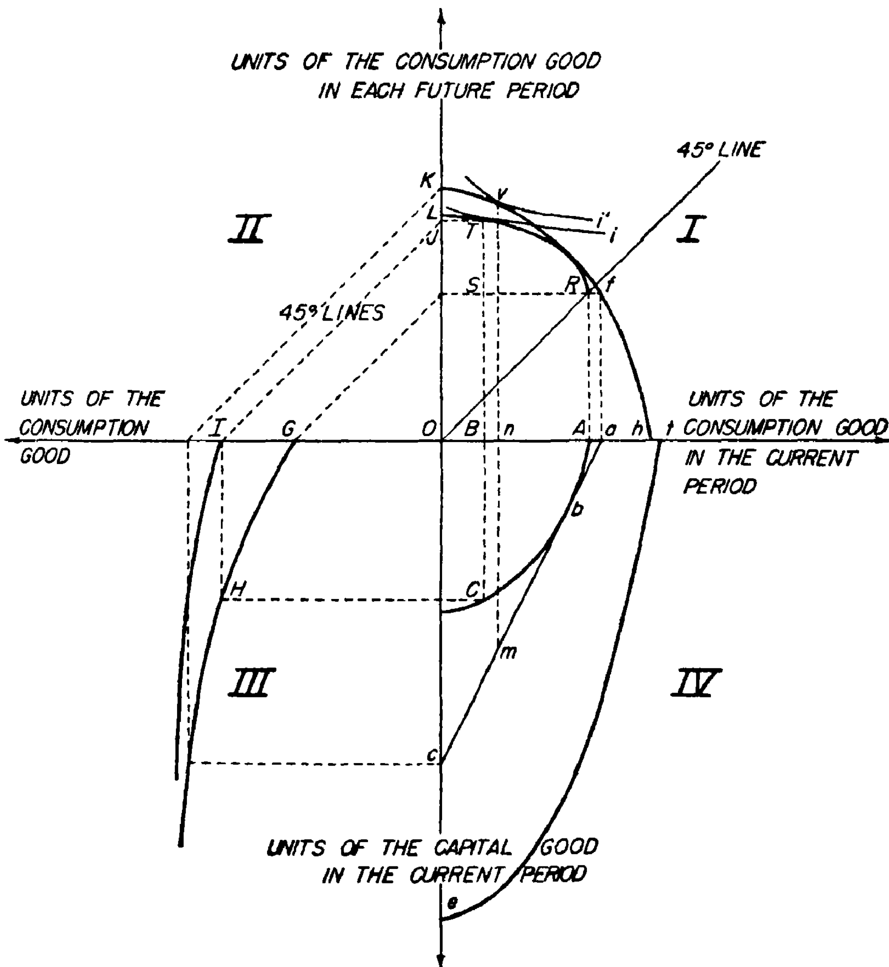


FIGURE 1

Since an indifference curve between present and future income is assumed to be tangent to the consumption-possibility curve between present and future periods at the point T, the community will save RS in the initial period.

On the basis of the capital accumulated in the first period, the production-possibility curve shifts outward to te. The capital-output curve beyond the point H is also redrawn beginning at OI (equals Ot) in order to depict the output possibilities associated with further accumulation. From these two curves a new consumption-possibility curve could be drawn in the first quadrant, and a new saving point determined. The consumption-possibility curves will eventually become flatter over time as the marginal productivity of capital declines. When an indifference curve is tangent to a consumption-possibility curve at a point on the 45° line, a stationary-state position is attained.[5]

II

Consider now the effects on this accumulation process if international trade in the two commodities is opened up. Should the country be able to trade all of its potential output of either good (as well as the stock of capital goods already produced) at an exchange ratio indicated by the slope of the line abc, its consumption-possibility curve would shift outwards to $hfvK$.[6] Compared to the former consumption curve (RTL), this new consumption-possibility curve permits the community to select a point at which both current and future consumption will be greater than previously. Assuming the point v to be the optimum point, the economy accumulates nm of capital in the period, partly by domestic capital-good production and partly by exporting the consumption commodity for additional amounts of the capital good.

For each hypothetical exchange ratio there will be a new consumption-possibility curve, a particular optimum volume of capital accumulation, and an international offer of one commodity for the other. The actual exchange ratio and trade that will emerge in the period depends upon the shape of the offer curves of all the trading nations and is set at the point where total exports equal total imports for each of the two items.

Unless countervailing saving preferences offset the production biases, a relatively capital-scarce nation will import the capital good, which is assumed to be uniformly capital-intensive compared to the consumption good, and export the consumption commodity. The effect of this trade will be to accelerate capital formation in countries importing the capital good, compared to the growth that will take place from a given initial factor endowment level in the non-trading case. However, the accumulation rate may or may not increase for the capital-good-exporting nation.[7]

[5] One could introduce given rates of population growth and technological progress so that the system approached some positive growth rate rather than a stationary position.

[6] The fh portion of the curve is based upon the exchange of the existing capital stock for the consumption good at the given exchange ratio.

[7] This point can be shown in Figure 1. Since the price of capital goods falls for this capital-good-importing country after trade is opened up, the new consumption-possibility

As explained in the no-trade case, capital formation pushes out a country's production-possibility curve. This output expansion will be accompanied by a gradual decline in the relative price of the capital good.[8] The interest rate will also decline as the marginal productivity of capital falls. Moreover, since the decision units in each economy revise their investment plans each period as their incomes rise and in fact continue to save as long as the expected marginal productivity of capital is greater than their marginal time-preference rate between present and future income, actual interest rates (as distinct from expected rates) will be equal among those trading nations who produce some of each commodity.[9] The interest rate will finally decline to a level where no further saving takes place.[10] If tastes are the same among the trading nations, a group equilibrium position will be reached with each nation possessing the same capital/labor ratio.[11] Trade in the two commodities will cease at this point.[12]

Shifts in the terms of trade for countries either exporting or importing capital goods are affected by much the same sort of factors determining the exchange ratio for countries trading consumption goods. For example, a shift in preferences that raises saving propensities in either group of nations works to worsen the terms of trade of the countries importing capital equipment. Increasing the initial stock of capital in either group of countries or raising the marginal productivity of capital, however, may either increase or decrease the price of capital goods, depending upon the relative strength of the substitution

curve directly east of the old equilibrium point T, must be steeper than at T. Therefore, if future consumption is not an inferior commodity, the total volume of capital goods imported and produced domestically will be greater than under the no-trading conditions. For the capital-good-exporting nation, however, the new consumption-possibility curve will be flatter than before directly east of the old equilibrium saving point. Consequently, a lower accumulation point could be chosen without violating the noninferior good assumption.

If the assumption of similar tastes and factor endowments for all individuals within each country is dropped, these conclusions will be modified. In particular, since—under the usual assumptions about production functions—the opening-up of trade increases the income of the abundant factor and decreases that of the scarce factor, there is likely to be a redistributive effect that lowers the aggregate saving propensity in labor-abundant countries and raises it in capital-abundant economies. It is possible for this shift to be so strong that initially the growth rate declines in labor-abundant countries that import capital goods.

[8] This follows from the export-creating effect of capital accumulation for countries exporting the capital good (and the import-replacing effect of accumulation for countries importing the capital good), i.e., the Rybczynski effect in production, and from the assumption of a positive marginal propensity to consume the consumption item in the future periods.

[9] Besides the production of some amounts of the capital good and the consumption good, this condition requires that the full set of other assumptions needed for factor-price equalization be satisfied. See [4, pp. 48-49] and [10].

[10] A country might still export capital goods at this stage, however.

[11] If tastes differ among the trading countries, relative factor endowments will not be the same at a no-trade, stationary-state equilibrium point. World income also will be higher than under no-trade conditions.

[12] The rate of interest, however, will still be equal among the countries, even though there is no production of the capital good.

and income effects between present and future consumption. The same holds true for increases in the labor supply, except that in this case the factor increase—besides having a production effect—tends to decrease a country's propensity to save and therefore its demand for capital goods.

Should the relatively capital-abundant nations impose tariffs on the export product of the capital-scarce areas and thereby lower the international price of this item relative to the capital good, the accumulation rate of the latter nations will slacken. On the other hand, if the capital-scarce nations possess and exercise their monopoly power without retaliatory action by the capital abundant areas, they will increase their capital-formation rate.

III

Consider next the effects of dropping the assumption that capital is immobile between countries. This means that capital goods can flow from one country to another without being financed by consumption-goods exports. Under the assumptions made in elaborating the previous model, the opening-up of capital flows will result in a transfer of part of the capital stock of capital-abundant nations to the capital-scarce countries. The transfer will cease when all possess the same ratio of capital to labor, since at that point the marginal productivity of capital will be the same in every country. However, this is not the end of capital flows and trade even under the assumption of similar preferences[18] in both trading nations. National income as distinct from domestic product will be higher in the creditor nations due to the interest payments on their invested capital. Whereas the consumption-possibility curves for domestic investment will be the same in all countries, the volume of savings will be greater in the creditor countries because of their higher income levels. Additional capital, therefore, will tend to flow into the initially capital-poor countries as the accumulation process runs its course. Furthermore, the capital goods needed to undertake this additional accumulation will not be entirely produced in the initially capital-rich countries. Since the production possibility curves are the same in all countries, it will be advantageous for the creditor countries to obtain some of the capital goods needed for their investment in the debtor nations by purchasing them with exports of the consumption commodity.

Under the similar taste assumption the equilibrium capital-labor ratio will be the same in this case as in the no-trade and capital-goods-trade cases. But, unless the equilibrium interest rate is zero, the income level of the initially capital-abundant nations will be higher than in the relatively capital-scarce countries, because of their interest receipts. The capital-scarce countries are, on the other hand, able to reach the equilibrium capital-labor ratio in a shorter period of time than in the trade-alone and no-trade cases. They reap the benefits of capital accumulation on the productivity of their labor without having to pay the full foregone-consumption price of the equilibrium capital stock.

[18] Time preference functions are assumed to have the characteristics described in footnote 4.

IV

The preceding conclusion concerning the cessation of trade when equilibrium is reached has been based upon a simple two-commodity, two-factor model. Introducing another capital good as both an input and output does not change this conclusion. However, if the output bundle is composed of (say) two consumption goods and one capital good and the factors of production consist of the capital good, labor, and a natural resource, then the outcome is modified. With two immobile factors, the same factor ratios in general will not be reached among trading nations under the similar taste assumption, and trade in consumption goods will continue at the stationary-state position.[14]

For a larger proportion of trade, however, fixed natural resources (as distinct from traded primary products) are likely to become a less important source of comparative cost differences among countries as technology and living standards rise in the world. It may be increasingly useful to regard international trade, especially among the more advanced countries, as essentially an adjustment process in response to a disequilibrium situation. Moreover, if we consider the skill component of the labor force as a manipulatable variable that, by means of educational expenditures, changes during the capital accumulation process, then factor-ratio differences other than those based on natural resources are not permanent. Therefore, trade should be regarded as based on such factors as short-run differences in the knowledge of productive techniques, and differences in tastes—a factor that may also be best regarded as short-run. Differences in the nature and rate of technological discoveries, differences in rates of population growth, differences in preference changes, etc., may well maintain the relative importance of international trade, but the pure theory of trade should make clear the dynamic forces tending to reduce trade. By ignoring capital-goods trade that is financed by current exports, international trade theory has failed to do so.

Robert E. Baldwin*

References

1. J. Bhagwati, "The Pure Theory of Trade," *Econ. Jour.*, March 1964, *74*, 1-84.
2. G. Haberler, *The Theory of International Trade.* London 1936.
3. C. Iversen, *Aspects of the Theory of International Capital Movements.* London 1936.
4. M. C. Kemp, *The Pure Theory of International Trade.* Englewood Cliffs 1964.
5. W. Leontief, "Theoretical Note on Time-Preference, Productivity of Capital, Stagnation, and Economic Growth," *Am. Econ. Rev.*, March 1958, *48*, 105-11.

[14] As has been analyzed in connection with the factor price equalization theorem, additional complications are introduced when the number of commodities differs from the number of factors. See [11].

* The author is professor of economics at the University of Wisconsin.

6. ———, "Time-Preference and Economic Growth: A Reply," *Am. Econ. Rev.*, Dec. 1959, *49*, 1041-43.
7. J. E. MEADE, *Trade and Welfare* (The Theory of International Policy, Vol. II). Oxford 1955.
8. B. OHLIN, *Interregional and International Trade.* Cambridge 1935.
9. H. ONIKI AND H. UZAWA, "Patterns of Trade and Investment in a Dynamic Model of International Trade," *Rev. Econ. Stud.*, Jan. 1965, *32*, 15-38.
10. P. A. SAMUELSON, "Equalization by Trade of the Interest Rate Along with the Real Wage," *Trade, Growth, and the Balance of Payments*, Essays in Honor of Gottfried Haberler, Chicago 1965, pp. 35-52.
11. ———, "Prices of Factors and Goods in General Equilibrium," *Rev. Econ. Stud.*, 1953, *21*(1), 1-20.
12. F. M. WESTFIELD, "Time-Preference and Economic Growth: Comment," *Am. Econ. Rev.*, Dec. 1959, *49*, 1037-41.

Foreign Graduate Students in Economics

Since the end of World War II there has been sustained growth in the number of foreigners undertaking graduate work at U.S. universities. For the last ten years the rate of increase has averaged 11 per cent per year,[1] considerably higher than the average 7 per cent rate of growth[2] in total graduate enrollment. Behind the expansion lies a complex of factors: a world-wide emphasis on education and development which has resulted in a rise in the demand for trained personnel; a willingness on the part of U.S. graduate schools to accept for training a large number of students from abroad; the ability of institutions to provide the necessary financing; and an improvement in education in many countries which has prepared more students for graduate work. Foreign students inevitably raise problems for the universities. While the problems are not different in kind, they are certainly exaggerated in degree from those associated with training Americans. It is harder to interpret and compare credentials of foreign applicants; to find adequate financing for foreign students who are frequently unable to draw upon personal funds because of currency regulations, and whose employment opportunities in the United States are restricted by law; to design courses with content and level appropriate for students of diverse backgrounds; to teach people whose comprehension of English is imperfect; and to find topics suitable and faculty able to supervise dissertations for those who wish to work on problems of their own countries.

As recently as a decade ago these difficulties, at least as regards departments of economics, were largely confined to a small group of universities

[1] This is the rate of growth for the decade 1954-1964. Each year the Institute of International Education conducts a census of foreign students in the United States. The figures on overseas students are taken from the Institute's annual reports [4].

[2] This rate is for the nine years 1954-63. The figure on graduate enrollment for 1963 was taken from [7], that for 1954 from [6, p. 210].

which trained the majority of students from abroad.[3] But today graduate students from overseas are found in almost all U.S. economics departments.[4] The problems, especially those of selection, are compounded by the fact that applications no longer come from a select group of overseas schools but from a wide array of countries and institutions.[5] The increase in graduate students from abroad in economics has averaged 20 per cent per year[6] compared to 12 per cent[7] for the total graduate enrollment in economics. Today with roughly one out of every four advanced students being foreign, economics is as a discipline second only to agriculture in the percentage of its graduate candidates who come from abroad.

While the number of graduate students in economics from all areas of the world has risen in recent years, the most rapid growth has been in students from Africa and Latin America. Table 1 shows the number of students coming from the world's principal regions and separately for countries with 35 or more graduate students in economics. Information is also given in the table on the country of origin of graduate students in business administration, all foreign students, and graduate students in economics for the year 1962-63.[8]

Some students from abroad never go back to their homelands. It has been estimated that in total about 10 per cent of all foreign students remain in the United States, but no more than 7 per cent of the Chinese students from Taiwan, Hong Kong, and other South East Asian countries ever return to that area of the world [3]. The percentage going back to Korea is not much larger. Other countries with a substantial "loss" of students include India, Iran, Egypt, Lebanon, Pakistan, Greece, Colombia, Argentina, and Ecuador [3]. No separate information is available for economists,[9] but anyone who has visited the campuses of the smaller colleges is aware that a substantial number of

[3] During the decade 1945-54 over half the foreign graduate students in economics were trained at 10 institutions.

[4] There is still considerable concentration of foreign students at particular schools; half the foreign economics students were enrolled at 18 institutions in 1964-65. But out of 170 universities offering graduate work in economics, 153 reported at least one foreigner enrolled.

[5] In 1964-65 graduate students in economics came from 109 countries. During the decade 1945-54, 40 per cent of foreign students in economics came from European and North American universities; in 1964-65 only 20 per cent came from these two areas.

[6] The data on students in economics are available only for recent years. The rate reported is for the three-year period 1961-64 and is based on special tabulations prepared by the Institute of International Education.

[7] This rate is for the three-year period 1960-63 and is taken from the annual reports of [7].

[8] These figures exaggerate the numbers coming annually from India, China, Korea, and the United Arab Republic. Of those who began their graduate work in economics in 1963 or 1964, only 4.7 per cent came from India, 6.4 per cent from China, 3.2 per cent from Korea, and a 1.4 per cent from the United Arab Republic. The difference in figures is due to the fact that students from these countries remain longer on average in the United States.

[9] Herbert Grubel has provided the author with some unpublished data on the number of foreign-born economists in the United States. His figures show that 1300 or 12 per cent of the total are foreign born. Of this number, however, only about 300 came from regions other than Europe and Canada.

TABLE 1—FOREIGN STUDENTS BY REGION AND COUNTRY OF ORIGIN[a]

Homeland	Foreign Graduate Students in Economics 1964–65							
	Total	Degree Status			% of Total Fgn. Grad. in Econ.	% of Total Fgn. Grad. in Econ. 62–63	% of Fgn. Grad. in Bus. Admn. 64–65	% of All Fgn. Stud. Undergrad. & Grad. 1964–65
		Ph.D.	M.A.	No Degree				
Far East	855	326	395	134	39.1	42.3	46.3	35.8
Latin America	326	95	193	38	14.9	11.1	8.1	16.6
Europe	300	109	118	73	13.7	14.5	16.3	12.3
Near & Middle East	293	124	123	46	13.4	14.9	10.3	13.7
Africa	227	82	99	46	10.4	8.9	8.4	8.4
Canada & Bermuda	160	96	34	30	7.3	7.2	9.2	11.4
Oceania, etc.	24	14	4	6	1.6	1.1	1.4	1.8
Total	2185	846	966	373	100.0	100.0	100.0	100.0
India	214	132	53	29	9.8	11.8	9.4	8.3
China[b]	183	63	91	29	8.4	7.7	11.6	12.3
Canada	160	96	34	30	7.3	7.2	9.2	11.3
Korea	146	50	65	31	6.7	7.9	3.9	3.2
Japan	86	28	41	17	3.9	5.3	8.8	4.1
Thailand	85	15	63	7	3.9	2.8	1.9	2.0
U.A.R.	84	42	19	23	3.8	3.9	4.5	1.6
Philippines	67	13	42	12	3.1	3.3	7.9	3.0
U.K.	64	26	25	13	2.9	2.7	4.6	2.4
Pakistan	49	28	17	4	2.2	3.0	1.0	1.3
Greece	48	21	16	11	2.2	1.8	1.3	1.9
Israel	47	26	14	7	2.2	1.7	2.0	1.9
Argentina	46	23	17	6	2.1	1.5	.9	.8
Nigeria	45	17	20	8	2.1	1.1	.5	1.7
Iran	38	12	21	5	1.7	1.7	1.0	4.5

faculty at these schools are foreign-born. The new immigration bill will prob bly increase the number of those remaining in the United States by grantii immigrant status to individuals "capable of performing specified functions f which a shortage of employable and willing persons exists in the United Stat . . ." [8, p. 7.].

Table 2 gives information supplied by economics departments with 30 more students from abroad on enrollment, degrees, and financing of forei graduates.[10] Eight of these 18 departments appear on Cartter's list of tl country's leading 20 [1]. In fact, of all foreign graduate students in econor ics in 1964-65, 40 per cent were enrolled at the leading 20 institutions.

Officials of the leading departments reported to the author that in the pa the mean academic performance of foreign graduate students in economics h been poorer than that of U.S. students, and the dispersion of the foreigne records has also been greater. Table 3 gives a breakdown on enrollment a degrees for 98 departments of economics as a group, and for 18 departments agricultural economics. This information suggests that, at least in regard earning degrees, foreigners do about as well in graduate schools as domes students. For example while 27 per cent of the present Master's candidates a from abroad, foreigners received 30 per cent of the Master's degrees betwe 1959 and 1964. For doctoral candidates the situation is that 25 per cent of t students are foreign and between 1959 and 1964 foreigners received 20 p cent of the degrees. In the past, however, the proportion of doctoral can dates from overseas was smaller than it is today; adjustment for the lag wou show that foreigners have also been about as successful as Americans in ear ing the Ph.D. degree.[11]

The data in Table 2 suggest that at some schools foreigners have been le successful than Americans in earning the doctorate. A comparison of enro ment and degrees is, however, less accurate for a single institution than fo group. At the 20 leading departments 28 per cent of the enrolled doctoral ca didates are foreign; between 1959 and 1964 students from abroad earned per cent of the Ph. D.'s awarded by these departments.

The tabulations of the Institute of International Education show that t percentage of foreign graduate students studying for the doctorate in econo

[10] The departmental figures used in this communication are, except where noted, dra from reports submitted by the departments to the AEA for the compilation of [5]. Thou all figures were checked with the departments, the author feels that certain questions m have been misunderstood and the figures inaccurately reported by some departments.

[11] It has been said that some departments do not require as high a level of performa from foreign students as from Americans for the degree. Wharton [9] [10] stresses t point and then, as have other commentators on the subject, condemns the awarding "second class" degrees to those from abroad. Wharton's policy prescription is to as foreigners to achieve the same level of attainment by spending longer on their educati There may, however, be a role to be played by "half-trained" economists; using Friedma analogy in discussing the training of doctors and lawyers [2, p. 153], the developing count probably cannot afford only "Cadillac" economists. Certainly some American colleges empl teachers without the highest level of training. Perhaps the policy solution is more "labe rather than more training.

TABLE 2—ENROLLMENT, DEGREES, AND ACCEPTANCE RATES OF FOREIGN GRADUATE STUDENTS IN ECONOMICS BY DEPARTMENT, 1964–65[a]

Department	Fgn. Enrollment 64–65	Fgn. Std. as a % of M.A. Cand. 64–65	Fgn. Std. as a % of Ph.D. Cand. 64–65	Fgn. Degs. as a % of M.A.'s 59–64	Fgn. Degs. as a % of Ph.D.'s 59–64	% of Fgn. Applicants Accepted 64–65	% of U.S. Applicants Accepted 64–65
Berkeley (Econ.)	108	27.4	31.9	36.3	23.1	78.0[c]	78.0[c]
Columbia	93	39.0	38.0	N/A	36.7	47.0	50.0
New School	68[b]	N/A	N/A	N/A	N/A	80.0[c]	80.0[c]
Wisconsin (Econ.)	65	36.4	30.1	36.4	25.0	50.0	75.0
Chicago	63	54.5	44.6	50.6	21.1	65.0	85.0
Minnesota (Econ.)	60	40.3	23.2	34.1	24.2	95.0[c]	95.0[c]
Iowa State	59	35.2	42.4	26.2	32.3	66.0	47.0
Harvard	52	N/A	38.2	N/A	24.4	5.0	10.0
Wisconsin (Ag. Econ.)	50	73.2	42.3	48.9	26.7	20.0	30.0
U.C.L.A.	50	36.8	30.0	20.7	18.2	N/A	N/A
Yale	48	73.9	27.4	N/A	N/A	10.0	20.0
Vanderbilt	48	78.0	38.1	88.1	42.9	60.0	70.0
Southern California	45	50.0	50.0	33.3	25.9	25.0	50.0
Howard	39[b]	N/A	N/A	N/A	N/A	N/A	N/A
Cornell (Ag. Econ.)	34[b]	N/A	N/A	N/A	N/A	N/A	N/A
Michigan State (Ag. Econ.)	33	52.0	31.4	22.6	21.4	50.0	82.0
Indiana	33	27.4	27.3	41.0	21.4	50.0	75.0
New York University	30	24.2	6.2	23.5	20.0	56.0	59.0

[a] Based on reports filed with the AEA by the departments except as noted. In some cases, noted in parentheses, the data refer to only one department (economics or agricultural economics) even though the university has both departments. Figures are given only for departments with 30 or more foreign students enrolled.

[b] Based on Institute of International Education tabulations.

[c] The figures on acceptances were not reported separately for foreign and American students.

TABLE 3—ENROLLMENT AND DEGREES BY NATIONALITY

	Departments of Economics[a]			Departments of Agricultural Economics[b]	
	Nationality			Nationality	
	U. S.	Foreign	Not Reported	U. S.	Foreign
Enrollment 1964–65					
M.A.	1970	711		282	126
Ph.D.	2418	764		245	111
Special	107	71	32	15	25
Degree Status Not Reported	161	25		—	—
Total	4656	1571	32	542	262
Degrees Awarded 1959–64[c]					
M.A.	2064	941	461	512	153
Ph.D.	1273	389	101	235	62

[a] Based on figures submitted by 98 departments of economics.
[b] Based on figures submitted by 18 departments of agricultural economics.
[c] For a few departments the period for which degrees were reported differed slightly from the dates indicated, e.g., 1960–64.

ics rose from 30 per cent in 1962 to 40 per cent in 1964.[12] This trend seems destined to continue. Universities in the developing world are now attempting to upgrade standards by employing better-trained faculty. In the future most of those seeking Master's degrees will probably be planning careers in administration.[13] And many of these will receive their training in their own or neighboring countries. For these reasons it seems unlikely that there will be much growth in the number of foreigners seeking the M.A. degree in the United States. On the other hand, an increase in the number of applicants for doctoral programs seems almost certain because of the expansion in the foreign demand for people with the Ph.D. and in the supply of those prepared to undertake graduate work at an advanced level.

Table 4 shows that in 1964-65 only a fraction of foreign students received all or part of their funds from the universities. Universities tend to award their own funds to doctoral candidates, students from developed countries, and those already on campus. Other institutions giving aid follow different policies. The United States Government, for example, emphasizes fellowships of one or two years for Master's candidates from developing areas. Graduates in economics get considerably more support from their own and the United States government than do students in general; the governments of the Near East and Africa are particularly active in supporting economists.

[12] The departmental figures presented in Table 3 indicate that 50 per cent of the foreign students were pursuing doctoral programs; the difference is explained by the fact that most of the departments not included in the table offered only Master's programs.

[13] For these students graduate schools of business and public administration may provide training more suitable to their career objectives than departments of economics.

TABLE 4—FOREIGN STUDENT FINANCING BY SOURCE OF FUNDS[a]

	Source					
	University	Self	U.S. Govt.	Foreign Govt.	Private Foundation	Total
Graduates in Economics 1964–65	29.5%	28.7%	15.1%	10.3%	16.4%	100%
Degree Status:						
No Degree	25.7	44.3	7.8	10.7	11.4	100
M.A.	18.3	34.7	20.0	9.9	17.0	100
Ph.D.	43.1	19.3	10.8	10.6	16.2	100
Areas of the World:						
Far East	31.1	38.4	13.1	4.3	13.0	100
Latin America	18.1	18.8	27.5	11.5	24.0	100
Europe	43.7	20.3	10.0	4.3	21.6	100
Near and Middle East	22.7	30.0	21.0	15.9	10.3	100
Africa	16.6	17.6	15.0	32.6	18.2	100
Canada and Bermuda	46.2	28.3	0.7	9.7	15.2	100
Oceania, etc.	63.2	10.5	5.3	5.3	15.8	100
Graduates in Economics 1962–63	28.9	27.9	14.6	10.5	18.2	100
Graduates and Undergraduates in Economics 1964–65	24.7	38.1	14.5	8.9	13.9	100
Years Enrolled						
First Year	20.0	36.1	19.2	7.7	16.8	100
Second Year	26.6	31.7	17.6	8.8	15.3	100
Three or more years	29.4	41.7	8.9	9.5	10.1	100
Graduates and Undergraduates All Disciplines 1964–65	24.3	46.2	10.1	6.5	12.8	100

[a] Based on special tabulations prepared by the Institute of International Education and tables in [4]. Those who were financed by two sources (e.g., university and United States Government) are divided equally between the two classes.

Foreign students may well find it increasingly difficult to win acceptance at the leading institutions. Seven out of the top 20 departments (as listed by Cartter [1]) no longer offer programs leading to the Master's degree. And because their preparation is different, foreign candidates frequently require semi-remedial training before beginning their doctoral studies; at institutions without a Master's program, courses at this level are commonly not available. The seven departments specializing in doctoral work follow restrictive admission policies, accepting in 1964-1965 on average only 13 per cent of their foreign applicants against a 43 per cent acceptance rate at the other 13 leading departments. If specialization spreads, there may be a demand in the future for some departments to offer a Master's program designed to prepare students with weaker backgrounds, both foreign and American, for doctoral work at the leading institutions.

To assist the economics departments determine which applicants meet their admission standards, the AEA has established a Foreign Students Screening Project. Committees of economists have been set up in many developing countries to interview candidates, evaluate their credentials, and report to the economics departments on student qualifications. Any department wishing an

evaluation of a foreign applicant should contact the Association's offices in Evanston. At least six weeks time should be allowed to obtain a report.

MILLARD F. LONG*

REFERENCES

1. A. CARTTER, "Economics of the University," *Am. Econ. Rev., Proc.*, May 1965, *55*, 481-94.
2. M. FRIEDMAN, *Capitalism and Freedom*. Chicago 1962.
3. G. HENDERSON, "Foreign Students: Exchange or Immigration," *NAFSA News Letter*, Nov. 15, 1964, *16*, 1-4.
4. INSTITUTE OF INTERNATIONAL EDUCATION, *Open Doors*, New York, I.I.E. Annual Volumes, 1955-65.
5. M. LONG, ed., *Graduate Study in Economics*. Evanston, American Economic Association, 1965.
6. U.S. DEPARTMENT OF COMMERCE, *Historical Statistics of the United States —Colonial Times to 1957*. Washington 1960.
7. U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE, *Summary Report: Student Enrollment for Advanced Degrees*. Washington, Annual Report, 1960-63.
8. U.S. HOUSE OF REPRESENTATIVES, "Amendment to the Immigration and Nationality Act," H.R. 7700. Washington, July 23, 1963.
9. C. R. WHARTON, "Revolutionizing Higher Education in the Developing World." (Mimeographed)
10. ———, *The U.S. Graduate Training of Asian Agricultural Economists*. New York, Council on Economic and Cultural Affairs, 1959.

* The author is presently associate professor of business economics in the Graduate School of Business at the University of Chicago. During the academic year 1964-65 he was director of the Foreign Students Screening Project of the American Economic Association.

An Implication of the Labor-Surplus Assumption

In recent articles, G. Ranis and J. C. H. Fei [1] [3] have significantly elaborated the basic W. A. Lewis model [2] of the labor-surplus economy. In their model, a successful development effort is fueled by the saving of industrial capitalists which permits a portion of industrial output to be invested in both the industrial commercialized sector and the agricultural labor-surplus sector. Investment must be allocated to permit the simultaneous absorption by the industrial sector of (1) a larger agricultural surplus, and (2) a greater proportion of the labor force (at a constant real wage) [3, pp. 540–46].

Since the allocation of investment between the two sectors is crucial to the growth and labor reallocation process, more should be said about the capital-allocation mechanism than the authors have already done [3, p. 545]. This brief note suggests that the micro foundations of the agricultural sector's private investment function may be extremely shaky so long as

labor-surplus conditions prevail. Official intervention in investment decisions may indeed be in order, as the authors suggest [3, p. 545], if the development effort is to succeed.

The labor-surplus assumption implies that the marginal social product of capital (its shadow price) exceeds its marginal private product. Moreover, an exogenous rise in the labor force (*e.g.*, due to higher birth or survival rates) will drive down the private return to capital while its social product rises. Conversely, an increase in capital employed with a given labor force will raise its private return although its marginal social product fails.

This "perverse" effect is illustrated in Figure 1. The capital stock and land are held constant, and labor is plotted along the horizontal axis. The marginal products of labor (f_L) and capital (f_K) are plotted along the vertical axis together with the traditional real wage ($\bar{u}$) and the private return to capital (r). Labor surplus conditions prevail if L exceeds L_0, since the marginal product of labor falls short of its real wage in that range. At L_0, the total product is

$$\sum_{0}^{L_0} f_L \Delta L.$$

The wage bill is $OL_0H\bar{u}$ and capital's rent is the shaded "triangle," E. The marginal private and social rates of return on capital coincide at $r_0 = f_{K0}$. If the labor supply were to rise to L_1, the wage bill rises to $0L_1D\bar{u}$, and capital's rent is reduced by the shaded triangle, F. The marginal social return on capital rises to f_{K1} but its marginal private return falls to r_1.

It would be hazardous to ascribe economic meaning to these results without detailed knowledge of the micro decision-making units and their institutional environment. The crucial point is whether or not investment decisions are made with respect to the marginal social product of capital (f_K) or its

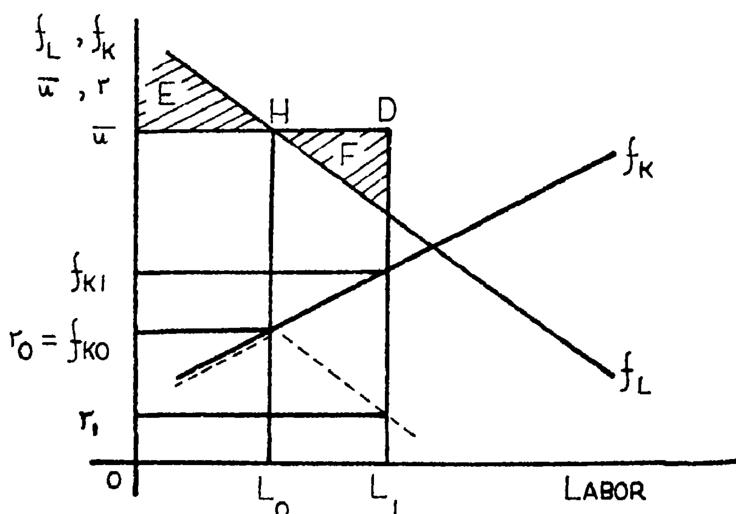


FIGURE 1

marginal private product (r). If investment decisions are made within an extended family unit, then little if any distinction is made between capital and labor income. Consequently, this type of unit can be expected to compare the change to its total product by adding more capital with the marginal cost of doing so. Thus f_K, not r, is the relevant decision-making variable, and no special problem is introduced by the divergence of f_K and r owing to labor-surplus conditions.

On the other hand, if the investment decision is made by a landlord who (1) has alternate investment opportunities, (2) is committed to paying the traditional real wage, and (3) is unwilling or unable to control L, then r is clearly relevant, not f_K. If r_0 is the opportunity cost of investing in his agricultural enterprise, then a rise in labor from L_0 to L_1 may induce him to allow his agricultural capital to depreciate and invest elsewhere. Alternatively, he may *raise* his rate of return by *more* investment, restoring r to r_0. But surely it is not clear that a falling rate of return will induce a higher rate of investment.

Various tenant and sharecropping arrangements bring both f_K and r into a joint decision-making process too involved to analyze here. Nevertheless, perhaps enough has been said to suggest that the labor-surplus assumption may imply private investment behavior in the labor-surplus sector which could lead to underinvestment in agriculture, a fall in the industrial terms of trade with agriculture, and a consequent retardation of the development process.

PHILIP A. NEHER*

* The author is a graduate student of economics at Brown University. He wishes to thank G. H. Borts, J. Hanson, and a referee of this *Review* for their suggestions. Remaining errors are the author's.

REFERENCES

1. J. C. H. FEI AND G. RANIS, "Innovation, Capital Accumulation, and Economic Development," *Am. Econ. Rev.*, June 1963, *53*, 283–313.
2. W. A. LEWIS, "Development with Unlimited Supplies of Labor," *Manchester School*, May 1954, *22*, 139–92.
3. G. RANIS AND J. C. H. FEI, "A Theory of Economic Development," *Am. Econ. Rev.*, Sept. 1961, *51*, 533–65.

Some Simple Propositions Concerning Cost-Push Inflation

The government's wage-price guidepost policy is predicated on the assumption that firms in basic industries possess discretionary pricing power. But this assumption in itself does not suffice to justify government intervention in steel, copper, and aluminum pricing decisions. The case for intervention rests on the presumption that price adjustments in those key sectors have a significant effect upon the general price level. Clearly, if wage-price guideposts are to be frequently invoked in an attempt to combat infla-

tion, the task of obtaining quantitative estimates of the inflationary impact of autonomous price and wage adjustments in key sectors of the economy is of critical importance.

How much inflation would have been generated if the price of steel had increased by $6.00 per ton in 1962? Obviously, the answer to this question must hinge in large measure upon the response of wages, profit markups, and prices in other sectors of the economy. In the first section of this paper we estimate the extent of the inflation that would have been generated if the price of steel had increased by $6.00 per ton in 1962 while the response of other sectors of the economy was constrained by the guideposts. Guidepost policy allows steel-using firms to respond to rising material costs by raising their prices rather than reducing their profit margins; similarly, the customers of steel-using firms may pass along their costs, and so on to the eventual consumer. On the other hand, the guide posts do not permit money wage rates to adjust in response to rising prices.[1] If the price response of steel-using sectors and labor are not governed by the guideposts, our initial estimate of the inflationary effect of a $6.00 increase in the price of steel must be modified. Section II considers the implications of certain sectors absorbing cost increases from profit margins rather than passing them along to their customers. Section III evaluates the effects of induced markup and wage increases that violate the guideposts.

Our estimates are based on a series of quite general propositions that can be employed for estimating the inflationary effects of autonomous price and wage adjustments in any sector. In deriving these propositions, we employ the input-output approach developed by Wassily Leontief [2] for analyzing price movements. This approach takes into account both the direct purchases of steel and the steel content of other purchased inputs in evaluating the effect of a change in the price of steel upon production costs in other industries. This same procedure was utilized by Otto Eckstein and Gary Fromm [1] in their analysis of the contribution of steel to the inflation of the 1950's. In contrast to Eckstein and Fromm, however, we shall employ the implicit GNP price deflator as a gauge of movements in the general price level. The complicated input-output procedure reduces to a matter of quite simple computations once the GNP deflator rather than the wholesale price index is selected as the gauge of movements in the general price level.[2]

[1] Wage increases under the guideposts are equal to the trend rate of over-all productivity increase, which can be regarded as exogenous in analyzing the incremental effects of autonomous price adjustments. On the other hand, firms are permitted to raise prices in response to increases in nonlabor costs. Cf. [5, p. 189].

[2] A disadvantage of the wholesale price index as a measure of inflation arises from the excessive weighting that it gives to such commodities as steel, because commodities enter the index repeatedly at successive stages of the production process. The GNP deflator has considerably broader coverage, including services and construction. While the GNP index has limitations as a measure of long-run price movements because of difficulties involved in making appropriate adjustment for quality improvements, this limitation is not critical when the objective is to measure the incremental effect on the general level of prices of autonomous price and markup changes. An advantage of the GNP deflator is that it is based upon current quantity weights.

I. *The Basic Proposition*

We shall establish the following proposition:

> The impact upon the GNP implicit price deflator of an autonomous increase in the gross profit margin of a particular industry may be approximated by dividing GNP into the product of the change in the margin times the level of gross output for that industry, provided that gross profit margins in other sectors and wage rates are unaffected.

his proposition suggests that if the steel industry had raised prices by 5.00 per ton in 1962 (an increase of approximately 3.8 per cent), it would ave raised the general price level as measured by the GNP deflator by only ne-tenth of one per cent;[3] that is, since 98.3 million tons of steel were prouced in that year and GNP was $553.9 billion,

$$\frac{\Delta p}{p} = \frac{\$6.00 \times 98.3 \text{ million tons}}{\$553.9 \text{ billion}} = 0.001065 \quad \text{or} \quad 0.1065\%.$$

lthough this figure may appear small, there are several reasons why it robably overstates the inflation that would be generated by a rise in the rice of steel if firms in other sectors of the economy and labor abided by the uideposts. We shall see that the derivation of our basic proposition requires ıe assumption that the composition of final demand and the input-output oefficients are insensitive to moderate changes in relative prices. In utilizıg the final bill of goods that was actually purchased in the absence of the eel price rise in this calculation we are in effect employing a Laspeyres rice index, and thus exaggerating the effects of the price increase.[4] The ssumption of fixed technological coefficients also contributes to an overatement of the inflationary impact of an increase in the price of steel.[5] 'urthermore, the proposition refers to a gross profit margin rather than to a rice increase; consequently, it leads to an overstatement of the effects of a 6.00 per ton increase in the *price* of steel to the extent that the price adjustıents permitted of other sectors under the guideposts, by increasing the

[3] This is an estimate of the *incremental* effect upon the general level of prices of an increase the steel margin. The actual change hinges upon how a host of inflationary and deflationary rces balance out. The implicit price index increased from 115.8 to 116.7 (1954=100) from)61 to 1962, and our estimate is that the index would have stood at 116.8 if the price of steel ıd increased by $6.00 per ton in 1962.

[4] The GNP deflator is a Paasche index; it uses current rather than base-period weights. But nce the $6.00 per ton increase was not maintained, our measure utilizes weights in the abnce of the price change, and is Laspeyres in the sense in which the term is used by welfare onomists. More technically, prices are weighted in the GNP deflator in accordance with the tual composition of final demand (the final bill of goods); if the price increase had been aintained, the application of our proposition would have yielded the change in a Paasche ıdex because the weights would have reflected the shift in the composition of final demand ıduced by the price change.

[5] A hypothetical example will illustrate the necessity of assuming fixed technological coeffients. If aluminum and steel were essentially perfect substitutes, and if relative prices were ıch that steel-users were on the borderline of indifference between using aluminum rather ıan steel, a $6.00 per ton increase in the price of steel would precipitate the general substituon of aluminum for steel without any material impact upon the price of final product.

costs of raw materials in the production of steel, prevent a full $6.00 per ton increase in the steel margin.

In demonstrating the validity of our proposition it is convenient to employ matrix algebra. Let $P=[p_i]$ denote the row vector of industry prices, $A=[a_{ij}]$ an input-output matrix of technological coefficients a_{ij} revealing the quantity of good i required to produce a unit of gross output of good j, $M=[m_i]$ a row vector of gross profit margins, $L=\text{diag}\ (l_i)$ a diagonal matrix of labor coefficients indicating the amount of labor of the ith type necessary to produce one unit of the ith commodity, and $W=[w_i]$ a row vector of industry wage rates.[6] Then the matrix equation

$$M = P - PA - WL \tag{1}$$

defines the gross profit margin of each sector as the difference between price and unit material and labor costs. Thus, the gross profit margin includes capital consumption allowances and indirect business taxes. From this expression it is apparent that given industry markups, wages, and the matrices A and L, the vector of sector prices may be determined by calculating

$$P = (M + WL)(I - A)^{-1}. \tag{2}$$

The GNP deflator is obtained by dividing GNP in current dollars by the value of final outputs measured by prices in the base period. Let the column vector $Y=[y_i]$ denote the final bill of goods whose components reveal for each commodity i the sum of investment, consumption, government spending, and the excess of exports over imports. Provided that trade, construction, etc., have been included as sectors in addition to manufacturing, GNP is simply PY, and the GNP deflator may be denoted by:

$$p = PY/P^0Y, \tag{3}$$

where the superscript indicates, in general, that the variables refer to the base period. Utilizing (2) in conjunction with the fundamental equation of input-output analysis, $Y=(I-A)X$, yields:

$$PY = (M + WL)(I - A)^{-1}(I - A)X = (M + WL)X; \tag{4}$$

this states that GNP is identical to the sum of value added by each industry. Substituting (4) into (3) and differentiating with respect to m_i, the ith industry's profit margin, we have:

$$\frac{\partial p}{\partial m_i} = \frac{x_i}{P^0Y}. \tag{5}$$

[6] Our basic proposition does not require the use of an estimated input-output matrix. This means that exogenous changes in the input-output and labor utilization coefficients as a result of such factors as technological change offer no difficulties, although we do require that the coefficients be insensitive to moderate changes in relative prices and wages. Note too that empirical considerations do not restrict the number of sectors, no problem of aggregation confronts us, and in principle we could regard quantities as measured in conventional physical units (e.g., tons of steel and hours of labor); alternatively, of course, the output of each sector can be measured in terms of prices in a base period, the units of measure being dollars, as is customary in empirical input-output applications.

Because the model is linear,

$$\Delta p = \frac{x_i \Delta m_i}{P^0 Y}, \tag{6}$$

which is our basic proposition. As a convention, it is convenient to adopt as the base P^0 the level of prices that actually would have prevailed in the absence of the margin change, hence, $100\Delta p$ is the percentage increase in the GNP deflator generated by the increase in the profit margin.

It is interesting to contrast the simplicity of (6) with the computations undertaken by Eckstein and Fromm [1] in their input-output analysis of the inflation of the 1950's. While they also assume that wage rates and profit margins in other sectors of the economy are unaffected by steel-pricing policy, they employ the wholesale price index rather than the GNP deflator. With this approach they have, instead of our equation (4), the expression:

$$PQ^0 = (M + WL)(I - A)^{-1}Q^0, \tag{7}$$

where Q is a column vector of weights in the wholesale price index. The level of the index, then, is

$$p_w = \frac{PQ^0}{P^0Q^0} = \frac{(M + WL)(I - A)^{-1}Q^0}{P^0Q^0} \tag{8}$$

and

$$\Delta p_w = \frac{c_i Q^0}{P^0 Q^0} \Delta p_i, \tag{9}$$

where the vector c_i is the ith row of $(I-A)^{-1}$. The implementation of this approach requires a reconciliation of the wholesale price index classification with that of an empirically estimated input-output matrix of flow coefficients, the inversion of $I-A$, and computation of the inner product of the vector of price index weights times the steel row of $(I-A)^{-1}$. This approach, unlike ours, is sensitive to errors in measuring the input-output coefficients.[7]

Charles L. Schultze [4] has employed a national-income-accounting framework which facilitates the comparison of industry price and cost movements with changes in the GNP deflator. He has emphasized [4, pp. 19, 52] that his detailed tables presenting the contribution of various sectors of the economy to changes in the general price level for the 1947–57 period should be given an accounting rather than a "casual" interpretation. Since his tables are based on a formula equivalent to that stated in our basic pro-

[7] Eckstein and Fromm argued that if it had not been for the "extraordinary behavior ot steel" the wholesale price index would have risen by 14 rather than 23 points from 1947 to 1958. This does not imply, however, that the application of their procedure would yield an estimate of the inflationary effect of a \$6.00 increase in the price of steel in 1962 grossly different from ours. The \$6.00 increase that generated so much excitement in 1962 was much smaller in magnitude than the steel price increases that occurred in the decade examined by Eckstein and Fromm.

position, our analysis suggests that precisely the same assumptions as those specified in this paper and by Eckstein and Fromm would have to be satisfied before Schultze's calculations could be regarded as indicating the extent to which various sectors have caused inflation. Specifically, changes in profit margins and money wage rates must be exogenous and, in addition, the technological coefficients must be insensitive to moderate changes in relative prices.[8]

II. *Cost Absorption*

If, as is sometimes alleged, firms in certain sectors of the economy exercise discretionary pricing power rather than responding promptly to changing demand and cost conditions, it seems probable that these same firms may choose partially to absorb cost increases, at least temporarily, rather than pass them along to their customers through higher prices. In these circumstances, the inflationary impact of an increase in a basic industry's markup upon the general price level will be reduced or delayed. We shall show:

> When some industries absorb cost increases, while the remaining industries preserve profit margins in the face of changing costs, the inflationary impact of an autonomous increase in the gross profit margin of a particular industry is governed by the proportion of that industry's total output not required, directly or indirectly, by the cost-absorbing industries.

To verify this assertion, which constitutes the price-dual of Paul Samuelson's application of the LeChatelier Principle to input-output models [3], let us suppose, as a matter of notational convenience, that the first n_1 sectors adjust prices in order to preserve profit margins while the remaining $n_2 = n - n_1$ sectors absorb cost increases. Let us consider the $n \times n$ matrix A of input-output coefficients in partitioned form:

$$A = \begin{bmatrix} A_{11}; & A_{12} \\ A_{21}; & A_{22} \end{bmatrix},$$

where the submatrix A_{11} is of dimension $n_1 \times n_1$, A_{12} is $n_1 \times n_2$, etc.; similarly, we partition $P = [P_1; P_2]$, and so forth. The matrix equation explaining prices in the margin-preserving industries is

$$P_1 = P_1 A_{11} + P_2 A_{21} + M_1 + W_1 L_1 \tag{10}$$

$$= (P_2 A_{21} + M_1 + W_1 L_1)(I - A_{11})^{-1}. \tag{11}$$

Since the last n_2 sectors are cost absorbers, the vector P_2 is exogenous. If we let the n_1 component column vector $X_1^* = [x_i^*]$ denote $(I - A_{11})^{-1} Y_1$, the market value of all goods and services produced in the economy is

$$PY = P_1 Y_1 + P_2 Y_2 = (P_2 A_{21} + M_1 + W_1 L_1) X_1^* + P_2 Y_2 \tag{12}$$

$$= P_2 (A_{21} X_1^* + Y_2) + (M_1 + W_1 L_1) X_1^*. \tag{13}$$

[8] See footnote 5.

Substituting this last expression into equation (3) yields a new expression for the GNP deflator:

$$p = \frac{P_2(A_{21}X_1^* + Y_2) + (M_1 + W_1L_1)X_1^*}{P^0Y}. \tag{14}$$

If one of the first n_1 (margin-preserving) sectors autonomously adjusts its profit margin, the GNP deflator will obviously change by

$$\Delta p = \frac{x_i^*}{P^0Y} \Delta m_i, \qquad (i = 1, 2, \cdots, n_1). \tag{15}$$

This is smaller than our original estimate of the inflationary impact of a markup change by the proportion x_i^*/x_i—the fraction of the actual gross output of the markup increasing sector that is not used, directly or indirectly, by cost-absorbing industries in the production of the final bill of goods. On the other hand, the sector initiating the inflation may be a cost-absorber; that is, it increases its price and allows any subsequent induced cost increases to erode its gross profit margin. From (14) we see that in this case

$$\Delta p = \frac{x_i^{**}}{P^0Y} \Delta p_i, \qquad (i = n_1 + 1, \cdots, n), \tag{16}$$

where

$$x_i^{**} = \sum_{j=1}^{n_1} a_{ij}x_j^* + y_i$$

is the ith element of the column vector $X_2^{**} = A_{21}X_1^* + Y_2$. The quantity x_i^{**} is that component of x_i that is not used, directly or indirectly, by cost-absorbing industries.

In utilizing our first proposition to estimate that the effect of a $6.00 increase in the price of steel would raise the GNP deflator by 0.11 per cent if all other sectors observed the guideposts, we were neglecting the possibility that the rise in prices precipitated by this action might raise the costs of inputs to steel and prevent the margin on steel from rising by a full $6.00 per ton. A $6.00 price increase under these circumstances means that steel is a cost-absorbing sector, and our estimate overstates the effects of the price increase by the ratio x_i^{**}/x_i, where x_i^{**} is the output of steel that is not used, directly or indirectly, as inputs by the steel industry.

III. *Attempts to Preserve Real Shares*

Our suggestion that a $6.00 per ton increase in the price of steel would have raised the general level of prices by about one-tenth of one per cent was based on the assumption that money wage rates and profit margins in other sectors are unaffected by increases in prices, as required by guidepost policy.

In this section we explore the consequence of attempts in other sectors preserve real wages or profit margins following an exogenous price increas We begin by considering the effect of attempts by labor in all industries maintain real wage rates. We will show that:

> If all money wage rates adjust to the increase in the cost of living, the original inflationary impact of an increase in the gross profit margin will be multiplied by the reciprocal of one minus labor's share.

Since labor's share in U. S. output is roughly three-fifths, the inflationa impact of a $6.00 per ton increase in the price of steel would have been abo 0.27 per cent instead of the 0.11 per cent, if all wages adjusted to the pri increase.

This revised estimate relies on the assumption that movements induced the cost of living index are about the same as the changes in the implic GNP deflator so that the vector of industry money wages is:

$$W = pW^0, \tag{17}$$

where the scalar p is again the GNP deflator and W^0 the vector of initi money wage rates. Substituting (17) into equations (4) and (3) yields:

$$PY = (M + pW^0L)X, \tag{18}$$

and

$$p = \frac{(M + pW^0L)X}{P^0Y}. \tag{19}$$

Hence,

$$p = \frac{MX}{P^0Y - W^0LX} = \frac{MX}{P^0Y}\left(\frac{1}{1-\lambda}\right) \tag{20}$$

where

$$\lambda = \frac{W^0LX}{P^0Y}$$

denotes labor's share.

The inflationary effect of a change Δm_i in the markup of sector i is there fore:

$$\Delta p = \frac{x_i\Delta m_i}{P^0Y}\left(\frac{1}{1-\lambda}\right). \tag{21}$$

If only certain portions of the labor force maintain real wage rates as price rise, λ in equation (21) corresponds to that fraction of GNP represented b their money wages.

More generally, equation (21) reveals the effects of the preservation c real gross profit margins and wages in specific industries, once λ is appropri

ately reinterpreted.[9] Suppose, for example, that while labor and producers in the manufacturing sector, other than steel, observe the guideposts, prices and wages in nonmanufacturing sectors adjust so as to preserve real shares. Under these circumstances, the inflationary impact of a \$6.00 per ton increase in the price of steel would be considerably larger than our initial estimate suggested. Since gross product originating in manufacturing is roughly one-third of GNP, $\lambda = \frac{2}{3}$ and a \$6.00 steel markup increase in these circumstances would raise the GNP deflator by approximately:

$$\Delta p = .1065\%\left(\frac{1}{1 - \frac{2}{3}}\right) = 0.32\%.$$

Following the procedure set forth in Section II, this estimate can be adjusted to take account of cost absorption, i.e., deterioration in the real wage or profit margins in various sectors.

IV. *Conclusions*

Any analysis of cost-push inflation is incomplete, for the concept neglects the fact that price increases reduce real final demand unless offset by expansionary monetary or fiscal policy. Subject to this limitation, the propositions set forth in this paper provide a simple method for obtaining quantitative estimates of the inflationary effects of autonomous price increases, once the responses of other sectors are specified.

The extent to which a departure from the guideposts actually contributes to inflation depends upon both the magnitude of the violation and the sensitivity of the general price level to price adjustments made by the sector deviating from the rules of noninflationary behavior. We estimate that a \$6.00 increase in the price of steel in 1962 would have raised the GNP deflator by less than 0.11 per cent, *if* labor and firms in other sectors of the economy had observed the guideposts. This suggests that violations of the administration's guideposts by so called "key industries" have not in themselves constituted a clear and present danger to price stability.

Autonomous price adjustments that induce other sectors to violate the guideposts have a more substantial effect. For example, we found that the inflationary impact of a \$6.00 per ton increase in the price of steel in 1962 would have been 0.27 per cent if unions negotiated wage adjustments preserving labor's real share. Additional inflation would have been generated if firms in other sectors of the economy were induced to raise their profit margins. This suggests that the case for enforcing the guideposts must be based on the argument that price adjustments in key sectors constitute a danger-

[9] An increase in a particular sector's margin will cause all prices to rise proportionately if all other sectors adjust their prices so as to preserve their real shares. This result, which is in conformity with classical general equilibrium theory, is derived by observing that in these circumstances equation (20) reduces to $p = MX/m_i^0 x_i$, where i denotes the sector attempting to increase its margin, the only sector that suffers from money illusion. If no sector suffers from money illusion, $\lambda = 1$ and prices will rise indefinitely as the result of a sector's attempt to raise the real value of its margin.

ous precedent which will generate inflation by precipitating wholesale violation of the rules of noninflationary price behavior.

WILLIAM BRAINARD AND MICHAEL C. LOVELL*

* William Brainard is a member of the staff of the Brookings Institution on leave from Yale University. Michael C. Lovell is at the Graduate School of Industrial Administration, Carnegie Institute of Technology. His research time was sponsored by a National Science Foundation grant and a Ford Faculty Research Fellowship. The authors are indebted to Samuel B. Chase, Jr. of the Brookings Institution and Leonard Rapping of Carnegie Institute of Technology for many useful substantive and stylistic suggestions. The authors retain full responsibility for the views expressed in this paper.

REFERENCES

1. OTTO ECKSTEIN AND GARY FROMM, "Steel and the Postwar Inflation," Study Paper No. 2, *Employment, Growth and Price Levels*, Joint Economic Committee, 86th Cong., 1st sess., Washington 1959.
2. WASSILY LEONTIEF, *The Structure of the American Economy, 1919–1939*. Oxford 1941.
3. PAUL A. SAMUELSON, "An Extension of the LeChatelier Principle," *Econometrica*, April 1960, *28*, 368–79.
4. CHARLES L. SCHULTZE, *Prices, Costs and Output for the Postwar Decade*, Committee for Economic Development Supplementary Paper. New York 1959.
5. *Economic Report of the President*. Washington 1962.

Rejoinder

We wish that finished steel mill product prices had not doubled from 1947 to 1957—or at least had risen no more than prices generally. We would have been spared a lot of writing. The steel companies would have been spared losses of markets to other materials, deterioration of their profit position, and the public attention which they do not welcome. Steel workers—whose wage increases contributed to the inflation in steel in the mid-1950's—would have been spared large job losses and a weakening of their bargaining position. And the economy would not have suffered a loss in its international balance of trade of $1.25 billion in steel alone between 1955–57 and 1965, plus additional losses in machinery and other metal products.

Our steel study of 1959 pointed out the obvious, that steel prices rose a great deal and that they had a pervasive effect on industrial product prices. Our methodology, using input-output, had some novelty, but the main points could have been made by simpler computations.

The paper has spawned a succession of other papers, some serious pieces of work, others not. (Our favorite reply is U.S. Steel's Jules Feiffer-like ad in a recent *New Yorker*.) The Brainard-Lovell paper does fall in the serious category, but not as serious as they think. Two of their three main points are

ot particularly novel or new, and where they venture into new territory ıey are not quite serious.

Our comments follow the organization of their paper.

1. The logic of Proposition I has long been recognized. The arithmetic roposed by Brainard-Lovell was actually applied in our paper in footnote [2]. Charles L. Schultze's analysis to which they refer is based on essentially ıe same scheme. And Martin Bailey presented a very similar mathematical gument in 1962 [1].

Brainard and Lovell's empirical point is that a $6 per ton increase in 1962 ould have raised the GNP deflator by *only* 0.1 per cent if all other sectors llowed guideposts. This seems like a tiny number, but if a single, price ıange can raise the economy's over-all price level by 0.1 per cent, that is ıite astonishing. During the period of industrial price stability from 1959 1963, the over-all GNP deflator rose by 1.3 per cent a year mainly due the rise of the indexes for construction, services, and government. Beıuse the deflators for construction and government are price indexes of puts rather than outputs, this figure overstates the price rise of that riod. From 1963 to 1965 the GNP deflator rose at an annual rate of 1.8 r cent, or an extra ½ per cent. And more recently the rise in the deflator celerated to 2½ per cent. If the difference between price stability and flation is only about one percentage point in the GNP deflator, then the rainard-Lovell findings should make us look out for steel price increases.

Brainard-Lovell also raise once more the old issue of the GNP deflator vs. ıe wholesale price index as an indicator of inflation. Both have their merits ıd their faults. The GNP deflator has substantial upward bias because its vernment and construction components are largely based on input rather ıan output prices. These two sectors account for almost half the rise in ıe period 1959–1963. The rest was mainly in services, another sector here prices are hard to measure. And these are all sectors with little direct ırticipation in international trade. So this index is not a good base for easuring the significance of changes in specific industrial prices.

The over-all wholesale price index suffers from a kind of "double countg" because it is weighted by the relative importance of sales, irrespective whether these are in final demand or intermediate markets. For this rean our study gave equal stress to the impact on the wholesale price index r finished goods, where intermediate products such as steel have no direct eight at all.

But more important, and as Brainard-Lovell agree, the isolated price crease is not the end of the story. Several basic materials, including eel, aluminum, plastics, (and also foreign steel) are substitutes; a price crease in one of these materials tends to produce increases in the subitutes. So substitution is a two-edged sword as far as inflation is conrned. More generally, the various feedbacks and spillovers in product ıd factor markets are the essence of the inflationary process.

2. The second proposition is obviously correct. It says that, if some dustries absorb cost increases and others pass them on, there will be less

inflation if the cost increases apply to cost-absorbing industries. Surely no one ever thought otherwise.

3. The third section is an attempt to take account of the wage side of a mechanical wage-price spiral, in which all of the price increases are passed on in higher money wages, and all of the wage increases are passed on in higher prices. This is essentially a form of multiplier process, in which the analogue to the marginal propensity to spend is the fraction of income payments going to wages. Nonwage income is the leakage.

The empirical result which emerges—a multiplier of 3 on the initial price rise because the wage share is $\frac{2}{3}$—is an interesting illustration but not a serious estimate. The model leaves a good deal to be desired. The more its logic is extended to other incomes, the larger becomes this multiplier. For example, higher steel prices will raise the price of equipment, making for larger depreciation charges. And we know that wage determination is rather more complex.

4. The conclusion states "any analysis of cost-push inflation is incomplete, for the concept neglects the fact that price increases reduce real final demand unless offset by expansionary monetary or fiscal policy." This commonly made statement overlooks the fact that a cost-push price increase simultaneously provides the increased income to validate itself. The increase of price does not directly cut purchasing power. It only transfers it. The beneficiaries of the cost-push price increase raise their own money income, thereby keeping total real purchasing power in the economy intact. A neutral fiscal policy, defined as absence of any discretionary action, does not offset the price increase except in so far as inflation may lead to an automatic increase in the full-employment surplus. Whether "expansionary monetary policy" is needed to permit the price increase to be matched by rising demand depends upon semantics. If a neutral monetary policy is defined as maintenance of a constant free-reserve target, a definition given weight by the monetary authorities, then a cost-push inflation will automatically lead to an increase of the money supply.

Lest we be misunderstood, we add that cost-push has been no more than a minor element in the recent increases of industrial prices. And steel prices were virtually stable in recent years. The steel price-wage episode of the 1950's might now best be turned over to the historians. The flood of imports and public concern have changed the behavior of steel prices in the United States. Perhaps this is one problem that has been solved.

Otto Eckstein and Gary Fromm*

* Harvard University and the Brookings Institution, respectively.

References

1. Martin J. Bailey, "Steel and the Postwar Rise in the Wholesale Price Index," *Jour. Bus.*, April 1962, *35*, 153–57. "Reply," by Otto Eckstein and Gary Fromm, *ibid*, Jan. 1963, *36*, 109–10.
2. Otto Eckstein and Gary Fromm, "Steel and the Postwar Inflation," Study Paper No. 2, *Employment, Growth and Price Levels*, Joint Economic Committee, 86th Cong., 1st sess., Washington 1959.

Lifetime Income and Economic Growth: Comment

In a recent article [1], Herman P. Miller discusses the effects of economic growth on lifetime earnings. His main findings are:

1. The effects of economic growth are substantial and "the usual procedure for estimating lifetime income based on cross-section surveys tends to produce underestimates because of the failure to take future growth into account."

2. " . . . the impact of growth appears to be greater for young men than for those past the prime working years" [1, p. 834]. These findings are based on Table 2 of [1] in which Miller breaks the relative change in income of given-age cohorts between 1949 and 1959 down into "change associated with age" and "change due to economic growth" and finds that the latter is of a substantial magnitude and that it declines rather rapidly with age.

In Figure 1, $B-A$ is the rise in income of an age cohort over the decade 1950–60. This increase can be expressed in terms of the income of the cohort at the beginning of the decade and broken down into two components:

$$\frac{B-A}{A}=\frac{C-A}{A}+\frac{B-C}{A}. \tag{1}$$

The first component can be regarded as the age effect, as measured in relative terms along the earlier cross-section income-age curve. The second com-

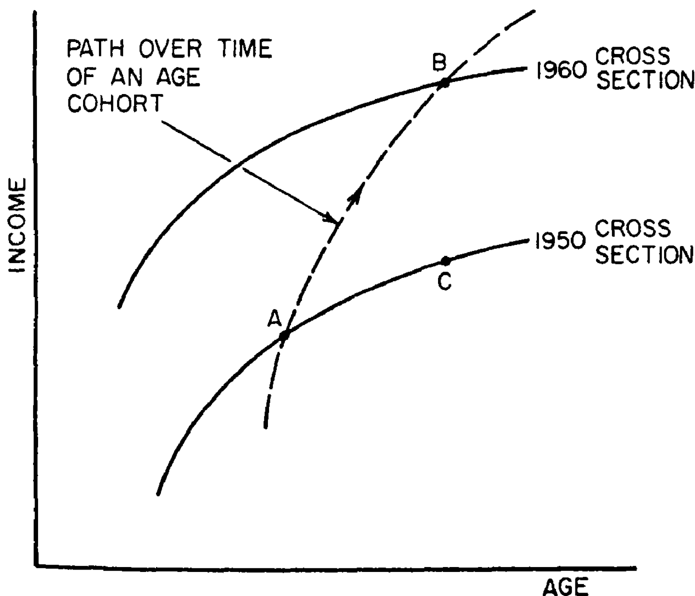


FIGURE 1

ponent is in fact what Miller treats as the "growth" effect. However, a complete breakdown of the relative rise of income of an age cohort over the decade is

$$\frac{B-A}{A}=\frac{C-A}{A}+\frac{B-C}{C}+\left(\frac{C-A}{A}\right)\left(\frac{B-C}{C}\right), \tag{2}$$

where $\left(\frac{C-A}{A}\right)$ may again be designated the age effect, $\left(\frac{B-C}{C}\right)$ may be regarded as a pure growth effect, and $\left(\frac{C-A}{A}\right)\left(\frac{B-C}{C}\right)$ is an interaction component. Miller's measure of the "growth" effect is the sum of the pure growth effect and the interaction component with the following results:

a. His measure is generally higher than the pure growth effect. This is because the age effect as observed in cross-section data is positive and relatively large over most of the span.

b. His measure of the "growth" effect will show a decline with age when the age effect is constant, will generally overstate the decline when it exists, and may understate a rise or even turn it into a decline. The reason is that the age effect, upon which Miller's "growth" effect was shown to depend, declines with age. This is reflected in the concavity of the log income-age curve as observed in cross-section data.[1]

The advantage of measuring the effect of growth by $\frac{B-C}{C}$ rather than by $\frac{B-C}{A}$ is particularly clear in the case where economic growth is neutral with respect to age. If neutrality is defined in terms of unchanging relative age differentials in income (i.e., the log income-age curve as observed in cross-section data slides uniformly upwards) then the growth effect as

[1] R_h is the relative change in income of an age cohort over a decade; R_a is the relative age effect in the cross section; R_g is the pure growth effect which is the relative change in income of an age group over a decade. Miller's measure of the growth effect, R_g^m, is

$$R_g^m = R_h - R_a = R_g(1+R_a). \tag{1'}$$

Differentiating with respect to age, a,

$$\frac{dR_g^m}{da} = R_g\frac{dR_a}{da} + (1+R_a)\frac{dR_g}{da}. \tag{2'}$$

The first term is negative because of the concavity of the cross-section income-age curve. When the pure growth effect, R_g, is constant with respect to age, the second term vanishes, but R_g^m will show a decline with age. If R_g declines with age (dR_g/da negative), R_g^m will decline faster unless the age effect is negative and large. If R_g rises with age, R_g^m can rise faster, or slower, or even decline.

measured by $\frac{B-C}{C}$ will be, in the case of neutral growth, equal for all age groups.[3] $\frac{B-C}{A}$, the measure used by Miller, will however show in this case a decline with age. Miller did not see any inconsistency between his finding that "the impact of growth appeared to be greater for the young men" [1, p. 834] and his own observation that "the variation in income with age based on cross-section data [i.e., presumably relative age differentials in income] . . . has been found to be quite stable" [1, p. 840]. The case of neutral growth is particularly important because only then can we interpret the relation between income and age in cross-section data as the static life cycle. Neutrality is a necessary but not a sufficient condition for unequivocal measurement of the age effect along either cross section curve.

Table 1 presents a comparison between the growth effect as measured here, $\frac{B-C}{C}$, and as measured by Miller $\left(\frac{B-C}{A}\right)$. The first is calculated directly as the relative difference in the income of identical age groups between the two censuses, based on the data in Table 1 of [1]. Miller's mea-

TABLE 1—THE EFFECT OF GROWTH ON INCOME BY AGE, 1949-59[a]

Color, Years of School Completed	"Pure Growth Effect" $(B-C)/C$				Miller's "Growth" Effect $(B-C)/A$			
	35-44	45-54	55-64	(1)-(3)	35-44	45-54	55-64	(5)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Classes								
Total	41	35	38	−3	51	37	35	−16
Elementary:								
Less than 8 years	31	23	26	−5	37	25	25	−12
8 years	29	24	27	−2	34	26	25	−9
High School:								
1-3 years	32	25	32	0	38	28	30	−8
4 years	31	17	22	−9	39	21	21	−18
College:								
1-3 years	30	27	40	+10	45	30	38	−7
4 years	29	18	19	−10	51	20	18	−33
White								
Total	41	35	39	−2	51	36	36	−15
Nonwhite								
Total	55	44	44	−11	57	43	40	−17

[a] *Source and Explanation:* see text.

[3] Alternatively, the effects may be defined in absolute terms with "neutrality" corresponding to a case where absolute age differentials in income are constant through time.

sure is based on columns (3), Table 2 of [1].[8] The pure growth effect is somewhat higher in the age group 35–44 than in older age groups but the decline, where it exists, is much smaller than reported by Miller, and in some cases there is actually an increase with age where Miller shows a decline. It is also interesting to note that the group least affected by growth is the group 45–54 and not the oldest group 55–64.

A discussion of the substantive issue of the relation of the economic growth to the shape of the income-age curve is beyond the scope of this note.

YORAM BEN-PORATH*

REFERENCE

1. HERMAN P. MILLER, "Lifetime Income and Economic Growth," *Am. Econ. Rev.*, Sept. 1965, *55*, 834–44.

[8] Miller's annual rates in his Table 2 are one-tenth of the decadal rates.

* The author is a graduate student in the department of economics at Harvard University. He wishes to thank Professor Simon S. Kuznets and Mr. Gur Ofer for their comments.

Engineering Production Functions and Capital-Labor Substitution in Metal Machining: Comment

I. *Engineering vs. Heuristic Production Functions*

For some time the theory of the firm has been subject to an unfortunate division: in the classroom we talk about production functions involving physical inputs and a physical output; in our empirical work we are content to estimate cost or production functions from value and accounting data.[1] The former have come to be called "engineering production functions,"[2] while the latter have been termed "index numbers of production" [3], "distribution functions" [4], and "surrogate production functions" [13]. Attempts to refine these empirical production functions to bring them closer to the physical production functions of the classroom have led to a mass of literature. The difficulties involve such questions as: Under what conditions may factors be aggregated [5]? Should capital be measured as a stock or service?[3] Should there be corrections for price changes? (If so, should these entail weighting by (1) constant prices, (2) moving averages, or (3) labor time equivalents [15]?)

In an attempt to break out of this dualism and its set of problems, Mordecai Kurz and Alan Manne used "engineering data" to estimate

[1] For a review of the literature, see [17]; for a detailed discussion of the Cobb-Douglas function, see [12].

[2] The definition of an engineering production function is a bit more specific as given by Chenery [1]. Output is a function of engineering variables, i.e., the qualities or characteristics of inputs. Equivalently, an engineering production function is a relationship between a physical output and physical inputs where each input is a vector of characteristics [1, pp. 511–13].

[3] For a discussion of these concepts, see Lave [10, Ch. 10, Sec. G].

capital-labor tradeoffs in the metal-machining industry [9]. The novel feature of the analysis was that output was measured by the numbers of operations (of each particular type) that could be performed on a particular machine during a work day. Since these observations consisted of output in physical units and capital in physical units (labor is accommodating), Kurz and Manne hoped to generate capital-labor tradeoffs free from the problems generally encountered in estimating production functions.[4]

In estimating Cobb-Douglas and constant elasticity of substitution production functions, Kurz and Manne measured capital as the purchase price of each type of machine. Inefficient observations, those inside the capital-labor-output frontier, were deleted from the analysis. In estimating the functions, dummy variables were used to take account of the difference between output tasks. For example, there were five classifications for the size of the piece being machined: very small to very large. These classifications, in the form of 0–1 dummy variables, are shown in equation (1):

$$(1)\quad \log Y = b_0 + b_1 s_1 + b_2 s_2 + b_3 s_3 + b_4 s_4 + b_5 s_5 + \cdots + b_k \log K$$

where the s_i indicate the five size classifications, K is the purchase price of a particular machine, and Y is the number of pieces turned out during a normal working day. (Groups of dummy variables for shape, tolerance, and lot size were also used.) An observation might consist of the number (Y) of pieces of size 3 ($s_3 = 1$, $s_1 = s_2 = s_4 = s_5 = 0$) which were turned out by a machine costing $10,000 during a work day (Y and K are divided by L, the number of men working on the machine). When this equation was fitted to the data, the statistical results were good and the coefficients made economic sense (the implied share of labor in output was compared to Census estimates and the two were quite close).

Eirik Furubotn argued that equation (1) was not an engineering production function since capital is defined in terms of price, not physical units [6]. He also argued that the measure of capital (machine purchase price) is inadequate and " . . . the quantitative results obtained are open to question . . . " [6, p. 515]. Furubotn contended that the analysis could not be corrected (" . . . the censoring rule is invalid as stated by Kurz and Manne, and incapable of useful revision" [6, p. 615]) since the valuation of capital is necessarily subjective depending on the expectations of entrepreneurs.

Is it possible to estimate an engineering relation of the sort defined in the classroom? Just what did Kurz and Manne estimate? How might their procedure have been changed?

II. *Theoretical vs. Empirical Production Functions*

Furubotn emphasizes the distinction between the physical production function of the classroom and the sort of function estimated in practice:

> . . . if a *production function* is to be established, information on prices is not needed. The efficient technical alternatives can be separated from the total array of processes on the basis of objective physical criteria;

[4] Other work along these lines includes [1], [2], [7], [14].

the problem here is technical rather than economic. Needless confusion is created whenever the distinction between these two levels of choice is blurred [6, p. 515].

In practice the technological relationships can not be kept so pure. As Chenery points out, the engineer is faced with a mushrooming set of possible input combinations, owing to the large number of possible materials that are relatively close substitutes [1, p. 509]. Unless alternatives can be ruled out the set becomes so large that there is no possibility of handling it empirically. (For example, a desk might be made of wood, steel, gold, diamonds, or many other materials; if the engineer attempted to consider all materials that would make a feasible desk, the number of alternatives would be too large to evaluate.) The engineer, according to Chenery, rules out inputs by assuming a set of prices. In contrast, the economist limits alternatives by aggregating inputs into two or three sets of factors, e.g., labor, capital, raw materials [1, p. 510].

Each of these simplifications leaves its mark on the estimated relation. The engineer's production function will continue to describe the efficient process only as long as relative prices are approximately constant. The economist's relation depends on each factor behaving as a Hicksian "composite commodity" (relative prices within each factor category must be constant) [8, pp. 33–34]. In addition, all relative technological relations between capital, labor, and raw materials must be essentially constant.

With a large number of inputs, and a moderately large book of technology "blueprints," it is extremely unlikely that a single, reasonably simple function would describe all efficient input-output combinations. Yet, it is just such a simple function which must be estimated in practice. Even neglecting the question of the constancy of the book of blueprints (neglecting technological change), only a part of the technology can be described, and that only approximately. In estimating a production function, one is estimating an approximation; it is important that the estimation procedure tailor the approximation to a particular use. Only if the biases and limitations of the data and estimation procedure are unimportant to the particular context can a useful relation be estimated.

III. *Capital-Labor Tradeoffs in Metal Machining*

To come back to the metal-machining context, the pure, physical production functional can be none other than the entire set of relations collected by Markowitz and Rowe [11]. These data, with which Kurz and Manne began their analysis, describe various physical outputs in terms of the labor and time required to produce them on a given machine; they are derived independently of prices. Unfortunately, the data are not very useful in this form. Without a scheme for aggregating or comparing the various machine tools, there is no way of summarizing the data and they remain 1,143 observations on 115 machines performing 129 tasks. Since these data were collected under the assumption of fixed labor-machine proportions, one cannot even get useful isoquants.

Simplifying assumptions are necessary before the Markowitz-Rowe data

can be used to estimate capital-labor substitution. Machines must be compared and so some sort of price weights are necessary. Kurz and Manne used purchase price of each machine tool, and so their relation gives an estimate of the tradeoff between labor and machine purchase price. This tradeoff is an extremely limited one, as Furubotn points out, since a machine costing one dollar more than another, but lasting five times as long, would be labeled "inefficient" (assuming both give the same output per labor hour). Such a limited tradeoff might be relevant for a firm with an extremely limited amount of funds for purchasing equipment.

To estimate capital-labor tradeoffs, Furubotn argues, present value, not purchase price, is the correct measure for comparing machines. Indeed, one must agree that purchase price is not likely to be a good surrogate for capital's services. But one must specify the nature of the required relation before capital's services can be measured.

Kurz and Manne decided to estimate an optimal efficiency locus rather than an "efficiency in practice" or "average efficiency" locus. This decision simplifies the analysis since it makes the problem of measuring capital more tractable. Which of the two measures (stock or service) will give the better approximation to capital's role and usefulness? Since it is the efficiency locus we are considering, we may assume that capital will be used efficiently. With full utilization and a fixed rate of interest, the two measures will be equivalent except for expectations about the future: what will be the rates of obsolescence, price change, and technological change? Furubotn is correct in arguing that entrepreneurs will answer these questions differently, and so the value of capital will vary between entrepreneurs. However, Kurz and Manne are again free to choose the "correct" or "perfect foresight" expectations since they are estimating the efficiency locus.

The Markowitz-Rowe observations were generated independently of price data; they represent an attempt to derive an exhaustive list of the operations that might be performed on 115 tools. No matter what set of prices is used to compare machines, some of these observations will be inefficient. What place should an observation inside the production frontier have in estimating capital-labor tradeoffs? As in a theoretical treatment, these observations must be deleted before fitting the production relation. However, the censoring rule must be specified carefully and must be relevant to the use to which the resulting estimates will be put. The estimates are likely to be quite sensitive to the criteria of efficiency.[5]

IV. *Changing the Kurz-Manne Procedure*

In an empirical study of the Kurz-Manne sort, a number of assumptions and approximations are required. An essential part of the study involves

[5] The set of "correct" expectations will be manifested in the model by the choice of capital (machine) values. Machines whose values render them "inefficient" will be deleted from the analysis. A machine will be purchased only if its price is less than or equal to the present discounted value of the profit that would be realized by using this machine. Thus, an independent hypothesis emerging from the analysis might be that inefficient machines, ones deleted from the analysis for all tasks, should be left unsold in the market.

testing the implications of the estimates. Kurz and Manne tested their est mates by comparing the share of capital in value added (implied by the estimates) with capital's share as given in the U.S. *Census of Manufacturer* The *Census* data are not pure and so the two " . . . cannot be compare directly . . . " [9, p. 677]; but the figures are comparable and lend som confidence to the estimates.

A more direct way to test the estimated model might have involved as suming a price of labor (the price of capital is already in the model) an getting the implied prices and price differentials of each "task" (performin each machining operation). If the metal-machining industry is competitiv and efficient, the prices implied by the model should be found in the market Even if there is imperfect competition in the market, some of the price ought to be correct (some of the tasks will be priced competitively) and al other prices should be above the predicted ones. Such a comparison (mor than one hundred prices for comparison would emerge) should prove a sensi tive one in testing the twin hypotheses of (a) competition and efficiency in metal machining, and (b) the reasonableness of the estimated model.

The last two comments are related to the Kurz-Manne statistical analy sis. Dummy variables are used to take account of differences in size, shape tolerance, and lot size of the piece being machined. As specified, the mode cannot be estimated since it would give rise to a singular variance-covari ance matrix [16]. Kurz and Manne follow the general procedure of excluding one variable from each of the sets of dummy variables and then using simple least squares. However, they do not note that the coefficients which are estimated will be the difference between the coefficient of each remaining variable and the coefficient of the excluded variable. Changing the excluded variable would have changed the number of "significant" coefficients.[6]

Finally, it should be noted that their estimation procedures are part of a covariance analysis. They have asked questions about the relation between the capital coefficient and the various tasks. Such issues are part of the general question of which groups of observations may be pooled to obtain parameter estimates of wider scope. Questions of interest include: (a) What sort of aggregation across machines or tasks is statistically permissible? In other words, is there a single model that describes all machine tools? (b) Just how many different capital coefficients and different intercepts should there be? (c) Is there really capital-labor substitution between different machines? Covariance analysis would provide answers to these questions.

V. *Conclusion*

The Kurz-Manne analysis represents a path-breaking attempt to estimate characteristics of the production function from engineering data. As with most pioneering studies, there were a number of difficulties and un-

[6] Suppose $Y=b_0+b_1s_1+b_2s_2+b_3s_3$, while $s_1+s_2+s_3=1$. Then $S'S$ will be singular and estimation by simple, least-squares techniques will be impossible. However, substitution is possible: $s_3=1-s_1-s_2$ and so $Y=b_0+b_1s_1+b_2s_2+b_3(1-s_1-s_2)$; which simplifies to $Y=(b_0-b_3)+(b_1-b_3)s_1+(b_2-b_3)s_2$. Thus, one would estimate $Y=a_0+a_1s_1+a_2s_2$. Testing the significance of a_1 is equivalent to testing whether b_1 is significantly different from b_3. If one chose to exclude an s_i whose b_i was quite different from the other b's, all of the a_i would be "significant."

necessary limitations. Some of these were corrected by Furubotn; others are suggested here. In large part the problems stem from the failure to set out a specific question for analysis. It is not possible to estimate a general capital-labor substitution relation for metal machining, even given the existing technology. To render the data susceptible to analysis, some sort of machine prices are necessary, a censoring rule must be specified, and many other assumptions are needed. Since a range of plausible alternatives exists for each of the assumptions, the one actually chosen must be justified as being particularly appropriate to the purpose at hand.

LESTER B. LAVE*

* The author is assistant professor of economics at Carnegie Institute of Technology. While not responsible for any remaining errors, Leland Case, Robert Strotz, Judith Rice, and Robert Eisner provided helpful criticism.

REFERENCES

1. H. CHENERY, "Engineering Production Functions," *Quart. Jour. Econ.*, Nov. 1949, *63*, 507–31.
2. P. DHRYMES AND M. KURZ, "Technology and Scale in Electricity Generation," *Econometrica*, July 1964, *32*, 287–315.
3. E. DOMAR, "On The Measurement of Technological Change," *Econ. Jour.*, Dec. 1961, *71*, 709–29.
4. C. FERGUSON, "Cross-Section Production Functions and the Elasticity of Substitution in American Manufacturing Industry," *Rev. Econ. Stat.*, Aug. 1963, *45*, 305–13.
5. F. FISHER, "Embodied Technical Change and the Existence of an Aggregate Capital Stock," *Rev. Econ. Stud.*, Oct. 1965, *31*, 263–88.
6. E. FURUBOTN, "Engineering Data and the Production Function," *Am. Econ. Rev.*, June 1965, *55*, 512–15.
7. E. HEADY AND J. DILLON, *Agricultural Production Functions.* Ames 1961.
8. J. HICKS, *Value and Capital.* Oxford 1939.
9. M. KURZ AND A. MANNE, "Engineering Estimates of Capital-Labor Substitution in Metal Machining," *Am. Econ. Rev.*, Sept. 1963, *53*, 662–81.
10. L. LAVE, *Technological Change: Its Conception and Measurement.* Englewood Cliffs 1966.
11. H. MARKOWITZ AND A. ROWE, "An Analysis of Machine Tool Substitution Possibilities," RM-1412, RAND, Santa Monica 1955; revised in A. Manne and H. Markowitz, eds., *Studies in Process Analysis.* New York 1963.
12. M. NERLOVE, *Estimation and Identification of Cobb-Douglas Production Functions.* Chicago 1965.
13. P. SAMUELSON "Parable and Realism in Capital Theory: The Surrogate Production Function," *Rev. Econ. Stud.*, June 1962, *29*, 193–206.
14. V. SMITH, *Investment and Production.* Cambridge 1961.
15. R. SOLOW, "Substitution and Fixed Proportions in the Theory of Capital," *Rev. Econ. Stud.*, June 1962, *29*, 207–18.

16. D. Suits, "Use of Dummy Variables in Regression Equations," *Jour. Am. Stat. Assoc.*, Sept. 1957, *52*, 548–51.
17. A. Walters, "Production and Cost Functions: An Econometric Survey," *Econometrica*, Jan.-April 1963, *31*, 1–66.

Engineering Production Functions and Capital-Labor Substitution in Metal Machining: Reply

A point of universal agreement is that the production function of pure theory raises the most difficult problems for empirical work. If the concept is interpreted rigorously, an impasse is reached because the set of efficient technical alternatives for the typical commodity is virtually limitless and, hence, incapable of capture by any practical econometric technique. Professor Lave has emphasized, correctly, the need for empirical studies to focus on what is, in effect, a *partial* production function—i.e., a function defined subject to a series of price conditions and able to show some subset of the efficient alternatives. However, as the Kurz-Manne paper [3] makes clear, the introduction of prices does not eliminate all difficulties; indeed, new problems arise which seem almost as intractable as those from which escape is sought. And it is these latter problems that deserve further discussion.

I. *Alternate Censoring Rules and their Deficiencies*

The K-M censoring rule is misleading because it asserts that, whenever a number of machines have the same output per labor hour, the best choice is necessarily the machine which has the lowest price and involves the least investment outlay.[1] To escape the bias this efficiency criterion imparts to the production model, an alternate rule is needed. One possibility is to base the selection of technical alternatives on a comparison of machine-tool prices with the corresponding present values which can be estimated for the separate tools.[2] The latter approach permits choices to be made from the standpoint of optimization over time. Thus, reasonable weight can be given to factors which operate throughout the whole life of a capital asset and influence its effective economic contribution to the firm.[3]

The use of the present-value concept may help to resolve one difficulty, but immediately raises another. Obviously, the present value attached to any machine tool depends on the expectations held concerning the economic environment in which the input will perform. However, since different observers are almost certain to see future technical and market developments

[1] Thus, any quality of a machine which tends to raise its supply price, without simultaneously altering its one-period productivity, must be viewed as undesirable.

[2] There are, of course, problems connected with the use of the present value approach. In general, machines will involve different investment costs and have different lifespans; thus, additional assumptions about the investment plan are necessary to insure comparability of the alternatives. Further, questions arise about replacement policy and the extent of the planning period.

[3] One implication is that choice of relatively high-priced machines is conceivable. The production function obtained need not be shaped by the odd strategy of investment minimization.

differently, varied estimates of present value are possible for a machine,[4] and the problem of subjectivity appears. It follows that there can be as many production functions as there are entrepreneurs considering investment.

II. *The Optimal Efficiency Locus as a Solution*

The line of reasoning pursued so far leads to rather depressing conclusions about the possibility for deriving useful empirical functions. Lave is more optimistic. His argument is that both the investment-minimization bias and the subjectivity problem disappear when the Kurz-Manne procedure is interpreted as a search for an "optimal efficiency locus." In other words, if we assume continuous utilization of equipment at the technically optimal output rate, a constant rate of interest, and perfect knowledge of all relevant factor and commodity prices over the planning interval, the K-M censoring rule is said to be satisfactory. Presumably, the same machine tools will be selected as efficient whether choice is based on present value or one-period productivity. Subjectivity ceases to be a difficulty because each entrepreneur acts on the same information. That is, each individual knows the correct values of future prices and, if rational, will adjust his production plans accordingly—with the result that uniform behavior emerges.

This defense of the K-M rule is ingenious; it is not, however, completely convincing. Even granting a universe of perfect foresight, there is no reason to believe that machines requiring the least capital outlay will always be optimal. For example, knowledge of future market conditions might indicate that relatively durable, and hence expensive, machines are preferable to less-durable units. While the arbitrary assumption of full utilization of equipment may make adaptability characteristics unimportant, machines possess other properties which have significance for the investment decision. Differences in safety, reliability of operation, ease of maintenance, etc. count for something, and thus conditions may easily be such that greater capital outlays rather than lesser outlays are desirable. One can assert that investment minimization is the best policy to follow in defining the production function, but the perfect-foresight model does not necessarily imply this.

Quite apart from the criticism just noted, there is a serious question concerning the usefulness of a model based on the idea that entrepreneurs are in possession of perfect knowledge of future events. In the limiting case,the economic system would have to be free of both primary and secondary uncertainty. But this conception of the world is purely theoretic. Merely to eliminate secondary, or social, uncertainty would require a complete set of forward markets [2, pp. 141–44], so there is no practical way of approaching the state where all entrepreneurs act alike. The fact is that, by tying such rarefied theoretical conditions to the K-M production function, the function becomes a very specialized construct. And, in consequence, there is a real problem as to where it can be applied. Since the assumptions of the "optimal efficiency locus" will not be met in the real world, the alternatives it shows

[4] The productivity of a machine tool also depends on the precise nature of the technical organization into which it is fitted. See [1].

for capital-labor substitution need not correspond to those seen (and acted on) by the average entrepreneur or government planner. On the other hand, the concept is less general than the conventional production function and, thus, can play no great role in pure theory.

III. *Some General Implications*

If Lave's explanation is rejected, the situation reverts to the one described earlier. We can use the original K-M rule and accept the investment-minimization bias, or consider present values and run into the problem of subjectivity. Moreover, the difficulty noted here transcends the specific case of production relationships in the metal-machining industry; a decision on the type of censoring rule to be followed must be made whenever a partial production function is defined.

The conventional production function, which encompasses *all* efficient alternatives, can be established (in principle) without recourse to price data. By contrast, a partial function demands the stipulation of prices so that a censoring rule can be framed and the field of acceptable alternatives reduced. There is, of course, latitude for the introduction of various price sets. However, the existence of durable capital equipment in productive processes implies the need for multiperiod analysis. And, in general, the information necessary to define a partial production function includes not only the current prices of capital goods but a complete array of factor and commodity prices for all dates to the planning horizon.[5] In short, what is required is just the kind of information which is impossible to know with assurance, and thus a large measure of uncertainty must attach to any production relationship drawn up with the aid of price data.

EIRIK G. FURUBOTN*

[5] A production function is supposed to show the greatest output obtainable from any given input combination. However, in the stock-flow or multiperiod case, both the physical output produced and the types of cooperating inputs used may change over time. Thus, a comparison of input combinations involving capital goods can become quite complex. In effect, dissimilar time streams of outputs (and inputs) have to be contrasted. The only way to evaluate such heterogeneous mixes is to convert all elements to discounted dollar sums; but this kind of undertaking requires the assumption of a multiperiod price structure.

* The author is professor of economics at the State University of New York at Binghamton.

REFERENCES

1. M. GORT AND R. BODDY, "Vintage Effects and the Time Path of Investment in Production Relations," mimeographed, Nat. Bur. Econ. Research, Conference on Research in Income and Wealth, New York 1965.
2. H. S. HOUTHAKKER, "The Scope and Limits of Futures Trading" in M. Abramovitz, *et al.*, *The Allocation of Economic Resources*, Stanford 1959, pp. 134–59.
3. M. KURZ AND A. MANNE, "Engineering Estimates of Capital-Labor Substitution in Metal Machining," *Am. Econ. Rev.*, Sept. 1963, *53*, 662–81.

BOOK REVIEWS

General Economics; Methodology

Essays on Economic Policy, Vols. I and II. By NICHOLAS KALDOR. New York: Norton 1965; London: Gerald Duckworth & Co., Ltd., 1964. Pp. xxi, 293; xxii, 320. $7.50 each; 50s.

The task of the reviewer of Nicholas Kaldor's *Essays on Economic Policy* is simplified by the fact that the author has included his own review of the volumes in two introductory chapters. He explains what the articles are about and why they have been included.

One reason for inclusion is to review the author's intellectual progress from pre-Keynesian economist to a neo-Keynesian Kaldorian. Thus, Volume I begins with the 1935 article, "Wage Subsidies as a Remedy for Unemployment." Kaldor explains that no one in his right mind would now agree with the analysis of the article. It is based on the erroneous premise that it makes sense to speak of the real marginal product of labor in the short run. And, even if that assumption were not erroneous, the real wage could not be equal to the marginal product since, in times of depression, the marginal product is greater than the average product. Keynes in the *General Theory* made the assumptions that Kaldor now rejects, and I continue to find it a useful starting point. Despite the author's disavowal, I continue to admire the article as the best treatment of the subject I know of, notwithstanding Kaldor's new theories concerning such matters.

The second essay, "Quantitative Aspects of the Full Employment Problem in Britain," already published as an appendix to Beveridge's *Full Employment in a Free Society*, must arouse nostalgic memories in the countless economists round the world who were working on the same problem, at the same time, on the same lines. As one of those, I express my admiration for Kaldor's performance. Incidentally, the article is a useful reminder to those who regarded the U.S. tax cut of 1964 as a new discovery in economic analysis. The achievement in 1964 was that the President and the Congress were persuaded to accept the economic analysis as the basis for political action.

Kaldor observes the economic scene with the cold detachment of a Ricardo. While he is presumably attempting to design the kind of world he would like to see, ideology and emotion rarely intrude into the argument. He is a functionalist rather than a sentimentalist. No public tears are shed for the starving poor. A man may be as rich as Croesus provided he invests rather than consumes. In fact, as with Ricardo, capitalist savings are virtually the sole source of private saving. *Landlords are not wicked; they are not functional.* Like Ricardo also, great intellectual power is exhibited with an absence of wit, a solemnity of purpose, and hasty drafting. Spectacular conclusions emerge from the "stylising" (to use Kaldor's word) of facts and premises.

Flattering though comparison with Ricardo must be, Kaldor may not like

it, since he claims to have solved the problem of distribution and growth which baffled the classical economists; and which in his view the marginal productivity theory cannot solve under conditions of technological change. Since this theory underlies many of his policy recommendations, it is incumbent on the reviewer to comment on it. Economists have been saturated with variants and elaboration of Kaldor's theory in recent years. But the central ideas remain unchanged, and presumably the 1958 model, now republished, is still held by its author to be valid.

The classical economists, Ricardo and Marx, said that savings came from profits, and that the profits share in national income determined the rate of investment and, hence, the rate of growth. Kaldor stands them on their heads. He argues that the full-employment rate of investment and, consequently, the rate of growth, is determined by the "technological climate," and is independent of profit rates, interest rates, and wage rates. At the same time wage earners generally save nothing, profit receivers save a fixed proportion of their incomes.

With the economy held at full employment, Keynesian Multiplier processes establish a rate of saving equal to the predetermined rate of investment. Since the multiplier cannot work on output, which is fixed in the short run, it must work on the distribution of income. The argument leads directly to the dramatically simple formulas; $\frac{P}{Y} = \frac{1}{a} \cdot \frac{I}{Y}$ and $\frac{P}{K} = \frac{G}{a}$, where P = profits; Y, real output; K, the stock of capital; G, the rate of growth; and a, capitalists' propensity to save.

In short, the profits share, and, consequently, the wages share and the rate of profits, and the rate of growth are determined by the technological climate and the capitalists' propensity to save.

The consequences of the theory are alarming—if capitalists want to increase their share and the rate of profits, all they have to do is to increase consumption. Their profits will rise correspondingly and the wage earners will pay for their Cadillacs. On the other hand, if wage earners decide to save, their share will be reduced; and the rate of profits will increase. The savings of the workers will go to support not investment, but capitalist consumption. We have learned over the years to accept topsy-turvy economics. But this theory imposes new strains on one's credulity.

I do not mean by these remarks to imply that there is not much of value in Kaldor's theory. In fact, it has much in common with Schumpeter. The innovator cannot, in the nature of the case, base his decisions on close calculation of factor scarcities and marginal productivities. A classical or neoclassical theory is almost bound to underplay the vital effects of innovation and technical change. But recognition of that fact does not require the overthrow of all that has gone before.

Kaldor recognizes, however, that his redistributive process is constrained by lower limits to real wages and profits. Wages cannot fall below "subsistence," and profits cannot fall below the levels determined by "degree of monopoly." He does not show that these boundary conditions are, in general, inoperative.

If they are generally operative, we are back in a classical or neoclassical world. The supply of savings will have an influence on the amount and character of full-employment investment.

Furthermore, to assume a fixed capitalist propensity to consume seems to me wide of the mark. Corporations can and do alter their dividend policy in the light of their investment requirements. Decisions to invest can alter the rate of saving, but they may do this by altering the propensities rather than through bringing about Kaldorian redistributions. In my own work, I am content to assume that the distribution between wages and profits remains fairly constant, apart from cyclical disturbances.

Kaldor uses his theory in its unqualified form to arrive at important policy conclusions. He deduces that an increase in the rate of growth is likely to be inflationary. According to the theory, it will involve a redistribution in favor of capital. Wage earners will then try to recover their former relative position by pressing for money-wage increases, and wage-price inflation will be the result. Most economists would argue that increased economic growth arising from productivity increases has effects on distribution that are not known in advance and, in any case, contributes to price stability by permitting increased money wage increases under conditions of stable prices.

Another application of the theory leads to the conclusion that inflation may be necessary in a slow-growing economy with a correspondingly low rate of profit. If the real rate of profit, determined by the model, is 4 per cent, and the money rate of interest is also 4 per cent, there is no margin with stable prices to allow entrepreneurs to cover risk and uncertainty. In such a situation, Kaldor recommends money-wage increases followed by price increases. For instance, 8 per cent inflation would provide a comfortable margin between the profit rate and the interest rate. While the real rate of profit and the distribution between profits and labor remains unchanged, the inflation permits the entrepreneurs to exploit the rentiers, on a permanent as well as a temporary basis. The question that Kaldor does not answer is how much of this treatment the rentiers will stand. It is quite clear that they will not continue to be happy with 4 per cent.

Kaldor complains (Vol. I, p. 189) that Keynes "who had such a remarkable intuition in this field, was curiously blind to the implications of continually rising money wages for the inducement to invest."

When we turn to Kaldor the tax expert, we return to the classical world, and Ricardo is on his feet again—especially with respect to underdeveloped countries. Savings again become the main determinant of investment and "taxation (and other compulsory levies) provides the most appropriate instrument for increasing savings for capital formation out of domestic sources" (Vol. I, p. 226). Here, the boundaries of the growth model have clearly been reached. In fact, he may well carry his point of view too far. Like many "development economists," he considers agriculture to be a large untapped source of revenue. The record of countries that have attempted to finance industrialization by taxation of agriculture is not reassuring.

In addition to raising resources for development, Kaldor regards progressive taxation measures as a major and necessary instrument of redistribution of

income and wealth "as the only alternatives to Lenin or Mao Tse-tung." Economists are rarely given opportunities to test their ideas in the "laboratory." Kaldor is an exception; his tax proposals have aroused political controversy and sometimes disturbance in India, Ceylon, Mexico, Turkey, Ghana, and British Guiana. As he wryly says, "The power, behind the scenes, of the wealthy property-owning classes and business interests, proved to be very much greater than the responsible political functionaries suspected." Landlords and businessmen had evidently read somewhere that the power to tax is the power to destroy. But the record is still incomplete. Some of them may wish they had heeded Kaldor when they get Mao.

I have not left myself enough space for a detailed review of the essays concerned with international problems. The earlier essays are concerned with the still-unsolved problem of reconciling domestic full employment policies with external balance. Kaldor is far more tolerant of direct import controls and bilateral arrangements than many economists. He does not discuss the possibility of floating exchange rates. His preference is for a scheme of automatic compensatory finance that will permit countries to finance their deficits on a basis that is largely automatic.

The problem that concerned Kaldor at the end of the 1940's was different from the one that preoccupies economists today. He, like many of us, was then concerned with the international consequences of a depression in one or more of the industrial countries, which would reduce the demands of those countries for exports from the rest of the world and hence plague other countries. Such countries would then receive financial assistance to help them cope with events beyond their own control.

The record of the intervening period has shown that deficits arise largely from the inflationary consequences of the country's own internal policies. In that event, the granting of automatic credit could enable the inflating country to exploit the rest of the world. Kaldor was not alone in failing to foresee the inflationary tendencies of the contemporary world, which have resulted in part from the types of policy he recommends. But not everyone republishes his 15-year old essays without modification or reconsideration.

A large part of the second volume is concerned with the alleged economic disadvantages of primary producing countries. Kaldor has struggled valiantly to devise commodity stabilization schemes and commodity agreements that are generally acceptable—with no more success than anyone else. The volume includes a proposal for a commodity reserve currency, prepared jointly with A. G. Hart and J. Tinbergen. It would be inappropriate for me to attempt to comment on this highly interesting proposal in the present review.

Whether or not one agrees, it is an exhilarating experience to observe Kaldor's unquestioned analytical ability applied to concrete problems. But the aspiring policy adviser will feel disconcerted about the power of logical argument to exert a direct influence on the course of events. Most advice is effective only when it goes in the same direction as the political tides. But one has the feeling that some puckish streak in Kaldor prevents him from giving his proposals a fair chance. In his report on Chile, which was presumably intended to influence the Chilean government, he states:

In the circumstances, the general presumption must be that neither from the point of view of economic growth nor of social justice did these [direct economic] controls yield any net benefit as compared with what the situation would have been if the allocation of resources had been left to the blind operation of market forces. There can be few fields of political action where the willingness of governments to prohibit and to prescribe is so out of proportion to the intelligence at their command as to what ought to be prohibited or prescribed. The application of detailed controls in capitalist economies is often no better than quack medicine. There is a great deal of straightforward corruption which goes to nullify the intended effects of detailed physical controls and yet leaves behind an unnecessary amount of social and economic rigidity" [Vol. II, p. 235].

Without question, Kaldor has the courage of his convictions. Whether he is adept at the art of persuasion is a different matter.

ARTHUR SMITHIES

Harvard University

Memoirs of Capitalism, Communism, and Nazism. By CALVIN HOOVER. Durham, N.C.: Duke University Press, 1965. Pp. x, 302. $8.50.

Economists have not customarily written their memoirs. I can recall only a few, of which J. S. Mill's classic *Autobiography,* Alvin Johnson's interesting *Pioneers Progress,* and John Ise's *Sod House Days* come readily to mind, and the last was concerned more with the life of Ise's parents than with his own. This is not as it should be. Lawyers regurgitate their briefs and extol their life in court, while doctors dwell fondly on their past operations. Why shouldn't economists have their say?

The book before us is a nice blend of personal reminiscence, the development of ideas, and description and analysis of the part-time career—but very extensive at that—of a public servant. Hoover came from a low-income family in Illinois—his father was a section foreman on the Minneapolis and St. Louis railway—and his tender years were not very tender. Since the reviewer also spent some time as a section hand he can testify that there is no better spur to the desire to advance oneself than working on a tired railway roadbed under a Midwestern sun. Hoover describes his difficulties, despite the assistance of a devoted family, in getting a high school education and dwells, with forgivable nostalgia, on the three years spent at Monmouth College before he joined the army and sailed for France to participate actively in the last few months of World War I.

After the war Hoover took up farming but soon decided, as so many have before and since, that there are easier and more profitable ways of making a living. One is reminded of that other Middlewesterner, that haberdasher from Kansas City, who also demonstrated that one of the routes to public advancement is inability to meet a payroll. After finishing his undergraduate work at Monmouth, Hoover, now married, decided to do his graduate work at Wisconsin on the advice of one of his teachers that "the economics department of the university was probably the best known in the United States." Here he came particularly under the influence of John R. Commons and Selig Perlman. Although his special field was labor problems, Commons awakened his interest

in economic history, and his first published work, in the *Quarterly Journal* in 1926, was entitled "The Sea Loan in Genoa in the 12th and 13th Centuries."

Before completing his work for the Doctor's degree Hoover accepted a position in the School of Business at the University of Minnesota, where he reports that "Professors Garver and Hansen went a long way in compensating for the deficiencies in my training in economic theory up to this time." Despite this encroachment of orthodox economics, however, one can justly say, surveying the full sweep of Hoover's work, that the Institutionalists of Wisconsin left a permanent impress.

After two years at the University of Minnesota he accepted a call as assistant professor to Duke University where he has served for over forty years. The relation between Hoover and Duke University seems to have been a singularly happy one. The University was extraordinarily generous with leaves of absence for research and government service and, in turn, Hoover accepted his full share and more of University obligations. For 20 years he was Chairman of the Department of Economics and for ten of these years Dean of the Graduate School. Greater love than this hath no man.

Most of the book is devoted to his activities during leaves of absence from the University in the Soviet Union, in Germany, and in the service of the government. Hoover's central intellectual and moral interest has been the life of man under the totalitarian regimes of Communism and Nazism as compared with Western Capitalism. It is a theme that has run through his major books and much of his other writing. It appears in his first book, *Economic Life of Soviet Russia,* reappears in his second, *Germany Enters the Third Reich,* is carried forward in *Dictators and Democracies,* and is stated in mature reflection in *The Economy, Liberty and the State.* The mixture of personal reminiscence and analysis of the political economy of the various regimes he subjected to study makes of his memoires a sort of *histoire raisonée* of the twentieth century.

Hoover's entry into government service came during the New Deal days of the 1930's. He describes himself as an early Keynesian convinced even before the appearance of the *General Theory* in 1936 that "a general deficiency of demand, such as existed during the Great Depression, could be cured by massive creations of purchasing power through deficit financing." In late 1933 he accepted an invitation from Rex Tugwell to become an economic consultant to the Agricultural Adjustment Administration. Here he came in contact with a group of people whose diversity of background, outlook and personality was not to be matched until his later association with the O.S.S.; among them Henry Wallace, Chester Davis, Jerome Frank, Lee Pressman, Fred Howe. In the course of time Hoover took over Howe's position as Consumers' Counsel in which position he reports that his attempts to protect the consumer by price regulation left him with a deep nostalgia for the free market. He returned to Duke in the academic year 1936-37, devoting attention during the next few years to the economic problems of the South and to writing and speaking in favor of preparation for the war he saw coming.

He entered government service again as a consultant to what later became the Office of Price Administration and, shortly after the United States de-

clared war, he joined the Office of Strategic Services. Hoover's life as an Intelligence Agent was more glamorous than that of most economists who temporarily entered that calling. After a few months in the Research and Analysis branch of the organization he joined the Secret Intelligence branch and was put in charge of operations in Scandinavia. Having contributed his mite to tearing down the German economy, he was soon to find himself, under Generals Clay and Draper, fighting off the minions of Morgenthau in order to build it up again.

Although later Hoover was to be called back into intelligence work for short periods and spent some time on the Marshall Plan and later aid programs, he has devoted most of the postwar years to teaching and research. Top management at Duke and his colleagues in the Department of Economics must have breathed a sigh of relief to have their wandering scholar on the ground once more. Hoover, as a matter of fact, was one of the first of that tribe of economists, now so numerous, who divide their time between public affairs and university life. In many cases scholarship suffers heavily or, if not that, attention to University affairs. Hoover, however, managed to publish eight books and innumerable articles while carrying more than his share of the load of academic administration. And no one can deny that his varied and colorful career has provided material for memoirs substantially more captivating than a more sedentary life would have been likely to generate.

EDWARD S. MASON

Harvard University

The Politics of Bureaucracy. By GORDON TULLOCK. Washington: Public Affairs Press, 1965. Pp. 228. $4.50.

Gordon Tullock's book is an analytic study of the internal workings of hierarchical organization, purporting to refer to governmental and business units alike, but drawing most of its factual illustrations from his close experience with the U.S. Foreign Service. The work presents an informal model of hierarchy. Roughly the first half has a preponderantly positive role: how such organizations operate. The second half has a more mixed but preponderantly normative role: why they operate inefficiently, and what can be done to improve performance. The distinctive approach of the book is to assume that each member of the hierarchy is a rational utility-maximizer, and to examine how this affects the setting of work goals at the different levels of the hierarchy and the degree of fulfillment of these goals. The analytic unit is not the multiple pattern of a table of organization but the simple dyadic relationship —taken one at a time—of inferior to superior, superior to inferior, and peer to peer within the hierarchy. The approach carries the economist's method of the individualistic decision-making context into organization theory. This is part of a recent general stirring of interest, marked by the efforts of Anthony Downs, Buchanan and Tullock, and the present reviewer among others, in applying the general methods of economics to the explanation of political and other nonmarket forms of decision-making.

The tenor of Tullock's conclusion is rather pessimistic. Because of individualistic self-seeking throughout the organization there is likely to be

significant slippage between what is the ostensible function(s) of the organization and what actually goes on. Only in an ideal, unrealizable state will an organization be efficient—i.e., will its rewards and punishments and operating procedures be structured so that individual interests dovetail to achieve the leader's formulated goals. To a greater or lesser extent in real-world organizations, for every dyadic relationship self-interest will generate a divergence between the behavior of an inferior which maximizes his superior's utility and that which maximizes his own utility. Therefore, detailed supervision at every level is critically necessary for tolerable performance, since good faith or intrinsic teamwork cannot be assumed. But such supervision is generally severely hampered by the unavailability of dependable evaluational shortcuts and the excessive costliness of what cumbersome observational methods are available. So supervision is allowed to be inadequate and so an organization will be inefficient.

Worse than this, it is not simply that the cost of achieving organizational objectives will be higher than they "have to be" (in some sense), certain types of objectives can not be realized by hierarchical organizations at all! The more intricate and extensive is the coordination of activities needed to achieve the objective, the more inefficient will the hierarchical form of organization be, since coordination demands supervisory relationships, and each such relationship results in slippage-error. Moreover, the errors of one supervisory level are cumulated at each subsequent level. The more levels of coordination that are necessary, the greater is the multiplier of cumulated error. Some complicated tasks—and Tullock makes it clear that he believes this applies extensively to present governmental objectives—engender so much cumulative supervisory slippage that they literally cannot be achieved by hierarchical organization, no matter how large the organization. It rests with decentralized modes of decision-making, such as the market, to accomplish such tasks if, indeed, they should be tried at all. (In the last section of the book, however, Tullock does consider supervisory methods that could improve efficiency somewhat.)

Tullock's results are not only disturbing, they are highly perplexing. Economic resource allocation theory suggests that a desideratum for internalizing certain processes under a "single" decision-making roof—a hierarchical organization—instead of leaving them external to an enterprise with a relationship based on periodic market transactions, is the need for intimate extensive coordination or integration with other processes operated by the enterprise. In other words, the need for coordination makes hierarchy more attractive relative to the market, not less. The key to the anomaly seems to rest in Tullock's misleading identification of coordination with supervision. It is not the *coordination* of separate contributions that is necessarily erroneous, it is the inability of the supervisor to be objectively certain that *each* or *any one* of the *contributions* is what he really wants. To be certain he would have to *duplicate* the work of each of his operatives. It is this duplication for each and every component that is administratively impossible. Tullock's argument about *coordination* is misplaced.

How serious is his argument about supervision? It stems from the argument that inferiors can have incentives to perform their tasks in ways that diverge from what their superiors want yet without their superiors knowing it. (The argument that they are giving superiors what they want but which diverges from the *organization's* objectives simply shifts us to the question of why these supervisors then do not perform *their* tasks in ways that conform to what *their* supervisors want, which is the logical structure of the issue we are considering.) It is one thing to argue the hypothetical existence of such an incentive, it is another thing to establish the circumstances under which the incentive will be acted on and the systematic manner in which it will be employed. Without these it is extremely difficult to know how seriously to take the argument.

We do have treatments in the literature about systematic management incentives for nonprofit-maximizing behavior in large corporations, e.g., overemphasis on growth, conservative investment patterns, excessive liquidity. But Tullock does not help us here. A real weakness of the book is that he is constantly *asserting* the significance of inefficiencies, but without giving a careful analysis of its conditions and directions. Only toward the end of the book in the somewhat different context of supervisory improvement can some of these issues be read between the lines. They are not systematically or deeply developed in the positive analysis. Most of the illustrations he gives refer to the U.S. State Department. These may be true, and, indeed, attention to the special reasons why such inefficiencies should be expected to appear in this sort of bureaucracy would enhance the model. But Tullock does not carry the *analysis* (as opposed to the illustration) this far. If he had, and had called to our attention the extreme vagueness of the "output" of this "enterprise" and its highly unusual "production function," he would be unwilling to use it as heavily as he does as the example for bureaucratic inefficiencies in general.

There are grounds for believing that slippage is not nearly so serious as Tullock insists. The main reason adduced by Tullock for slippage is the inferior's desire to please his superior when the latter's lack of information or preconceived opinions would be disturbed by an honest decision. But surely reality-testing is not so far away for the superior in most organizations (*pace* State Department) that it is to *his* interests to be and remain importantly in error by encouraging his inferiors to make him vulnerable with respect to *his* superiors. In the absence of such problematic bullheadedness from above, there are important payoffs to the inferior to perform his job well. And this subject represents another weakness of the book, since, while so much is made to depend on rewards and punishments for each participant, the character of these stimuli is inadequately treated. (One of the results of this is an incomplete treatment of Parkinson's Law.) Almost no attention is given to the internal reward from seeking to do a good job. One is almost made to believe that an employee will deliberately add up every column of figures wrongly unless his supervisor is checking him over his shoulder. Similarly, Tullock discounts the use of persuasion by superior to inferior to underscore the goals of the organization. Empirical studies disagree. Meaningful participation, praise from

one's superior, and the harmonious relationship which these entail, are chief ingredients of "good working conditions"—surely an important reward for directing human effort.

Tullock's book is peppered with items designed to irritate readers who do not share his deep suspicion of government in the modern world, as well as being based on a misleading distinction which identifies the term "government" with "monopoly" and "hierarchical organization," and "economic" with "pure competition" and "market transaction." Notwithstanding these irritations and the limitations on analytic scope referred to earlier, the book does lay a provocative groundwork for, and provides some lively insights into, the study of bureaucracies. While it swaggers and poses a bit like Machiavelli and Castiglione, it beckons us to follow in with some of the heavier—but also more ponderous, alas—ordnance of modern analytic discipline.

JEROME ROTHENBERG

Massachusetts Institute of Technology

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

The Stationary Economy. By J. E. MEADE. Chicago: Aldine Publishing Co., 1965. Pp. 238. $6.00.

According to the cover description, *The Stationary Economy* is intended to be "volume one of principles of political economy." J. E. Meade's treatment is unconventional and one must be careful to note just what is being attempted. The intentions are stated quite clearly on the cover: "A modern statement in the classical tradition, elegant and rigorous, of the basic concepts of economic analysis," and in the Preface: "There is thus a crying need for attempts to translate, as far as this can be done, precise mathematical results into less technical terms and to relate all the branches of economic analysis to each other" (p. 8). Meade delimits his framework sharply: "It [this first volume] is based on models of economic systems in which there are no capital goods, and in which consumers' tastes, technical knowledge, and the size and composition of the population are static" (p. 8).

Thus Meade states his intentions. The series is to be a comprehensive treatment of modern theory as it applies to the classical tradition. This volume represents an attempt to integrate and explicate (in clear and nonmathematical terms) those parts of modern theory having to do with the stationary economy (with no capital goods).

"A major function of our analysis in this volume will be to explain what economic efficiency means and to show how in the conditions now assumed an equilibrium based on the free operation of the price mechanism will achieve an economically efficient use of resources. But it will also be a major function of our analysis in this volume to point out that this 'efficient' equilibrium may also be a very 'inequitable' one" (p. 33). Beginning with the individual in the market place (the theory of consumer behavior), Meade goes on to describe market equilibrium. Then he develops, expertly and elegantly, the terms of

trade between shops and farms (under the assumption that total goods are fixed, but income distribution changes). The next step is to hold income distribution constant and vary the production of consumer goods, under conditions of constant cost (consumer tastes must have changed). The following six chapters deal with various cases of fixed factors and increasing cost; the last two of these chapters make a brief excursion into changes in technical knowledge (Meade assumed that technology and tastes don't change) and into fixed coefficient production functions. Chapter 12 is concerned with the efficiency and welfare issues surrounding the distribution of income. The last three chapters deal with planning in a socialist economy.

Perhaps Meade attempts too much in a short book. Certainly, he can do no more than introduce most of his topics. Unfortunately, there is no attempt to refer the reader to other works for further study: except for two casual citations in the Preface, no citation or reference appears in the book (there is not a single proper noun in the Index).

Little attempt is made at a comprehensive statement of the body of microeconomic theory. For example, in discussing the derivation of the consumer's preference map, there is no hint that various consistency conditions are necessary before the preference points can be integrated into a two or greater dimension iso-utility surface (the problem that caused Pareto so much concern). It seems strange (in a principles of economics) that, in examining income and substitution effects, Meade presents the method developed by Hicks, but completely neglects Slutsky's analysis. One searches in vain for modern consumption theory (beginning with the theory of revealed preference) or modern production theory (e.g., maximization under constraints and duals). Both of these areas would seem to be part of the analysis of the stationary state—certainly both could use a clear exposition. Does Meade succeed in translating and integrating modern theory? If so, the success can be only a limited one since Meade seems to have chosen to ignore many of the results of the last 35 years.

Meade states his intention of translating results into less technical terms. One will search in vain for a derivative, either in text, footnote, or appendix; one will search in vain for any mathematical statement more complete than a simple algebraic equation or geometric illustration. This framework imposes a distinctive character on the book. Of necessity, the arguments are closely reasoned and often tedious; they must be kept simple, with the focus on two, or perhaps three, variables at a time. In view of the importance of the geometry, it is unfortunate that the figures are never titled, the axes are not clearly labeled, and sometimes not labeled at all.

And yet in one sense the book has succeeded. The central argument is clear and there are few distractions presented to the reader in this logical progression from the individual to the planned economy. ". . . one should be extremely frank about the assumptions which are being made at each stage for each model. This will unfortunately make many students despair of a discipline which appears so unrealistic. But this risk must be taken. Any other course means that the economist bamboozles himself as well as his students" (p. 23). "The art of political economy is to choose models which combine simplicity with relevance to certain important features of the real world. The economist can never be sure that he knows the answer" (p. 23). One might question the

relevance of the analysis (since capital, imperfect competition, and changes in taste and knowledge are excluded), but one cannot question the quality of the exposition.

The book seems unlikely to displace the line of intermediate or advanced microeconomic theory books presently available—the lack of citations alone would insure that. Yet its potential usefulness is quite large. For the student who is confused, or a bit too glib with his sophisticated tools, the book will help gain a real understanding of the methods of economic analysis. Yet, despite this attempt to "translate precise mathematical results into less technical terms," one cannot help feel "But mathematics is helpful."

LESTER B. LAVE

Carnegie Institute of Technology

A Scheme of Economic Theory. By G. L. S. SHACKLE. New York: Cambridge University Press, 1965. Pp. xi, 209. $7.50.

Professor Shackle has written a book of essays on the following subjects: general equilibrium, Marshall's use of short and long periods, Keynes's employment theory, Harrod and Domar on growth, Hicks on the trade cycle, the Austrian theory of capital, and the Leontief input-output model.

The string on which these themes are threaded is the notion that different concepts of time are used in the various theories. The differing treatments of time, though each legitimate for its own end, form, nevertheless, a hierarchy of sorts, the highest in the firmament being those which take seriously the problem of decision in an uncertain world.

I confess myself unable to grasp Shackle's concept of different time-concepts. But one can agree that theories vary in their assumptions about, and emphasis on, expectations and uncertainty. This seems to be the nub of the matter.

The essay on Marshall is perhaps the best. Shackle attributes to Marshall, as his "daring innovation" (p. 28), the use of equilibrium to analyze processes of change; for he took the classical idea of market price adjusting to natural price and transformed it into a theory of economic evolution. His method was to interpret the "period," whether short or long, as something seen *ex ante* by the businessman. The long-run normal expectation, though constantly influencing business behavior, is itself constantly evolving under the pressure of events, so that the classical stationary state is never attained.

Keynes inherited Marshall's use of the equilibrium method but, more impressed by the uncertainty clouding the future of business, thought of long-run expectations as so liable to sudden catastrophic shifts that no formal explanation of them could be attempted. In Keynes they become nothing more than a highly unstable parameter of the short-run equilibrium. Shackle's exposition of Keynes is praiseworthily explicit about expectations. But it might have been clearer to the uninitiated if the propensity to consume had been formally introduced. We meet the marginal propensity, but are not told that it is the derivative of consumption as a function of income.

Shackle seems to have missed an opportunity to relate Harrod's concept of warranted growth to the Marshallian idea of evolving expectations. He has an

unfortunate misconception of the meaning of C in Harrod's Equation, $GC = s$. He thinks of it as a *technically required* capital-output ratio, not as an *ex post* quantity. He is therefore at a loss (p. 108) to say what extra function is performed by the warranted growth equation, $G_wC_r = s$, and what can be meant by a difference between C and C_r, on which Harrod's instability thesis depends (p. 115).

Turning to the equation for natural growth, $G_nC_r = s$, we are told (p. 122) that if G_w is above G_n, growth at G_n cannot be maintained. There will be a cumulative slump. On the other hand if G_w is "far below" G_n, involuntary unemployment will appear and quickly grow. We are in for a cumulative slump either way, then.

Hicks is praised for his ingenuity, but gets low marks for having the accelerator, still lower for having autonomous investment, growing at the natural rate, and lowest of all for having a self-contained cycle model. Apparently investment is so unsystematic *in principle* that it is hubristic to build a model in which investment is a determinate function of other variables in the system.

Shackle's dislike of closed dynamical systems is partly philosophical. They are "an abstract representation of perfect determinism," and are unfavorably contrasted with Keynes's "conception of the unpredictable caprice of things" (p. 185). The nature of his anxiety is not altogether clear. It seems not to be such that a stochastic reformulation would allay it. Yet he must be aware that no prediction is possible unless the model is complete, that even the predictions of a complete dynamical model are conditional statements, and that predictions can be of value notwithstanding their "falsity," because they may be sufficiently accurate for the purpose in hand.

He also dislikes *specific* dynamical assumptions, on the ground that there is always an infinity of alternatives to choose from. But is not this as true in statics as in dynamics? Are we without criteria for choosing between alternative assumptions, whether statical or dynamical?

This is not a book brimming with new ideas. The style is heavy with simile and metaphor, and is not always perspicuous. It makes somewhat exhausting reading. But the student may derive benefit, provided that he undertakes considerable preliminary study of the theories elsewhere.

H. ROSE

University of Rochester

The Economics of Inflationary Processes. By K. K. F. ZAWADZKI. New York: Frederick A. Praeger; London: Weidenfeld and Nicolson, 1965. Pp. 288. $10.00; 50s.

In an article published a decade and a half ago Ralph Turvey[1] distinguished four basic types of inflation:

	Prices		Wages
Type A	Prices:	Cost-determined;	Wages: Cost-determined
Type B	Prices:	Flexible;	Wages: Cost-determined
Type C	Prices:	Cost-determined;	Wages: Flexible
Type D	Prices:	Flexible;	Wages: Flexible

[1] "Some Aspects of the Theory of Inflation in a Closed Economy," *Economic Journal*, Sept. 1951, 531-43.

According to Turvey inflation is "a process resulting from competition in attempting to maintain total real income, total real expenditure and/or total output at a level which has become physically impossible, or in attempting to increase any of them to a level which is physically impossible" (p. 534).

K. K. F. Zawadzki's book, as the author readily acknowledges, is devoted largely to spelling out the implications of Turvey's models. Zawadzki's treatment has several distinctive features. First, it is highly abstract from beginning to end. The book contains no numbers, no references to specific countries or inflationary episodes, and only a bare minimum of special institutional assumptions. As a result the reader learns nothing about the relevance of the various Turvey-Zawadzki models to the real world. Second, despite its abstract and intricate nature, the exposition makes no use whatever of algebra or geometry. Fortunately the author writes in simple straightforward English and has organized his complex material with great care. Nevertheless the end product is neither easy to grasp nor, in my judgment, definitive. Third, except in a short introductory chapter, this book contains only a few skimpy references to other work. The literature on inflation, of course, is vast; in their survey article (*AER*, September 1963) Bronfenbrenner and Holzman cited nearly 200 contributions from the postwar period alone. Perhaps. the greatest lack in this area is an integrative treatise. Zawadzki's book does not meet this need.

But let us turn from the omissions—after all, only the author can determine the scope of his work—and examine the book on its own terms. Chapters 2 to 9, comprising nearly half of the volume, deal with inflation in closed stationary economies. Each of Turvey's types of inflation is given a name and analyzed in some detail.

In *struggle-for-income inflation* (type A) both prices and wages are cost-determined; they adjust to the cost levels prevailing at the end of the preceding period. In *excess-demand inflation* (type B) prices adjust immediately to changes in demand, while wages follow with a one-period lag; in *labor-shortage inflation* (type C) the reverse is true. Both prices and wages are flexible in *fully open inflation* (type D). The discussion proceeds by taking up the various types of inflationary shocks that may initiate each inflation. Zawadzki then examines "the factors and forces which may make inflationary processes tend towards a stable equilibrium at a finite price level" (p. 79), the speed of inflation, and the effects on relative prices.

The flavor of the argument can be apprehended if we take a quick look at the discussion of struggle-for-income inflation. The initial shock may take any of four forms: "(a) an autonomous wage increase resulting from the dissatisfaction of the workers with their real income position; (b) an autonomous increase in gross profit margins resulting from the dissatisfaction of the entrepreneurs with their real income position; (c) a decrease in the average productivity of labor; (d) an autonomous increase in costs other than labor costs" (p. 44). Unit costs rise under (a), (c), and (d) and therefore cause prices to rise in the next period; prices rise at once under (b). Since wages are cost-determined, they rise further as soon as prices rise; and so on. But the process is not likely to continue indefinitely. Either wages or prices may not

adjust completely to cost changes; there may be income redistribution toward high-saving groups; progressive taxation may generate a budget surplus for the government.

Zawadzki recognizes that a process of this type requires either a passive monetary policy or a passive velocity of circulation to finance the augmented money flow and he assumes without further ado that somehow this will come about. It is precisely here that many readers will begin to lose interest, for there is little reason to believe (at least in the United States) that either the supply or demand for money will passively accommodate such an inflationary process. Nor is this difficulty unique to struggle-for-income inflation.

In Chapters 10-14 Zawadzki turns to the analysis of inflation in open economies. He assumes that the external effects of inflation will be continuously offset by exchange depreciation. The problem to be explored is under what circumstances inflation permanently worsens or improves an economy's international position, as gauged by changes in its terms of trade. The results are rather inconclusive, and I shall not attempt to report them here.

The final four chapters deal mainly with the interrelationships between growth and inflation. Zawadzki's analysis of growth, in accordance with the modern vogue, focuses exclusively on investment in nonhuman capital. Hence the effect of inflation on growth depends on how it affects the propensity to save and the inducement to invest. Zawadzki believes that income redistribution because of inflation is extremely unlikely to increase the propensity to save since wages are apt to gain relative to profits. Leaving redistribution aside, Zawadzki argues that personal saving propensities are likely to fall, but that business saving, as a proportion of profits, will probably rise enough to produce a net rise in the propensity to save for the economy as a whole. The effects of inflation on the inducement to invest are harder to analyze, but Zawadzki believes the net impact is more likely to be favorable to growth than not. On the whole Zawadzki gives some very cautious support to those who believe that inflation will stimulate growth. But he ends his book with the thought that there are "no grounds for the belief that a mild inflation is a simple and costless vehicle of economic progress" (p. 283).

RICHARD T. SELDEN

Cornell University

Die Grundlagen der Theorie der Preisdifferenzierung. By ULRICH W. ELLINGHAUS. Tübingen: J.C.B. Mohr Verlag, 1965. Pp. 275. DM 40.

Practical experience led the author to the conclusion that traditional price theory cannot include certain price strategies which play an essential role in modern economies. Therefore, no satisfactory explanations or guidelines, applicable to the real world, can be offered. Ulrich Ellinghaus is of the opinion that with the help of new analytical methods it will be possible to arrive at equilibrium conditions which are of greater practical value for a world in which *human beings* produce and consume. The traditional theory assumes that economic subjects establish contacts among themselves only through markets and with conflicting interests as the prime motivational factor. However, in reality, many types of individual and social relations exist, and a degree of

interdependence creates a common economic interest. Price format influenced by such circumstances and the reaction of individuals to ther economic process cannot be explained by models in which a specific be is axiomatic. Economic behavior is based on certain social structures must become a part of such models.

The new methods which the author wants to introduce and to apply problem of price differentiation are game theory and economic morpl He selected price differentiation because he sees in it an important as the circular flow within the economy. Accordingly, the first three chap the book are devoted to definitions of terms and methodological que Only the last two chapters deal with price differentiation specificall

According to Ellinghaus, price differentiation exists when different are demanded and paid for the same economic good or service during o gle period of time and at the same location. Differences in prices cau transportation costs are excluded. The term "price discrimination" is re as a *terminus technicus,* for special and exceptional cases. The purpose nomic models is explained, followed by a careful description of various of models. Special attention is given to the general equilibrium concept the contributions of Walras and Pareto. The production and distributi pects are used to compare equilibrium models with input-output relatio game theory which is described in some detail.

Ellinghaus comes to the conclusion that game theory together witl phological methods alone make it possible to include the conflict of indi decisions in the analysis. Of particular interest is what the author has about economic morphology. Whereas models are used for abstract isola typical symptoms, morphology is concerned with micro- and macroeco structures and forms. However, it is not sufficient to describe and co these structures, but it is essential to expose the causality of their exi and their effects on economic behavior. Very interesting are the author cussions of morphological concepts found in Euken's and in Weisser's a cal works. The author also relates morphology to classical and neocl theory. Fully realizing the usefulness of these studies, he still finds th automatic consequences are only the result of the fact that many variabl axiomatically eliminated and fixed. But in real life they remain variabl the behavior of the economic individual does not depend upon rational sions alone, but also upon ideals, ethical attitudes, temperaments, trad etc.

Making use of game theory and including morphological methods inghaus analyzes price differentiation first in the microeconomic setting conditions of monopoly, oligopoly, and regional price formations. I framework of partial analysis he describes the processes of price forn and distribution. Various forms of price differentiation are introduce supported by instructive examples which are used in typical models. Tl thor shows which of these forms can be chosen under different market tures to reach optimality in terms of various goals. Finally he describes profit, quantity, and cost problems of price differentiation. But Ellingh not interested in the purely microeconomic concepts alone. In the last cl

he turns to macroenconomic aspects and offers a theory of price differentiation for both free-market and centrally planned economies, enriched by the use of the new analytical instruments. However, he is unable to produce at present a complete model (Totalmodell).

This, no doubt, is a remarkable attempt to expand the traditional price theory and to produce more satisfactory explanations for certain price strategies in modern structures. An English translation of the book wuld certainly prove useful. Unfortunately, it will remain the only contribution of this young and promising economist, who died from the consequences of a traffic accident.

LUDWIG H. MAI

St. Mary's University

Life of Adam Smith (1895). By JOHN RAE. With an introduction "Guide to John Rae's *Life of Adam Smith*" by Jacob Viner. New York: Augustus M. Kelley, Bookseller, 1965. Pp. 145, xv, 449. $20.00.

A reprint of a book as well known as Rae's *Life of Adam Smith,* as welcome as it is, would not normally call for a review. In the case of this volume, it is not the reprinted material but, rather, the scholarly introduction provided by Jacob Viner that calls for reviewing. While the stated purpose of the "Guide" is to " . . . facilitate for the present-day reader the use of John Rae's *Life of Adam Smith,*" it is more than what is implied by such a description. It is a critical and comprehensive survey of the progress of our knowledge concerning Adam Smith's life since the first publication of Rae's work. Apart from a new and definitive biography of Smith, nothing could be more helpful to the student of Adam Smith who has, in the past, been forced to check and cross-check the many books and articles among which recent (since 1895) discoveries about Smith's life are spread. In addition, however, to having brought existing facts together, Viner's incisive logic throws considerable light on the interpretation of these facts.

The "Guide" is divided into five sections, the first four of which, on grounds of both length and organization, can be called chapters. The fifth is a two-page chronology of Smith's life, followed by a brief bibliography of his publications. This, in turn, is followed by an index of names.

The first section, entitled "Introduction," opens with an extensive discussion of the difficulties of biographical research about persons living in the eighteenth century in general and Adam Smith in particular. The second and concluding part is a thorough outline of the major sources from which Rae gathered his material and from which more recent scholars have drawn further information.

The second section discusses the relationships between Smith and a number of his contemporaries. Rae's interpretations of these relationships are supplemented and corrected with the help of new discoveries by numerous scholars and by Viner's clever reinterpretation of the facts.

The third section discusses several aspects of Scottish economic life in Smith's time, i.e., the economic state of the Highlands, the licensing of physi-

cians, pin-making and nail-making, slavery in the Scottish mines, and his views or his apparent lack of concern with the problems involved. Although the entire section is interesting and not without importance, the discussion of the Highlands and slavery in particular seem rather distantly related to Rae's *Life*.

The fourth section, on Adam Smith's library, argues convincingly that a large number of books from Adam Smith's library have never been identified and that arguments concerning influences on Smith that are based upon the absence of a book or books from the lists of those known to have been in Smith's library can have little validity.

The Smith chronology and bibliography that make up the fifth section are references that the student of Adam Smith will find many occasions to use.

Many students of Adam Smith will welcome the opportunity to purchase a copy of Rae's *Life*. Even those with a copy of the original edition will find that Viner's introduction is worth the price of the reprint.

JAMES A. GHERITY

Northern Illinois University

Economic History; Economic Development; National Economies

The Cambridge Economic History of Europe. Vol. VI, *The Industrial Revolution and After: Incomes, Population and Technological Change*. 2 Vols. Edited by H. J. HABAKKUK and M. M. POSTAN. New York: Cambridge University Press, 1965. Pp. xii, 601; xii, 437. $19.50 the set.

Somewhat like the Holy Roman Empire, which has none of the attributes of its title, this book is not restricted to Europe, to industry, or to after, but deals also with the world, with agriculture, and before the Industrial Revolution. This is misleading, but perhaps unimportant. What makes a greater difference is the permissiveness of the editors in giving their contributors free rein unquestioningly. The consequence, as in most symposia, is a highly uneven performance. Douglass North polishes off the industrialization of the United States in 32 pages, and offers a bibliography of 2½ pages. Landes writes an essay on technological change and development in Europe, 1750-1914 (right time, right place), but covers a myriad of topics beyond the relationship of technology to development and spins out 330 pages and 63 pages of bibliography. At the same time, if the substantive-importance-per-page ratio be computed, it is Alexander Gerschenkron who patrols the opposite boundary from North, with his 93 pages plus 3½ pages of bibliography, on whether the emancipation of the serfs in 1861 was or was not a contribution to the industrialization of Russia.

This is, by and large, the old economic history. There are two meanings given to the new economic history, one, that it uses econometrics, and the other that it is concerned with the testing, however primitively, of economic theories, and especially, over the long period, theories of economic development. This work is excluded more from the former than the latter. The first essay, by W. A. Cole and Phyllis Deane, produces a Kuznets-type survey of

the Growth of National Income in the world, but there is only a handful of tables, and the emphasis is on description, rather than measurement. Elsewhere in the testing of theories the views of Gerschenkron on backwardness, H. J. Habakkuk on the impact of factor prices on technological change, Albert Hirschman on linkages, Innis on staples, W. A. Lewis (or Marx) on growth with unlimited supplies of labor, W. W. Rostow on stages, and Schumpeter on entrepreneurship all come in for attention but it is rarely systematic.

As the old economic history, most of it is very good indeed. G. C. Allen's essay on the industrialization of the Far East is concise on Japan, if perfunctory on China, India, and South-East Asia. Largely descriptive, it emphasizes the contrast among the Japanese (imitative), Chinese (scornful), and Indian (passive) responses to Western industrial penetration. Equally descriptive are Glass and Grebenik in their essay on world population, which offers little or no demographic explanation, but compresses much fact into small space; Youngson on the opening up of new territories, who, however, organizes much of his material about an implicit Innis model of the differing significance for settlement and development of different staple commodities; Girard on transport; and Portal on the industrialization of Russia. The Girard piece is a brilliant example of the old economic history with the interesting generalization that new forms of transport—toll roads, canals, steamships, railroads, airplanes—start out as complements to the existing major forms, gain efficiency and compete on a wide front, succeed in substituting, but finally as quasi-monopolists raise prices to the point where still newer forms of transport are encouraged. (This may be old economic history, but it contrasts favorably with some of the new in which cost functions for competing forms of transport are *assumed* to be infinitely elastic, from which it follows, after a formidable analytical exercise, that the railroads had no impact on the economic development of the United States. Where competing activities have constant costs, any one of them is redundant—by assumption.) Girard's essay is also studded with sweeping generalizations of the sort that the design of a transport network may be unrelated to economic activity, but its maintenance is so related; that the transcontinental railroad altered the world political balance, which until then had favored thalassocracies; that cheap transport by making the masses mobile made democratic civilization possible.

North apparently did not take his assignment seriously. There is some analysis of declining transport costs and merging of markets, forward and backward linkages, economies of scale in a given process which led to disintegration (owing of course to diseconomies of scale in administration which are not mentioned). Some of the analysis of comparative advantage and location theory is either wrong or badly expressed. On the whole, however, he is content to offer a pedestrian account, of which an extreme sample can be culled from page 697:

> Iron and steel played a prominent part in the manufacturing development of the American economy during this period. The major technological developments were the Bessemer and the Open Hearth processes. These were followed by a whole host of innovations, in bigger and better blast furnaces, improved rolling mills, and a vast array of new techniques which increased

> the size and efficiency of the steel industry and made steel a substitute for almost all the uses which iron had previously served.

While it is an outstanding example of tepid prose and lame analysis, it is a little unfair to pick this passage which may have been intended to serve as a summary introduction to what followed. North's predilection for quasi-quantitative words in this essay, however, is illustrated by his repeated use of the word "important" which occurs four times each on pages 684, 685, and 689, and six times on page 687.

The editors admit, or possibly even boast, that there is no necessary agreement among the contributors. This is illustrated effectively in the Lewis model of growth, and the extent to which the industrialization process depended upon a steady supply of labor from the farm. Landes states that the Lewis model is applicable to Britain only with major modifications (p. 346n.). In his careful discussion of European agriculture Dovring focuses on population, adopting the Lewis view that retention of surplus labor on the farm harms both industry and agriculture. In contrast to the recent views of T. W. Schultz and others, he believes in disguised unemployment, and suggests that growth in agricultural productivity through investment must wait on its removal. Portal starts out his essay with a half-sentence exactly contrary to what Gerschenkron has taken broadly 100 pages to disprove, though he later modifies this in Gerschenkron's direction (pp. 810-12). The half-sentence asserts that the industrial revolution in cotton textiles in Russia "was conditioned by the emancipation of the serfs, which facilitated the recruitment of a labour force by freeing the peasant masses and making them more mobile" (p. 801). Gerschenkron demonstrates conclusively that the issue was much more complex (cf. Evans' law that everything is more complex than most people think), and that serfdom was replaced by other links to bind the peasant to his village—undivided rights in village communal property, undivided obligations to pay for the land to the gentry or the state, and passport control. He explores the economic, social, and legal status of the peasants at length and concludes that Russian industrialization did not depend upon a flow of peasant labor but involved labor-saving investment instead—a substitute for what is usually called a prerequisite.

But the most interesting, challenging, provocative essay in the volume is Landes' sprawling discussion of technological change in Western Europe. His mastery of technical process, demonstrated in a dozen industries in half as many countries, is dazzling at the micro-level, and the descriptive passages, which make up two-thirds of his book-within-a-book, are must reading for student and scholar. When it comes to explaining why things have happened the way they have; why Britain pioneered in the Industrial Revolution and the Continent lagged in emulation; why the hand-loom weavers and the nailers survived after the development of the factory, why Britain fell behind after 1870, and it was Germany which overtook and surpassed her in technological achievements, he is not quite so satisfactory. Partly it is a question of the organization of the essay, which has difficulty in making up its mind whether to proceed on chronological, geographical, or analytical lines. Partly

it is a woods-and-trees problem. Partly, this reader believes, he underestimates the power of the Lewis model and what happens to profits when wages start to rise; and that of the Salter model, where old methods can survive so long as total average costs on the new process are greater than marginal costs on the old.

But Landes caps his brilliant virtuoso performance in the description of technology with the right answers. He has moderated his earlier insistence on the primacy of the entrepreneur, though the latter continues to play an important part. He eschews all single-valued explanation. In dealing with the loss of British leadership, for example, he puts aside resources, population, and overseas investment, and whilst skeptical of the advantages of backwardness, focuses on the handicap of the headstart—interrelatedness, the flagging of entrepreneurial energy, the weakness of education and science. Economic history is good only for middle-range conclusions. Economic development by and large is too complex.

Unlike the curate's egg, this book is decomposable, but like it, parts of it are very good indeed.

The index suffers from omission of all references to cited literature. The 100-page bibliography, largely Landes' 63 pages, is superb.

C. P. KINDLEBERGER

Massachusetts Institute of Technology

The Roots of American Economic Growth, 1607-1861: An Essay in Social Causation. By STUART BRUCHEY. New York and Evanston: Harper & Row, 1965. Pp. xiii, 234. $4.95.

The express purpose of this slender volume is to trace "the historical emergence of [industrialism's] necessary and sufficient conditions in the United States. What aided and what inhibited the emergence of those conditions? In what attitudes and values, institutions, processes and events, did they have their roots?" (p. 13). In a discursive, engagingly written commentary on aspects of the economic, social, and political history of the American colonies and the young nation, Stuart Bruchey sifts the earth already turned by others, rather than reporting on fresh digging into primary sources. *The Roots of American Economic Growth* is intended to provide an interpretive guide to the recent literature and, above all, an interdisciplinary synthesis that will "place the process of economic growth within the mainstream of American history" (p. xi.). In interdisciplinary matters the author appears to be of the total immersionist persuasion, and the subject of this baptismal exercise emerges lamentably waterlogged.

Taken one at a time, many of the resumés of the literature provided by the book are quite useful, and, all in all, the state of knowledge remains in much the same condition as that in which Mr. Bruchey found it—comparatively few new confusions are introduced. Students, however, should be particularly alerted to steer clear of the sections dealing with international trade and finance, which is rather a drawback when one considers the openness of the economy under discussion. The following extract (p. 142) may stand as a warning:

> Americans also tapped foreign savings by selling goods and services abroad, and while resultant earnings financed the importation of some capital equipment, one main contribution to domestic capital formation was indirect. During three periods in the antebellum years, namely 1793-1807, 1815-18 and 1832-39, the terms of trade on which American exports were exchanged for imports of consumer goods were favorable to the United States. That is to say, a greater rise in export than in import prices permitted a given quantity of American goods to command a larger quantity of foreign goods. By buying abroad instead of at home Americans thus saved money, and the savings could be released for domestic capital formation.

The preface discloses that the author is, by training and inclination, a social historian; he modestly avers economics is not really his forte. Thus disarmed, the reviewer must follow Bruchey in being more concerned with the grand design than with details of its execution.

"Community will and acts of governments, the structure of society and its values, knowledge and education, attitudes toward technological change, the actions of private investors, and the effects of widening markets"—in these are found the roots of industrial development and America's economic growth (p. 208). The thesis of the book is that the acceleration of per capita output growth during the first half of the nineteenth century resulted from manifold complex interactions among economic and noneconomic changes. Bruchey is enough of an economist to posit that within the foregoing set everything must have depended upon everything else, in at least two different ways. Widening markets encouraged specialization and paved the way for technological advance; specialization combined with education to foster occupational differentiation, which, in turn, increased social mobility. Greater social mobility intensified the vigor with which Americans pursued material gain, thereby increasing their receptivity to technological innovation and the responsiveness of private investors to opportunities created by widening markets. "Heightened alertness to the possibility of material betterment," a by-product of greater social mobility, also led communities to prod governments into actions which improved transportation, expanded markets, encouraged private investment, and furthered mass education. Education, as we already know, raised productivity and increased social mobility. And so on. As far as one can tell, these processes occurred similtaneously, at unspecified speeds, each leading to highly significant changes of unknown magnitude, all of which contributed to growth.

Bruchey turns this wonderful Chinese puzzle over and over, peering at it from all angles, marveling at the way it fits together, and ultimately decides it has no key pieces and is not meant to be taken apart. At one or two points in the book he is sorely tempted by the thought that the whole thing may have hinged upon the "progressive permeation" of the "desire for efficiency in the use of economic resources"; this "wide diffusion of the demand for growth" increased individual motivation and projected the federal government into "a place of central importance in American growth, because of its contribution to the formation of a national market and the national credit" (pp. 208-9, 213). But such impulses to stress the sociopolitical (or, indeed, any) parts of the

process as having been more consequential than others are firmly controlled. The result is a "balanced" appraisal, in which all the pieces are seen to have formed an historically necessary and obviously sufficient whole.

So jejune a rendering of American economic history will come as a disappointment for readers who do not quickly get the message telegraphed by the methodological discussion in Chapter I: "To force a translation of qualitative factors into the language of numbers is to guide us to reality in the way of parody. To apply formal economic analysis to the complexities of long-run change is to invite the rigor of *rigor mortis*" (p. 10). It is not simply a matter of the prevailing irrational passion for dispassionate calculation having taken all the joy out of writing economic history. The problem goes deeper, for "quantomania" and narrow adherence to economic logic may choke off discussion of the sociopolitical contributions to American economic growth, although their importance is, for the author, clearly demonstrable by the exercise of "historical judgment." The proposed cure for this real ailment is no mere antidote for a bad case of the "new economic history." It is also a powerful specific for cramps caused by reliance upon evidence and connected argument.

Take the treatment of the role of vertical social mobility as a case in point. Bruchey asserts that the increasing fluidity of the American social structure accelerated the pace of per capita output growth by raising "incentives to produce." Yet he can grant there is presently no evidence that the structure of society had become more (or less) rigid between, say, the mid-eighteenth and the mid-nineteenth century. Nor does it seem necessary to define social "incentives to produce" in a way that would distinguish them from economic incentives, or permit identification of changes in the degree of "social motivation." When one comes right down to it, the author barely offers an explanation for the putative functional connection between the time derivatives of these two state variables. The whole idea seems to derive (pp. 201-2) from de Tocqueville's comparative observation that in America, where differences in social status were confined mainly to those deriving immediately from differences in wealth, inferiority of economic position became an overriding preoccupation; immense energy was poured into attaining material "equality" with others. This insight is surely too slender a reed to carry the weight of even a partial explanation for the upward shift in the long term rate of U.S. per capita output growth. Moreover, it bends easily in quite different directions. Might one not infer from de Tocqueville's comment that in the American setting the proximate governor of individual economic vigor was the perceived degree of inequality in wealth? Then, more pronounced inequality in the distribution of wealth (or income)—which, by the way, Bruchey would equate with greater *rigidity* of the social structure (pp. 59-60)—should have led to still more ardent dollar-chasing.

In an appreciative blurb on the dust-jacket of this book, Thomas Cochran says: "I think it will be widely accepted as the standard interpretation of the first phase of American Industrialism." One must trust that this was just an idle threat.

P. A. David

Stanford University

International Aid—A Discussion of the Flow of Public Resources from Rich to Poor Countries with Particular Reference to British Policy. By I. M. D. LITTLE and J. M. CLIFFORD. New York: Aldine, 1966 and London: George Allen and Unwin, Ltd., 1965. Pp. 360. $7.50, paper $2.95; 45s, paper 25s.

If, in the outpouring of development literature, any branch of the subject can possibly have been underwritten, it has been foreign aid. Memoirs of aid administrators, governmental reports, and much economic journalism on the subject exist; but the comprehensive and systematic type of study that would appeal to professional economists has been surprisingly rare. This book is therefore to be welcomed—and it is to be lauded for its excellence in sorting out the principles of aid-giving and for its lucidity in analyzing the complex interrelationships between donors' and recipients' problems within the framework of development planning.

While comprehensive, the argument never bogs down in secondary detail; while rigorous, the analysis never becomes overly formal or unnecessarily technical; and while grounded on fundamental economic principles, there is still much of the needed contact with the tradition of political economy. It is readily admitted that almost all aid decisions require political judgment, both as regards the donor's political interest, and as regards the influence of political factors on how effective a particular act of aid-giving will be in promoting development. But granting this, the authors still have much to contribute on the parallel economic judgment that is necessary to get the best particular decisions made.

The book is divided into four Parts. The first traces the history of aid (unfortunately, this makes for a rather dull and slow start); the second concentrates on problems of aid recipients (here are some of the most incisive analytical sections); the third examines donors' problems; and the last part largely applies what has been said in earlier chapters to current and future British aid policy (but many of the observations would also apply to U.S. aid policy).

The vagaries of their subject notwithstanding, the authors state their conclusions forthrightly and unambiguously. On the capacity of poor countries to use aid, they conclude that, over the next few years, around $1-$1½ billion more public aid could be applied with considerable effect, mostly in India. But, for many recipients, improvements in the conditions and forms of aid might do more to raise the rate of development than an increase in the volume of aid. They further believe that the political impact of aid could be improved if Western donors would concentrate more on economic development, and worry less about the effects on their own economies and trade, and less about their own political aims. Aid-tying is strongly criticized: it distorts investment priorities and twists the import pattern in favor of capital goods, resulting in an underutilization of domestic resources and a less effective use of aid for economic development. For balance of payments reasons, U.S. and British aid cannot be untied completely, but it is submitted that in giving aid the donor should be prepared to pay such proportion of the domestic costs as is appropriate to the nature of the project, or maintain a policy of donor-or-local (or, even better, donor-or-developing-country) procurement; or, in countries

like India, where it can reasonably limit its project aid to import costs, it should give an adequately large proportion of general balance-of-payments aid. (It can, however, be questioned whether aid-tying has really been of substantial help to the U.S. balance of payments.) To ease the problem of aid-tying, the authors join those who advocate an increase in world liquidity and the channelling of newly created international credit to the poor countries (as is proposed in the Stamp Plan). To allow greater attention to the economics of aid usage, there should also be closer coordination among donors in the field. Lastly, on the financial terms of aid, the authors conclude that, in the case of almost all donors, repayment terms are still far too short to be appropriate for development aid. "Donors as a whole cannot seriously expect to be receiving resources back from the present recipients in this century. With few if any exceptions, if a single donor gets paid back it will be from the aid of others. Interest payments also serve no function in aid to governments. Logically, all such aid should be grant-aid. Although we do not, in fact, advocate this, we do suggest that all development aid should be in the form of very long-term loans at merely nominal rates of interest" (pp. 335-36).

The real merit of the book, however, does not lie in its (essentially unsurprising) conclusions—but rather in the fact that the authors sharpen a number of critical and confusing issues on their way to reaching these conclusions. On page after page, the reader is offered an abundance of good sense on such issues as what is the true cost of aid; whether aid is needed for balance of payments' reasons quite distinct from the need for aid as an adjunct to savings; the relation of aid to trade, and the extent to which they are substitutes or complements; whether any burden-sharing formula is feasible; how aid should be used to maximize the present value of the increases in consumption per head (the general principle of maximizing utility, of which maximizing development is a borderline case); whether aid should be given on a project or plan basis; how to meet the "home cost" problem; how to limit the abuses of private export-credits; whether a reasonable quantitative estimate of the need for aid can be produced; and how to ameliorate the debt-servicing problem by tailoring the terms of aid to suit the respective recipient.

In sum, a substantial and thought-provoking study that merits wide attention—especially at the present time when the capacity to utilize aid more effectively has risen in many poor countries. but the real value of aid has declined to a distressingly low level.

GERALD M. MEIER

Stanford University

Canada: An Appraisal of Its Needs and Resources. By GEORGE WILSON, SCOTT GORDON, STANISLAW JUDEK, and ALBERT BRETON. New York: Twentieth Century Fund, 1965. Pp. lxx, 453. $7.50.

This study is a stepsister to the other volumes in the Twentieth Century Fund's *Needs and Resources* series. It differs quite remarkably from the others both in omission and commission. The main statistical work in the Canadian study is on basic trends in the broad aggregates of economic and social development. This statistical material is fully integrable with the data for other volumes of the series. However, on many subjects, the statistical treat-

ment in the study is much less detailed than in the other studies. Separ
chapters are not included on each of a large number of classes of consumer
quirements; there is very little on urban development and redevelopment,
housing, or the composition of government expenditure. The novel elements
the Canadian study are the analytical and evaluative texts and the forays in
political economy. Some applications are made of theories of economic grow
to explanation and forecasting of Canada's performance. Some analysis
made of Canada's migration experience and policies, of the changing structu
of social power and of the fundamental political and social drives of the n
tion. Also the book contains much more evaluation of ideas, performanc
and policies than did the other volumes. The authors are quite outspoken, f
example, on Canada's tariff policies, the issues of foreign (mainly U.S.) ow
ership and control of resources and activities in Canada, the country's postw
developmental policies, and the role of Canadian policy as a cause of Canada
relative economic stagnation between 1957 and 1962.

To a reader in the United States, two political strains in the book will
startling and yet among the most valuable features. The first is the impact
"the French fact." The superficial aspect is in the appearance of both Englis
and French-language texts for several sections and a French version only of
Commentary by Albert Breton. The more important aspect is on the Frenc
"facts" in the creation of the nation in the first place—and as major forces
the current reshaping of the nation. The other feature which may startle t
U.S. reader is the (now nearly two-centuries-old) concern over the challeng
to Canada's independence from the United States. The forms of the challeng
have changed. Nowadays one hears an astonishing amount of talk in Cana
about U.S. economic and social imperialism. These two political strains a
brought together in the introduction: "There is a continuing uncertainty an
irresolution about Canadian nationhood . . . for there are subtle and abidi
strains upon it. . . . These strains spring from two sources . . . the intern
cleavage between French and English, and the external proximity of t
United States . . . and both of these are as powerful today as they were i
1867" (the birthday of Canada's confederation). Breton's commentary som
what discounts the difficulties of the French "facts" for the viability of the n
tion, and rightly so in my view. And the main authors discount the U.S. im
perialism line considerably, also rightly so in my view. But these two forc
are among the most prominent ones still shaping Canada's economic pro
pects.

The book begins with a long introductory essay on A Nation Still in th
Making. This is an interpretation of the forces which brought Canada int
being as a nation; and of the success of the railroad building, the partial suc
cess of the migration policies and the failure of Canada's tariff—the three ele
ments of the historic National Economic Policy. More recent trends and issue
are also reviewed. Part One, The Recent Past, consists of four chapters look
ing back on Canada's economic experience, mainly during the last 30 years
Chapter 1, Dimensions of Growth, analyzes the usual data on the nationa
economic aggregates, and general indicators of structural change; data o
changes in income distribution are also presented. Chapter 2 is on Populatio

Patterns, the one rather unique feature being the presentation of some detail on immigration and emigration. Chapter 3 is on Regional Differences in economic performance, history and structure within the country, a long-standing concern in Canadian life and policies. Chapter 4 is on the Foreign Influence. Canada's trading relationships and trends are analyzed, as well as the various indicators of foreign indebtedness and uses of foreign financing.

Part Two is on Resources. Chapter 5, on Land and Natural Resources, presents a very optimistic view of unutilized potentials in Canada's position in nearly every respect—agricultural land, minerals, forests, urban sites, etc. Chapter 6, on the Labor Force, is fairly routine, except for the analysis of external migration as a factor in the size and composition of the labor force. Chapter 7, Capital, presents data on the capital stock, and estimates of both average and marginal capital-output ratios. Saving and investment relations are also analyzed. Chapter 8, on Management, Productivity, and Research is somewhat unique in analyzing the supply of management, in presenting a view (commonly referred to in Canada as the WASP theory) of the structure of the power elite in the country, and in some attempt at analysis of data on research and development expenditures. The discussions of productivity are more routine, various forms of gross measures being produced as well as some discussion of productivity differences among various sectors of the economy and differential productivity trends. The WASP theory of the power elite attempts to show (and then to analyze) the extremely heavy weighting of White Anglo-Saxon Protestants in what is identified as the commanding heights of business, political, and institutional power.

Part Three, The Future, contains projections to 1970 and 1975, essentially as a simplified general exercise in economic forecasting, not using advanced techniques or conceptual framework. Projections are made for population, labor force, unemployment, productivity increase (in the sense of output per man-year employed), and a few broad categories of expenditure. The more elementary cross-checks for internal consistency and plausibility are made. The book concludes with an Epilogue, which brings the trends, economic analysis, and projections into relationship with the political and social problems; and Breton's Commentary. The Director of the Fund describes the latter as "themes in the study which might have special interest for the French-speaking community in Canada." Breton has certainly not limited himself to this task, or even made it his central concern. He has made comments on the French-English issues, which are at least as interesting to the English-language groups in Canada as to the French. More importantly, he has offered important qualifications to the general content of the study, of interest to all readers—comments, for example, on interpreting the productivity trends, on the analysis of changes in income distribution, on the processes of adaptation in the country, on neglect of money and finance in the book and so on. It is indeed a pity that this commentary was not translated into English.

The book is quite a mixed bag. There are exciting insights, such as the hypothesis of inferiority of Canada's management talents because of the preeminence of the WASPs in the power elite; the themes of missed opportunities of economic maturity and increased productivity in the postwar world; and the

analysis of brain "gains" and "drains" in the country's external migration. But there are pieces that are thoroughly pedestrian, such as much of the demographic analysis and the discussion of labor force participation. The modernized "export staple" theory of Canadian economic growth is not really convincing, nor is the analysis of Canada-United States differentials in capital-output ratios. While I share the authors' doubts about the usefulness of Canada's tariff, the treatment of the tariff in the book is rather weak, relying on older and somewhat discredited estimates and concepts of the effects of Canada's commercial policy. One gets little treatment in the book of the important issues of major business cycles and growth cycles, the neglect of both of which limited the effectiveness of the Gordon Royal Commission a decade earlier. On economic policy the book is a mixture too of extremely perceptive analysis, for example of migration policy and of some incidents of fiscal and monetary analysis; and of disappointments, for example regarding the role of the post-war structure of taxation as a factor in the country's economic performance. Nevertheless the study is a very useful one indeed, both because much of the economic description and analysis is excellent and because of the attempt to integrate political and social considerations with the more narrowly economic ones.

DAVID W. SLATER

Queen's University
Kingston, Canada

The Political Economy of Latin America. By WENDELL C. GORDON. New York: Columbia University Press, 1965. Pp. x, 401. $8.75.

Although the author writes in his preface that the "royalties for this book have been waived so that it can reach the greatest number of students," the price still precludes its use as a supplementary text in the classroom. This is unfortunate, because the commentary on the need for institutional change will receive substantial support from regional specialists. The book begins with a brief discussion of institutional theory (à la C. E. Ayres) and then outlines the evolution of Latin American economic systems. The historical introduction is interesting, but needs elaboration in order to promote genuine understanding of relevant issues. The author's tendency to oversimplify complex problems in a rather naïve fashion is demonstrated when he suggests that perhaps the United States should promote a policy whereby "no member of the United Nations . . . will diplomatically recognize any but a democratically elected government in the future" (p. 35).

Part II consists of five chapters dealing with various aspects of market organization, e.g., private practices, government regulation, commodity control schemes, etc. The complicated legal arrangements for carrying on business activities in Latin America are discussed, while sharp criticisms are leveled at certain prevailing practices. Professor Gordon is unhappy with the lack of competition in industry, the excessive use of price controls, and the problems associated with commodity control arrangements. Concerning the latter, his propensity to rely on normative analysis is evident when he describes the Organization of Petroleum Exporting Countries as a "conspiracy against the gasoline

buyers of the world . . ." (p. 89). Under the caption of Welfare, Part III traces the labor movement in the region, social security legislation (which, it is alleged, has been enforced only against foreign-owned companies), and the level of living, income, and production. Some readers will be troubled when they read that "the mythology of free private enterprise is that profits are a reward for risk taking, or innovation, . . . or something of the sort" (p. 98), or that "the conclusion should not be drawn that Latin America is relatively more socialistic than is the United States" (p. 99). These statements may be true, but they are not supported empirically in this volume.

Part IV, Economic Development, makes up the core of the book with eight chapters devoted to such topics as the basis for industrialization, planning, capital formation, priorities, etc. Although some of the descriptive remarks are provocative, the experienced development-oriented economist will encounter little original analysis. Gordon argues that "the accumulation of technical knowledge is the great dynamic force in economic progress [and] that the chief . . . planning problem is to create the institutional conditions which will permit technical knowledge to be assimilated" (pp. 159-60). The alleged institutional barriers that must be dealt with include landowners (or the oligarchy?), dictators, the army, the Church, the public bureaucracy, and the Spanish language itself. In short, the formidable list essentially covers the major existing institutional characteristics of the region. It is obviously not suggested that they should be abolished, but the necessity for near-term modification is urged—man should be the master, not the slave of his institutional environment.

On the subject of planning, Gordon's pen is particularly sharp when he criticizes the common practice of using foreign advisory personnel to construct an ambitious plan, and then leave the all important implementation to local politicians after the draftsmen have departed. Rather than an unrealistic emphasis on ten-year development plans, it is suggested that the real need in Latin America is for "unprogrammed planning" that would be directed toward more effective mobilization of the "least effectively used" resource, namely, underemployed manpower. Also, industry studies would help mitigate the thorny complementarity question of which country or region should develop a particular industry. The Alliance for Progress is validly criticized for its failure to grapple with this by placing undue emphasis on national plans for self-sufficiency, even though the desirability for increased regional integration is recognized.

Gordon believes that a shortage of domestic saving has not been a major factor in holding back Latin American development. He rejects the notion that monetary savings must precede investment (an argument used to defend skewed income distribution and to attack progressive taxation) and suggests that a government can create the funds needed for capital formation if a scarcity of voluntary, private savings is evident. This is the familiar inflationary route to development and has merit if unemployed real resources are in fact available, and the new investment projects do not place excessive strain on foreign exchange availabilities. Historical experience, however, indicates that Latin American governments have had difficulty in exercising adequate self-

discipline in their monetary and fiscal policies. Foreign investment as a development vehicle is recognized for its potential in transferring technology, but the impression that the developed countries have made a significant resource contribution to Latin America between 1946-61 is rejected. "Net capital inflow . . . was $11.8 billion and net debt service transferred out was $16.0 billion for the period . . ." (p. 240). Developmental priorities are examined with a critical appraisal of the usual conspicuous production (i.e., steel-mill) approach to government planning. It is argued that the entire region should be developed in terms of six or eight major industrial complexes. Many economists would agree with this recommendation, but political realities make meaningful implementation unlikely.

Part V, Trade and Finance, contains chapters on trade patterns, common markets, monetary standards, banking and public finance. A strong point is made in support of improving the qualitative ("how it is used") aspect of credit control. Indeed, traditional lending practices leave much to be desired, but it may be difficult to accept the remark that "there is in general in Latin America no shortage of money and credit in terms of total quantity" (p. 348).

In conclusion, the reviewer appreciates the author's statement that "this book is much more of an expression of opinion than it is a rigorous report of research findings" (p. 387). The book is too impressionistic, negative, and sweeping in coverage. The lack of scholarly documentation will disappoint many students, but the writing style is lively and designed to have an emotional impact. The shortcomings would have been minimized, however, if the author had devoted greater attention to prescription for remedial policy action.

J. D. DEFOREST

Parsons College

Statistical Methods; Econometrics; Social Accounting

Studies in Mathematical and Managerial Economics. Vol. I, *Optimal Decision Rules for Government and Industry.* By HENRI THEIL, in association with others. Chicago: Rand McNally & Co., 1964. Pp. xvii, 364.

In this book Henri Theil presents a statistical decision analysis, and gives example applications to a number of concrete decision problems drawn from the economic histories of the United States and the Netherlands, and drawn from the production and employment problems faced by a particular business firm. The reader is led successively through the development of the theory from the simpler static case under certainty to the fully dynamic case under uncertainty. The theory developments are followed immediately by concrete examples. Both the technical developments and the applications are discussed fully and in detail, and the exposition attains a high level of clarity. This book should be an important contribution to those who are interested in quantitative decision-making in business, government, and elsewhere. In addition it should help to clarify many fundamental points in economic theory relating to decision-making.

Perhaps the best way to indicate the range and depth of this book is to discuss in some detail one of Theil's more advanced examples. He considers the

problem of making decisions on national economic policy for the Netherlands for the period 1957 through 1959. The policy instruments are taken to be the general wage level, three tax rates, and the level of government expenditures on commodities. National economic welfare is assumed to depend quantitatively on the level of unemployment, the change in the level of prices in private consumption, the share of wages in national income, and the surplus in the balance of payments. In addition, welfare weight is assumed to attach directly to the above instruments of policy which the government uses.

The welfare weight which attaches to the above variables is assumed to differ somewhat for different interest groups. Specifically three sets of preferences are formulated, one for each of the following: labor, employers, and a neutral group that is intended to represent the broad national interest. The problem posed by Theil is to find the decision strategy that, looking forward over a three-year planning horizon, will achieve an optimal compromise over the various objectives and interest groups in setting the four instruments of policy, taking into account the fact that uncertain disturbances also will have impacts on the economy. The conditional forecasts that are needed for this decision analysis are based on a 40-equation econometric model of the Dutch economy. Theil makes a thorough exposition of this problem including such subtleties as penalizing erratic fluctuations of the instrument variables and carrying over unfulfilled objectives to succeeding years.

He then proceeds to solve the problem. This is done by approximating the welfare functions by quadratic equations and maximizing the expected values of welfare subject to the constraints imposed by a linear econometric model of the economy. The political compromise between the conflicting interest groups is attained by putting equal weight on the welfare loss that each group suffers from being forced to compromise its ideal plan for running the country and then finding the compromise that minimizes the total loss.

Not only does Theil treat the problem of finding the optimum strategy for making the decisions, but he also considers: (1) the welfare cost of the government's inability to forecast with perfect accuracy the 13 exogenous variables that the government does not control, such as exports, (2) the consequences in terms of welfare loss of making errors in estimating the parameters of the econometric model, and (3) the welfare cost of errors in estimating the parameters of the welfare functions. He studies the rate at which the cost of forecast errors declines as the forecast span increases. By using the econometric model and the optimum strategy, forecasts are made of future actions and the corresponding future outcomes. He considers the effect of truncating the planning horizon to three years or extending it to an infinite horizon. He considers the extent to which some policy instruments are substitutes for others and the extent to which they are complements.

Although one can quite rightly question the rather severe restrictions imposed by a symmetric quadratic utility function and by a linear econometric model, this linear decision rule analysis has the great virtue that its solution is sufficiently easy through matrix manipulations to enable quite subtle and profound questions to be studied directly. Hence the power of this approach should not be underestimated, particularly as a first approximation.

Each section of the book contains a clear nontechnical summary at the end,

and many readers will find this a useful way to start the section. A similar remark might be made about the theory and applications chapters. Many readers will find it easier to read the theory chapter after having first examined a concrete application. The availability of the nontechnical summaries and the clear separation of the theory from the applications mean that readers unfamiliar with matrix algebra can still obtain a great deal from this book.

Now for more technical points.

The matrices arising from practical problems may readily become too large for hand computation, and the reader looks in vain for Theil to mention the availability of a computer program[1] that might perform the mathematical manipulations of the decision analysis.

On page 123 Theil lays considerable stress on the important fact that a decision strategy for the sequential uncertainty case should take into account the fact that new information will become available before subsequent decisions are made. He shows a case in which the mere knowledge that information will be available influences the action that is taken in the initial period. In strong contrast to this, however, he demonstrates that the same linear decision rule serves for decision-making under certainty, for making simultaneous decisions for many time periods under uncertainty, and as the optimum strategy for making the first decision in a sequential uncertainty situation. Thus Theil has given concrete examples of two cases, one in which the strategy approach makes an important difference, and one in which it does not. This suggests that there are two classes of mathematical decision models, one of which is vastly simpler since it does not require formal treatment of the strategy problems in the solution process. The property of certainty equivalence is undoubtedly very special, perhaps being limited to the linear decision rule case, but there may be a larger class of decision models in which the strategy problem may be treated implicitly rather than explicitly in the solution process.

Because of the relatively small amount of work that has been done in quantitatively estimating social welfare functions, and also because of their somewhat intangible nature, it comes as good news that Theil finds on page 84 that the errors from misestimating the parameters of the welfare function are much less serious than those arising from errors in estimating the parameters of the econometric model itself.

Another encouraging note is the finding that forecast and other errors are less serious in their consequences for a dynamic sequence of decisions than for one-shot decisions. Even though the solution for an optimal strategy is usually more difficult to obtain, individual decisions are less critical because of the possibility of partially reversing mistakes in the periods that follow.

Theil's book is by all odds the best one to date in the exposition of the linear decision rule analysis and there is every reason to expect that it will make an important and lasting contribution to the introduction of such methods in government and industry.

Charles C. Holt

University of Wisconsin

[1] Of course, programs are readily available at most computing centers for all of the basic matrix manipulations that are involved.

Econometric Analysis for National Economic Planning. Edited by P. E. HART, G. MILLS, and J. K. WHITAKER. Proceedings of the Sixteenth Symposium of the Colston Research Society held in the University of Bristol April 6–9, 1964. London: Butterworths, 1964. Pp. xii, 320. $14.50.

The Colston Research Society has held symposia and published papers annually since 1948 on diversified subjects including natural and social sciences and humanities. The main criterion for selecting a topic is that the subject should be at an interesting and active stage of development. The subject of the present volume easily meets the criterion.

Approximately eighty members participated in the 1964 symposium; most were from England, and a few from the rest of Europe and the United States. The papers included are J. J. Johnston, D. D. Bugg, and P. J. Lund, "Some Econometrics of Inflation in the United Kingdom"; J. D. Sargan, "Wages and Prices in the United Kingdom: A Study in Econometric Methodology"; Richard Stone, "British Economic Balances in 1970: A Trial Run on Rocket"; G. Pyatt, "A Production Functional Model"; L. R. Klein, "The Social Science Research Council Econometric Model of the United States"; P. J. Verdoorn and J. J. Post, "Capacity and Short-Term Multipliers"; A. Moustacchi, "The Interpretation of Shadow Prices in a Parametric Linear Economic Programme"; Leif Johansen, "Saving and Growth in Long-Term Programming Models"; W. Sadowski, "Economic Organization and Planning"; A. P. Barten, "Family Composition, Prices, and Expenditure Patterns"; and G. Menges and H. Diehl, "Time Stability of Structural Parameters." Each paper is followed by often interesting and instructive discussions.

To classify these papers into broad fields, four papers (Johnston, Stone, Klein, and Verdoorn) are on econometric applications, two papers (Sargan, Menges) on econometric methodology, two papers (Pyatt, Barten) on economic theory, and three papers (Moustacchi, Johansen, Sadowski) on programming. Although some papers (notably Sargan and Johansen) are of considerable theoretical interest, the main emphasis is on practice rather than on theory. Most authors impress me as having had much experience in econometric model-building or planning programs and being familiar with the complicated problems of the real world, and the hints and insights they throw here and there amidst formidable notation and lengthy mathematics must be very useful for practically minded econometricians or planners.

In this review I have chosen a few that have especially aroused my interest. I have not commented on papers by Stone and Klein on econometric models of the British and American economy, respectively, since each of them is a part of the greater program described elsewhere and had better be reviewed thoroughly as a whole. Stone's paper is related to the series of books, Department of Applied Economics, University of Cambridge, *A Programme for Growth* (London 1964), and Klein's paper to L. R. Klein, James S. Duesenberry, Gary Fromm, and Edwin Kuh (eds.), *The Brookings Quarterly Econometric Model of the United States* (Chicago 1965). Both papers are good introductions to the more detailed studies cited above.

The paper by Sargan is in a sense a sequence to his important work, "The

Maximum Likelihood Estimation of Economic Relationships with Aut regressive Residuals" (*Econometrica*, July 1961). However, it is se contained and can be understood without reading the previous work. In t *Econometrica* article Sargan proposed a generalization of limited inform tion, maximum likelihood estimator which is consistent and asymptotical efficient when the residual of the equation to be estimated follows a firs order auto-regressive process. The spirit of the article was that, even if t real process may be more complicated, it is often better to assume at least first-order autoregressive process rather than complete serial independen for the residual. The present paper is written in the same spirit and solv some of the practical problems that arise in the use of the estimator pr posed earlier.

The following is a summary of Sargan's paper. Let the regression equatio be $y=X\beta+u$, $u=\rho u_{-1}+e$, where y is a vector, X a matrix of observed ra dom variables, u a vector of unobservable random variables, e a vector independent and identically distributed random variables with mean ze and an unknown constant variance, and β is a vector of unknown paran eters, ρ an unknown parameter. The subscript "-1" means that each el ment of the vector is shifted one-period backward. Combining both equ tions, we have $y-\rho y_{-1}-X\beta+X_{-1}\beta\rho=e$, or simply $\xi a=e$.

1. If X is predetermined, estimate ρ and β by maximum likelihood, whic is equivalent to minimizing $a'\xi'\xi a$ with respect to ρ and β. Test the hypot esis on the process of u by likelihood ratio test.

2. If X is partly endogenous, Sargan's *Econometrica* estimator may b used. Or use a 2SLS-type estimator which is asymptotically equivalent to and which is obtained by minimizing $a'\xi'Z(Z'Z)^{-1}Z'\xi a$ with respect to ρ and where Z is a matrix of properly chosen instrumental variables. Again th hypothesis on the process of u may be tested by likelihood ratio test.

3. Sargan compares the speed and efficiency of various iterative method of solving the minimization problem described in (1) or (2). Since the min mand is a quadratic form, convergence to the absolute minimum is certa with probability one.

4. The asymptotic distribution of

$$\frac{\hat{e}'_{-1}\hat{e}}{\hat{e}'_{-1}\hat{e}_1}$$

is obtained, where $\frown$ indicates the estimate.

5. The estimator that takes into account first-order autoregression of th residual is compared with the one that assumes serial independence in esti mation of some equations of Klein-Ball model, and they are shown to pro duce different policy implications.

Johansen's interesting paper analyzes the nature of solution of a finite period planning program with linear and nonlinear objective functions in two-sector, vintage-capital model. In this review Johansen's conclusions ar illustrated in a one-sector, no-vintage model.

A simplest finite-period planning program with a linear utility function may be formulated as: Maximize

$$\sum_{t=1}^{T} \lambda_t C_t$$

under constraints $C_t \geq C_t^*$, $I_T \geq I_T^*$, $Y_t = C_t + I_t$, $Y_t = \beta I_{t-1}$, $C_t \geq 0$, $I_t \geq 0$, where C_t is consumption, Y_t available stock, I_t input, and λ_t, C_t^*, I_t^*, I_0 are given constants. In this case it can be easily shown that the nature of the solution is $I_1, I_2, \cdots, I_k$ to be made maximal possible under constraints, and $I_{k+1}, \cdots, I_T$ minimal possible for some k between 0 and T. In other words, invest in the early part and consume in the later part of the plan.

From this we learn the following: (1) $C_t \geq C_t^*$ is necessary, for otherwise we may have $C_t = 0$ for some t. (2) $I_T \geq I_T^*$ is necessary, for otherwise $C_t = 0$ for $t \geq T$. (3) Solution varies greatly by a slight change in λ's. To avoid this we must include more constraints on C's.

In short, the use of a linear objective function makes a "programming plan" almost like a simple "target plan."

Johansen shows by means of admirable algebraic skill and perseverance that the same nature of solution as above holds also for a two-sector, vintage-capital model.

The use of a nonlinear utility function makes rigid constraints of consumption unnecessary, although the lower bound for the terminal capital stock must be still set. If we replace the linear maximand with a nonlinear

$$\sum_{t=1}^{T} \delta^t \log C_t$$

in the previous problem and omit the constraint $C_t \geq C_t^*$, the problem becomes: Maximize

$$\sum_{t=1}^{T} \delta^t \log C_t \quad \text{under} \quad \sum_{t=1}^{T} \beta^{T-t} C_t = \beta^{T-1} Y_1 - I_T^*,$$

where Y_1 and I_T^* are given constants, and it can be solved by the method of Lagrange multipliers. The solution is

$$(1 + \delta + \cdots + \delta^{T-t}) C_t = Y_t - \frac{I_T^*}{\beta^{T-t}}, \qquad t = 1, 2, \cdots, T.$$

Johansen obtains the formal solution of a similar problem with a nonlinear maximand in a two-sector, vintage-capital model by means of Kuhn-Tucker theorem. The nature of the solution is analyzed by a thorough and ingenious intuitive inquiry and by a numerical example.

Pyatt argues that one should estimate the locus of *ex post* equilibrium points rather than the *ex ante* production function. The former may be linear, even when the latter is nonlinear. Here I shall illustrate it for the case of a Cobb-Douglas production function.

Let the production function be

$$X = \alpha N^{\beta} C^{1-\beta} \tag{1}$$

where X is output capacity, N is labor, C is investment expenditure divided by money wage rate, and α,β are constants, of which only α is assumed to change through time as a technology indicator. Defining $n=N/X$, $c=C/X$, we have from (1)

$$n^{\beta} = \frac{1}{\alpha} c^{\beta-1} \tag{2}$$

From (2) we have

$$n = \frac{\beta}{\beta - 1} \frac{dn}{dc} \cdot c. \tag{3}$$

Thus, if $dn/dc = -r$ (r being the rate of interest) at equilibrium, we have

$$n = \frac{\beta}{1 - \beta} r \cdot c, \tag{4}$$

which describes the locus of equilibrium points of n and c as α changes through time.

Pyatt seems to prefer, for reasons not clear to me, the production function,

$$X = \frac{\beta}{\gamma} N + \frac{1}{\gamma} C(1 + \log \alpha + \log X - \log C), \tag{1$'$}$$

where α, β, γ are constants, of which only α is assumed to change through time. For this production function, the equivalent of (4) is

$$n = \frac{\gamma}{\beta} - \frac{1 + \beta r}{\beta} c. \tag{4$'$}$$

His use of the Johansen-Kurz vintage-capital model as the general framework is only incidental as far as the main problem of the paper is concerned.

Verdoorn and Post discuss a 36-equation econometric model of the Netherlands economy, estimated by the limited information, maximum likelihood estimator. The "unused capacity" variable is measured by the unemployment percentage $\bar{w}$, and it affects endogenous variables nonlinearly, as the new variable is defined by $4.34\ \Delta \log (\bar{w}+2) - 0.2\ \Delta\bar{w}$, which in turn affects variables linearly. The variables affected by the unused capacity are exports, export prices, investment, imports, wages, and employment. The short-term multipliers of the impulse variables (competing exports, autonomous expenditure, wage rate, import prices) upon endogenous variables are then calculated for various levels of the unused capacity variable.

There have been many econometric and noneconometric works that attempt to explain the cost-push or demand-pull nature of the postwar

British inflation. The paper by Johnston, *et al.*, presents a simple model that purports to explain the price change as a function of the ratio of the demand for consumption to the supply. Various variants of the model are compared on the basis of the mean-square error of the estimated prices in the sample period. The novelty lies in the estimation of the *ex ante* variable—the demand for consumption, which is an important step into a new interesting problem. Moustacchi discusses a linear programming model concerning the French Fifth Plan. Sadowski calls the reader's attention to a new and interesting problem of finding an optimal economic organization in planning. Barten considers the problem of measuring the effect of a change in the family composition upon the family expenditure. Menges and Diehl mention conventional methods of testing the hypothesis that samples come from the same population and apply some of them to testing a structural change of the parameters in a regression.

TAKESHI AMEMIYA

Stanford University

Systems of Social Accounts. By G. STUVEL. New York: Oxford University Press, 1965. Pp. xiv, 270, $6.40.

This book by Professor Stuvel is primarily concerned with organizing and presenting economic data on the flow of goods and services as well as its counterflow of financial claims among various transactors by means of matrix accounting based on the so-called "double-entry" bookkeeping principle. Each transactor is accorded a row and the corresponding column in a square matrix and the entry in the *i*th row and the *j*th column represents the monetary value of the goods and services that flow from the *i*th transactor to the *j*th transactor or the counterflow of money from the *j*th transactor to the *i*th transactor. The basic scheme of matrix representation of economic data in this book is, therefore, identical to the one used in Leontief's input-output analysis.

Stuvel analyzes basically three types of problems in such matrix accounting: (i) methods of setting up transactors or *accounts* as well as consolidation and deconsolidation of transactors, (ii) relations between the *entries* in each cell of the matrix and the actual economic flows, and (iii) *measurement* problems which affect the amount of the entries. He demonstrates, by using the actual data of economic flows in the United Kingdom, how these problems may be partially solved.

The book consists of three parts which are further divided into 21 chapters. In Part I, "The Basic Accounting Framework," Stuvel starts by emphasizing the importance of representing economic data based on the double-entry bookkeeping principle. "They [identities in the double-entry system] ensure consistency between the values of different economic aggregates appearing in different parts of the macro-economic theory concerned" (p. 7). The basic two-account model which consists of the Production Account and the Financing Account is then introduced, which is next extended to include two sectors, the National Economy and the Rest of the World, each having Produc-

tion and Financing accounts. Finally, the Capital account is introduced to take care of the investment-savings aspect of economic flows. The last chapter in Part I discusses the valuation problem, introducing the Speculation account in the matrix presentation.

Part II, "Systems of Sector-Accounts," is essentially on the decomposition of accounts (discussed in Part I) into more detailed accounts in order to arrive at the "grand design of sectored systems." For example, each of the Production, Income, and Capital accounts is further subdivided into Personnel, Corporate, and Government sectors. The Production account is decomposed into agriculture, manufacturing, and service; Capital account into various financial institutions. The nature of the transactions among these detailed transactors is explained in detail.

The last part, Part III, "The Flow Content of the Accounts," discusses various special problems that may arise in implementing the national matrix accounting. Such problems as the separation of the market economy and the subsistence economy, the problems in the measurement of fixed assets formation and consumption, the valuation basis, i.e., market price versus factor cost, factor income allocation, etc., are discussed together with miscellaneous definitional, measurement, and recording problems.

My major criticism is that Stuvel seems to have neglected a large body of theories and practices in business accounting which may very well be applied to the kinds of problems discussed in this book. Of course, there are differences between business accounting and national accounting but at the same time there are similarities between the two in many important aspects of the problem which he could have taken advantage of.

He states "In the national accounts double-entry reflects the fact that each transaction involves two different transactors, one of which will make a debit entry in his books in respect to the payable arising out of this transaction, while the other makes a credit entry in respect to the receivable that accrues to him as a result of the transaction concerned. In the business accounting, on the other hand, the double-entry principle refers to the two entries made in the book of one and the same transactor in respect to simultaneous changes in two items of his balance sheet" (p. 12). Although this latter statement on business accounting is true, it does not follow that accounting theories and practices in business accounting are, therefore, inapplicable to national accounting. Various issues on measurement and valuation that have been developed in business accounting may very well be made useful for national accounting, especially for those problems that are discussed in Part III. In addition, there are important branches of accounting which specialize in transactions among various segments of an organization, just as national accounting deals with transactions among various segments of a nation. Good examples are consolidation, home office-branch accounting, and other divisional accounting, etc. Cost accounting which traces the flow of goods from department to department can also provide a rich body of knowledge which may be translated into national accounting terms.

Furthermore, such translation immediately high-lights the one-sidedness of national accounting presented in this book. In general, the commodity flow is

matched by the money flow in the opposite direction, but not always. Such a matching between the two flows requires the recognition of the cause-and-effect relationship between the two, which is difficult to do in some cases such as taxes, government subsidies, etc. Therefore, one cannot describe the economic flow by looking at the money flow only, as in the system presented in the book. Double-entry accounting is, of course, capable of handling both flows simultaneously as is already established in business accounting. Actually, the power of double-entry is that it forces us to trace the causal chains among various commodity and money flows and never to look at a flow independently from the rest of the flows.

Of course, the above comment is applicable to other articles in national accounting. However, I am disappointed because Stuvel's system is not different from the one that has been traditionally used in the input-output, interindustry analysis.

The main contribution of the book, therefore, seems to lie in the detailed discussions on the "sectoring," i.e., how accounts may be established and how transactions among accounts may be reflected in the matrix accounting. The book presents a well organized step-by-step approach to this problem of sectoring. In addition, Part III of the book provides a good discussion on important problems in the measurement and the valuation of economic flows that need to be solved. In this respect, this book will provide a good basis for understanding the ways in which national economic data may be organized, although, I must reluctantly add, the very limited number of references to related work in the field and the lack of bibliography isolate the book somewhat from the rest of the work in national accounting.

YUJI IJIRI

Stanford University

Economic Systems; Planning and Reform; Cooperation

Monopoly Capital—An Essay on the American Economic and Social Order. By PAUL A. BARAN and PAUL M. SWEEZY. New York and London: Monthly Review Press, 1966. Pp. xiv, 402. $8.75.

This book is worth reading; it deals with a vital area of economics, has a unique approach, is stimulating and well written. It represents the first serious attempt to extend Marx's model of competitive capitalism to the new conditions of monopoly capitalism. Even non-Marxists who read it will find some important additions to the economist's toolbox.

Paul Baran and Paul Sweezy begin with a long and careful analysis of price and profit decision-making under oligopoly conditions. They find that the modern large corporation still practices profit maximization, though over a longer time horizon and with *more* rational calculation than ever before. They find complete elimination of price competition, replaced by coordinated policies among firms (without need of any overt conspiracies). They further conclude that the degree of monopoly continues to increase, and that the increasing degree of monopoly—coupled with the astounding rise of productivity—has caused a very rapid increase in the "economic surplus."

This "surplus" is defined as "the difference between what a society produces and the costs of producing it." It is broader than the traditional concept "surplus value" (or profit, rent, and interest) because it also includes wasteful and nonproductive expenditures. The problem is the lack of effective demand to absorb the entire surplus. Basically, where Marx argued that competitive capitalism suffers cycles of boom and bust (but said "there are no permanent crises . . ."), Baran and Sweezy see monopoly producing a permanent tendency toward slowed growth and stagnation.

Some surplus is absorbed by capitalist consumption and investment, but they contend that both of these are relatively limited by the cautious outlook of oligopoly corporations with respect to dividends and new investment. Yet they believe that most surplus *is* absorbed, but by new institutional channels. They calculate the whole surplus in 1963 at 56.1 per cent of GNP. Of this total surplus, however, only 32 per cent was the traditional rent, interest, and profit (which must be offset by capitalist consumption or investment). Another 17 per cent of the surplus was spent on the "sales effort," in which they include a large part of all distribution costs, most advertising, most model and style changes, finance, insurance, real estate, and legal services. Finally, a huge 51 per cent of the surplus is absorbed by government spending, in which they include both warfare and welfare expenditures. The contention is that these latter elements of surplus absorption not only cut down on workers' consumption, but also on productive investment, so that they lower the rate of growth.

In spite of all these avenues of surplus absorption, however, they contend that all the potential surplus is still not absorbed, with the result that there is a certain amount of chronic unemployment. The question arises why government spending cannot absorb an unlimited surplus. They argue that private interests are no longer opposed to government spending per se, but will still oppose most nonmilitary spending, either because it competes with private enterprise, e.g., low-cost housing, or because it undermines class privileges, e.g., high-quality public education for all. Only government spending on highways is fully supported by big business, but this is already reaching a point greatly in excess of usefulness (since the auto is choking urban transport).

This leaves military spending, which does not conflict with private interests, but rather provides exceptionally high profit rates. Yet they argue that even military spending cannot be increased enough to provide peacetime full employment, in part because an increasing proportion goes into scarce research and engineering skills, and a decreasing proportion into mass production. Interestingly, the authors also argue—contrary to Hobson and Lenin—that foreign investment does *not* offer a vast area of surplus absorption. They point out that the profit and interest flow to the United States (earned by our huge "multi-national corporations") has been greater than the entire investment flow abroad.

They recognize that U.S. history since the Civil War (approximately the period of "monopoly capitalism") does not actually reveal such continuous unemployment nor stagnation. The model is accordingly modified to recognize the vast amounts of surplus absorbed at times by (a) wars and (b) major innovations (namely railroads and autos).

Having elaborated the economic model, they turn to its effects on society in the last three chapters. They argue that race prejudice has been an inevitable concomitant of the exploitation of the Negro under slavery and as the cheapest labor under monopoly capitalism. They also consider the effects on juvenile delinquency, divorce, and a whole range of expressions of pervasive alienation. Included are powerful essays on poverty, a long analysis of housing conditions, and a very detailed discussion of educational deficiencies.

Without detracting from the general importance of the book, some weak spots may be mentioned. First, the main point of the new "surplus" concept turns out to be the vast waste of social resources on war and war production. This could be handled conceptually as well, however, in terms of a deduction from Marx's surplus value; and could be more precisely calculated as a factor lowering the rate of growth within the terms of a Domar-type growth model. Secondly, the concept of "nonproductive" expenditures on "sales effort" and other capitalist "waste" is contrasted to a perfect socialist economy, but they neglect to contrast it to any actual socialist economy.

Third, how does the remaining *cyclical* unemployment relate to the long-run stagnation model of the monopoly stage? Fourth, granted that the degree of monopoly affects the distribution of the surplus among industries, by what mechanisms does it affect the *aggregate* surplus? Finally, their reasoning as to the limitations of military spending appears insufficient. Given the lack of political obstacles, it seems that *some* level of military spending will always create full employment (granted that it may be an absurdly high level, may lower the growth rate, and may tend to produce nuclear holocaust).

HOWARD J. SHERMAN

University of California, Riverside

The New Economics. By E. PREOBRAZHENSKY. Translated by BRIAN PEARCE, with an introduction by A. NOVE. Oxford: Clarendon Press, 1965. Pp. xx, 310. 55s.

This is a translation of a work which was originally published, first in article and then in book form, in the mid-1920's during the period when Stalin was consolidating his power and a great debate over the Soviet Union's economic course was raging. E. Preobrazhensky was the leading economic theorist of the Left Opposition and was subsequently executed in the purges of the 1930's. The book was of course suppressed in the Stalin era, and apparently neither it nor its author has been rehabilitated since. We owe its translation into English (a French translation has also just been published) at this time to the great increase in interest in problems of economic development which has been so marked a characteristic of the postwar period.

In choosing *The New Economics* for his title, Preobrazhensky was certainly not claiming to have discovered some brand new economic principles. He was an orthodox Marxist in the Leninist mold, and his purpose was to apply Marxian methods and categories to the Soviet economy as it existed in the period of the New Economic Policy. What was "new" was the character of this economy: all the theoretical tools and concepts were taken, with but few

modifications, from Marx. This will doubtless make the book difficult going for non-Marxists; some may even find it quite incomprehensible. It is perhaps worth noting, therefore, that as a Marxist I found Preobrazhensky's exposition a model of clarity and lucidity, a far cry from the kind of "Marxian" scholasticism which has become so common in recent decades. The high intellectual level, the evident scientific spirit, and the absence of jargon and dogma in Preobrazhensky's work make one realize how seriously Marxism was set back in the USSR—and not only in the USSR—as the result of the ideological needs of the Soviet leadership in the period of forced industrialization, war, and cold war.

Where bourgeois economics has always sought to find and emphasize the elements of harmony, equilibrium, and cooperation in a socioeconomic system, Marxism has done just the opposite in the conviction that both structure and change are dominated by contradiction, opposition, and struggle. In keeping with this approach, Preobrazhensky saw the Soviet Union in the 1920's as a strictly transitional society composed of two totally opposed systems locked in mortal combat. One of these systems was simple commodity production (peasant agriculture, handicraft production, petty trade), the other was that of emerging socialism (industry, transportation, communications, finance, all owned by the state). The private economy was governed by the "law of value," which in Marxian usage is roughly equivalent to Adam Smith's "invisible hand"—in other words, it responded to and was shaped by competitive market forces. If the socialist sector should content itself with the role of catering to the needs of the still much larger private economy, it would become a mere appendage of the latter and sooner or later would be disintegrated and taken over: this, in short, would be the road to the restoration of capitalism. The alternative was for the state deliberately to build up the socialist sector, not in response to the pattern of effective demand generated by market forces, but with a view to the transformation of the country's economic structure and the eventual absorption of the private economy. This was the road to a fully socialist society.

Preobrazhensky was of course a passionate partisan of the socialist alternative, and he took it for granted that the Soviet state was committed by its class character to follow this road. His central purpose in *The New Economics* was to spell out what this implied and to identify and analyze the difficulties and obstacles which would have to be overcome.

This background should help to clarify what Preobrazhensky meant by the "law of primitive socialist accumulation," which he insisted was the governing law of the socialist sector of the Soviet economy and which he counterposed to the law of value as the governing law of the private sector. "Primitive accumulation," it will be recalled, was the term Marx applied to the appropriation, usually by violent methods, by the nascent capitalist class of wealth (land, precious metals, currently produced surpluses) of precapitalist countries and population strata.[1] This process, Marx held, was essential to the es-

[1] Marx actually used the word *ursprünglich,* which would have been better translated by its literal equivalent "original"; what he wanted to stress was that this kind of forcible accumulation *preceded,* and laid the foundations for, capitalist accumulation proper.

tablishment of capitalism, and later on the continued plundering of precapitalist peoples added to and accelerated the process of capitalist accumulation. "Primitive socialist accumulation," as seen by Preobrazhensky, was an analogous process whereby the nascent socialist state appropriates the surplus product of the private sector and channels it into the socialist sector to speed up the growth of the latter. Preobrazhensky believed that the socialist state was precluded by moral and political considerations from having recourse to the violent methods used by capitalism; its primary methods would have to be taxation and a monopoly price policy by state-owned industries. Little did he guess what violent forms the process would take under Stalin's forced-collectivization program only a few years later!

By speaking of a "law" of primitive socialist accumulation, Preobrazhensky meant no more—and no less—than that the policy of taking from the private sector to accelerate the growth of the public sector was one which, in the conditions which existed in the USSR in the 1920's, the socialist state must necessarily adopt in order to survive as a socialist state. This is not the same meaning as the term has in the "law of value" where it signifies certain regularities which impose themselves regardless of the will, or even knowledge, of the people involved. The reader who keeps in mind that Preobrazhensky used the word "law" in these two quite different senses will find it easier to follow his arguments.

The focus of all these arguments was the policy of the Soviet state at the time the book was being written, and many of them are couched in a highly polemical form. For these reasons it seems clear that a reasonable knowledge of the historical context is essential to get the most out of Preobrazhensky's work.[2] This fact, however, should not be allowed to give rise to the assumption that *The New Economics* has only an historical interest. Many of the problems which were faced by the USSR in the 1920's exist today in countries which are struggling to break out of the prison of underdevelopment, and anyone who is interested in them, and in particular in working out viable solutions for them, is sure to find this a stimulating and valuable work. I subscribe wholeheartedly to the judgment of Ernest Mandel, the Belgian Marxist who wrote the Introduction to the French translation of the book (*La Nouvelle Economique,* Paris, 1966): ". . . one will not find in this book a code of conduct for economic leaders of underdeveloped countries engaged in the transition from capitalism to socialism. But one does find in it an arsenal of concepts and chains of analysis which permit the freeing of the elaboration of political economy in these countries from the grip of empiricism and vulgar pragmatism. One finds the essential instruments for defining, in all such cases, the great options and the lines of possible evolution. *The New Economics* is not only a guide to the better understanding of these basic problems and thus to an escape from debilitating pragmatism. It is also a weapon for cutting these Gordian knots, not in the sense of some academic truth but in the inter-

[2] Apart from general works on Soviet history, the most useful companion volume to *The New Economics* is Alexander Erlich, *The Soviet Industrialization Debate 1924-1928,* Cambridge, Mass., 1960.

ests of a class and of a cause to which Preobrazhensky devoted his whole life: the working class and the cause of socialism."

PAUL M. SWEEZY

New York, N.Y.

Private and Public Planning. By NEIL W. CHAMBERLAIN. New York: McGraw-Hill Book Co. 1965. Pp. viii, 238. $7.95.

Professor Chamberlain's study is about private and public planning in the proportion of one rabbit to one horse—40 pages on private planning and 174 on public, with 16 on definitions used in both. The discussion of private planning is intended to introduce and illustrate certain ideas which will then be applied to the analysis of public planning. These ideas constitute, according to the author, "whatever contribution this study makes." They include "the conception of planning as the systematic management of assets, the inescapable conflict of objectives between system and subsystem, and the consequent dependence not only on technical-economic but also organizational-political coordination; the ongoing nature of the planning process, with a continuing interplay between intent and event, and the significance of the distinction between what are here called specific and categorical social objectives."

We may have encountered these ideas before in plainer garb. I must confess a certain irritation at the repeated use of "subsystems" or, worse, "subunits" for what turns out to be "people." Nevertheless, these would probably be a useful set of ideas for analyzing public planning. The distinction between the technical-economic and organizational-political aspects seems to me especially promising. However, an analysis of public planning in these terms does not come through. Rather we get an anecdotal account of government planning in Western Europe in the postwar period on which these ideas have been plastered without apparent necessity or effect. The reason is, I think, that we just do not know enough about what is really going on in the public planning process, about the objectives of those who do it, about the techniques employed, and about the results achieved or thought to have been achieved, to subject the process to any sophisticated analysis. It is as if we are listening to a professor of art analyze a painting in a room so badly lit that we can't see the object. Symptomatic of how little we know about the object being analyzed is the fact that in the leading case, French "indicative" planning, even the practitioners don't know, or disagree about, what the method is.

Chamberlain starts with a definition of planning as the systematic management of assets, something that even U.S. businesses do and that therefore is not the property of the Socialists or the Communists. The operation of the U.S. Department of Defense under Secretary McNamara is cited as "perhaps the purest, although partial, example of governmental planning in the West today." But that is the last we hear of the Department of Defense. Chamberlain is not interested in partial planning, however pure. He is interested in comprehensive and detailed management of the economy, however, impure. Whether the kinds of government policies to which this book is mainly devoted meet the author's definition of planning as "systematic" is never asked,

although it is pretty obvious that most of them do not. The test applied is not that the government's management of the "nation's" (whose?) assets should be systematic, but that there should be a lot of it.

The author probably did not intend to give comfort to those who believe that any government "intervention" in the economy is the narrow edge of the wedge leading inevitably to total dictatorship. But this is precisely the impression given. From modest origins, the objectives and necessary techniques of government economic management grow in this short and undramatic book by inexorable logic until the requirements of "planning" threaten to devour all other social institutions. In the end, even the poor old Congress is on the verge of mummification because it does not fit in with a long-run, expert, and functionally representative management of our economy.

As one example of this inevitability, the really very interesting discussion of incomes policy may be cited. "However reluctantly governments have been driven to the conclusion, a price and incomes policy is an essential ingredient in management of the economy." There follow a number of convincing pages showing that none of the policies heretofore tried has worked or could work. Then we are treated to several new, "unorthodox" suggestions, each more quixotic than the last. But still, there is no government management without incomes policy, and if you can't make an incomes policy work, have one anyway.

The basic problem with this book is not that it is a very ideological book or that its ideological argument is thin, both of which are true. The basic problem is that the book doesn't tell the reader very much about postwar economic policies and actions (to avoid the charged word "planning") in Western Europe. There is no consecutive account of what happens in any country on any subject. Somewhere between systems theory and ideology, the facts got lost. But there is much internal evidence that Chamberlain has had exceptional opportunities to learn, and knows a great deal about what has been going on in Western Europe. If he would now write this down, he would make a real contribution.

HERBERT STEIN

Center for Advanced Study in the Behavioral Sciences

Business Fluctuations

American Economic Association Readings in Business Cycles. Edited by ROBERT AARON GORDON and LAWRENCE R. KLEIN. Homewood, Ill. Richard D. Irwin, Inc., 1965. Pp. x, 731. $8.50.

Random shocks, acceleration principle, ratchets, permanent income, and econometrics! Take your pick, or put them all together, and you have the business cycle. Or do you?

The current sequel to the AEA's *Readings in Business Cycle Theory*, which played a major part in the graduate education of the post-World-War-II generation of economists, has a couple of welcome prewar articles but otherwise is devoted to papers reflecting work of the last two decades. It gives good rep-

resentation to the resurgence of flexible accelerator models in an initial section on "Theory," which includes an article by Goodwin, Hicks's review of Harrod's *Towards a Dynamic Economics*, presenting the guts of his own *Trade Cycle*, the Smithies (1957) *Econometrica* article adding ratchet effects and a profit-dominated investment function, Minsky's "A Linear Model of Cyclical Growth," and a delightfully clear, classic paper on inventory cycles by Metzler (*Review of Economic Statistics*, 1941). The section on theory is concluded with a Haberler effort to redress the balance, as he sees it, by emphasizing the role of monetary factors.

The next group of reprints, on "Methodology," contains Frisch's (1933) "Propagation Problems and Impulse Problems in Dynamic Economics," which it is particularly good to have accessible. The section also includes the Koopmans-Vining exchange, which holds up rather well and may be read with interest or profit by the old and the young and will probably have little influence on most practitioners.

Section III on "Econometric Models" comprises the Duesenberry-Eckstein-Fromm "A Simulation of the United States Economy in Recession," the examination of "The Dynamic Properties of the Klein-Goldberger Model" by the Adelmans, and Christ's review of models of Tinbergen, Klein, Klein and Goldberger, and himself.

Section IV on "Studies of Particular Variables" offers Kisselgoff and Modigliani and Jorgenson on investment, Ando and Modigliani and Farrell on new theories of consumption, and Friedman's 1958 *Journal of Political Economy* application of the permanent income concept to the demand for money. It also includes Lipsey's discussion of Phillips curves of unemployment and wage-rate changes (1960) and a 1962 report by Geoffrey Moore on work of the National Bureau of Economic Research.

Section V on "Long Cycles" includes articles by Abramovitz and O'Leary and by W. Arthur Lewis. Section VI on "International Aspects" contains a bold, interesting econometric effort by Polak and Rhomberg at analyzing the transmission of disturbance between broad groups of nations. In Section VII on "Forecasting," Suits points with understandable pride (1962) at results of his continuing use of an econometric model, and Alexander and Theil and Jochems report on diffusion indexes in the analysis of economic and business data.

The final section on "Policy" includes one of Phillips' interesting and sobering papers exploring the dynamic implications of some stabilization mechanisms. It also contains an excellent presentation of the balanced budget theorem by William Salant and articles by Heller and Schlesinger, differing predictably in their evaluations of automatic and discretionary monetary and fiscal policies.

There is much that one can say, article by article, about a volume of papers such as these, frequently by distinguished authors, and clearly designed to sample major aspects of recent work on business cycles. I will permit myself a number of scattered observations, both critical and approving, and then some general reflections.

While the editors explicitly eschew attention to growth models, much of the

theoretical section has an underlying motif of Hicksian fluctuations around a long-run growth path bounded by ceilings and floors. It might be well, therefore, to rehabilitate the oft-criticized "autonomous investment" which is Hicks's exogenous moving force. This may be done by a simple assumption and semantic twist. For "autonomous investment" read "autonomous expenditure" and assume that *this* grows at a more or less constant rate dictated by growth in population and growth in the minimum standard of living which people will insist upon maintaining by means of dissaving, borrowing, private charity or government expenditure or relief. All of this may be imbedded in the ratchet effects of Duesenberry, Eckstein and Fromm, Smithies, and Minsky, but someone ought to set it forth in terms of either individual maximizing behavior or the necessary assumption for viability of the socioeconomic system whose behavior we are trying to explain.

Having referred to growth models, I cannot let pass the opportunity to correct again a widespread misinterpretation of Harrod's warranted rate of growth. For Smithies, unfortunately in good and numerous company, repeats the mistaken argument that the warranted rate of growth is a full-employment rate. But Harrod was quite explicit in asserting that his "G_w is the entrepreneurial equilibrium; . . . in Keynesian fashion he contemplates the possibility of growing 'involuntary' unemployment" (*Towards a Dynamic Economics*, p. 87). And this, I would insist, is quite basic to the analysis.

It is interesting to reread Frisch's article setting forth the role of erratic shocks in the light of the Adelmans' analysis of the Klein-Goldberger models and a pregnant observation by Haberler on the "contributions" of the major periodic shocks of wars. But in view of the large role in business cycles attributed by many to fluctuations in investment, these might have somewhere received further, systematic attention. (Or is this reviewer being too parochial?) For one thing, I continue to view with some dismay the argument that "the dominant influence on the rate of gross investment decisions is the level of present or past gross profits" (Smithies, p. 43). This is not readily squared with appropriate theory that investment relates to the expected contribution of investment to profits and not expected profits per se, let alone past or current profits. The Kisselgoff-Modigliani article puts the matter in better theoretical and empirical perspective but is cautious in generalizing the particular characteristics of the electric power industry to which it relates. Jorgenson does set forth a general theoretical model but may unfortunately mislead the reader, in his cryptic presentation, as to the extent to which his results reflect empirical findings as opposed to the specific assumptions of his model. In particular, Jorgenson presents empirical estimates of distributed-lag relations between his measures of actual and desired capital stock. But his "responses and elasticities of investment with respect to changes in market conditions and tax structure" (p. 377) reflect essentially his own assumed relation between equilibrium capital stock and prices of output and capital, rate of interest, and tax and depreciation rates. No empirical evidence of separately identifiable roles of these factors is presented.

It is particularly welcome to have income expectations and assets brought explicitly into treatment of the consumption function, as in the Ando and Mo-

digliani and Fisher articles. This is surely more sound than earlier empirically oriented reliance on previous income peaks and ratchet effects. Farrell's references to acceleration-type investment in consumer durables and capital gains are also welcome. Sound economic treatment of the role of capital gains in income and consumption is indeed long overdue.

Lipsey's interesting review of the relation between wages and unemployment, perhaps inadvertently highlighting major gaps in our understanding, offers a theoretical rationale which dwells on supply and demand relations really meaningful only for individual industries. Empirical estimates of a dynamic theory appropriate at the aggregative level are not to be found. Nor indeed do we see attention to the possibly varying effects over the cycle of imperfectly competitive forces in both product and labor markets.

It was good to see Alexander's warning on the false as well as correct leads noted in diffusion indexes. And Theil and Jochems offer some jolting information on the uncertainty or inaccuracy of business responses on *ex post*—let alone *ex ante*—data. This may suggest to some the disutility of sophisticated theoretical and statistical treatment of business data. I see rather a cue for more stubborn persistence in applying data to theoretical relations that make sense rather than merely following "the facts" in broadly impressionistic interpretations. (I fear that I am relatively unmoved by many of the charts and tables of conformities reported by the National Bureau of Economic Research and its associated scholars. I prefer explicitly stated mathematical relations and estimates of coefficients and standard errors.)

But this may well lead to a general observation. While one can hardly expect a collection of 31 separate pieces by about as many different authors to present a unified analysis of the business cycle, and while one should congratulate the editors for choosing a broad sample of the various areas of activity, one may be impressed by the numbers of prominent economists working without contact with any central theme. Much of the theoretical work involves the construction and manipulation of fairly mechanical relations with no clear grounding in maximizing or sensible behavior. The econometric studies are frequently forced in the direction of such mechanical relations by lack of appropriate data. But we really do not want another generation arguing about whether consumption is a linear function of this period's income or last or whether investment must "lead" the peak in consumption.

For people should not be expected to react to the past. Decisions must relate to initial conditions and expectations of the future. And here is where the most intractable difficulties develop. It is very difficult to find a stable relation between expectations of the future and observable past variables or initial conditions. Changes in taxes, government expenditures, monetary policy, consumer demand, and income may all have quite different effects as they relate differently to expectations of the future. Analysis and prediction of business cycles will remain sharply limited in their power as long as this nexus from past to present to future is so incompletely specified.

It is a merit of this volume and of the work which it samples that some of these considerations, along with the use of more systematic econometric techniques for the application of data to theory, have come to the fore. The cur-

rent generation of graduate students and their instructors should find it a useful introduction to recent research and analysis of business cycles.

ROBERT EISNER

Northwestern University

Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Determinants and Effects of Changes in the Stock of Money, 1875-1960. By PHILLIP CAGAN. National Bureau of Economic Research Studies in Business Cycles No. 13. New York and London: Columbia University Press, 1965. Pp. xxviii, 380. $10.00.

A persistent association of money stock, prices, and output has been demonstrated for many different periods and institutional environments. The observations have been interpreted in three different ways. One attributes the pattern to a causation running from money to prices and output. Another notion reverses the line of causation, and a third admits a mutual interaction of real and monetary processes. The evaluation of the rival interpretations usually relied on a more or less detailed analysis of the chain linking money with output. Economists have only recently become aware that a study of money-supply patterns provides useful information bearing on the comparative relevance of the alternative interpretations.

The book authored by Phillip Cagan and recently published by the National Bureau of Economic Research thoroughly exploits these opportunities. It begins with an excellent description of the major issues. This material is systematically organized with the aid of an analytic framework which reduces variations in the money stock (measured in the inclusive sense following the Friedman-Schwartz procedures) to changes in high-powered money H (i.e., the monetary base), the banks reserve ratio r, and the currency ratio c. The formula $M = H[c(1\text{-}r) + r]^{-1}$ also guides the author's search for useful hypotheses. The formula has, by itself alone, no empirical content. This poses no problem so long as the author expands beyond the initial statement in a search for fruitful explanations of the entities H, r, and c proximately "determining" the money stock. His search begins with a careful description of the secular and cyclic movements of the money stock and the relative contributions made by the three proximate determinants. The results deserve a close scrutiny by monetary theorists and in particular the most diligent attention of future textbook writers.

This detailed examination of secular and cyclic patterns of the money stock yields useful clues bearing on the nature of the money-supply mechanism. These clues are systematically explored in subsequent chapters. The inquiry opens with a thorough and penetrating analysis of secular and cyclic movements of the monetary base. These studies successfully mesh description and analysis. The shifting arrangements governing the supply of base money are patiently investigated and their manner of operation carefully traced. Changes in the gold stock, silver purchases, Treasury operations, and the issue of na-

tional bank notes determined before 1914 the changes in the base. After 1914 the portfolio of Federal Reserve Banks loomed as an important determinant. The background of these source-components is illuminated, their behavior characterized and explained, and their relative importance indicated.

The cyclic patterns underlying the movement of the base are singularly instructive. Both Chart 7 on page 102 and Chart 24 on page 271 show a remarkable change in the cyclic behavior of the base around World War I. Before this period the growth of the base reached a peak in stage VII, about midway during the economic downswing. It rose sharply before and fell even more decisively in subsequent stages. After the war a peak was reached in stage III or IV; in the later phases of economic expansion and before the peak of activity. There followed a retardation through most of the economic decline until after stage VIII, or almost the end of the downswing. The charts also demonstrate that this substantial change in cyclic patterns cannot be attributed to the gold movements, but resulted from the behavior of the nongold sources of the base, which was essentially from the emergence of Federal Reserve credit. An adequate explanation of these observations thus leads us to the peculiar policy conception centered originally on bank borrowing and later on free reserves which has dominated the policy-makers' view since the 1920's. It can be shown that policies emanating from a "free reserve conception" generate the behavior of Federal Reserve credit so intriguingly described by the author's charts.

The role of the currency ratio has rarely been properly recognized in the literature. Here the author pioneers with his investigations of the secular and cyclic movements of this ratio. His efforts certainly contribute to the emergence of a reasonably satisfactory explanation of the longer-run behavior of the currency ratio. On the other hand, the cyclic evolutions of the ratio remain substantially unexplained. It is not clear whether the pattern conforms positively or inversely to the cycle. The author's choice of an inverse pattern is not convincing at this stage. The unsatisfactory state of affairs is also clearly revealed in the attempted explanations. The suggestions offered by Mitchell and Hawtrey yield very little, but neither can I find the author's suggestion of linking currency demand with a permanent expenditure concept very helpful. It may be useful to reconsider the author's framework guiding his search for explanations. The ratio is exhibited as a product of three factors: the ratio of currency to consumers' expenditures, income velocity, and the ratio of consumers' expenditures to income. The author attempts to explain the currency ratio in terms of the three new ratios. The underlying idea appears very sound indeed. The factoring decomposes the ratio into separate entities which permit a useful classification of a host of relevant substitution processes reflected by the currency ratio. But I would argue that the first and second factors substantially overlap and effectively prevent a useful separation. For precisely this reason Meltzer and I worked with the ratio of currency to check deposits (in lieu of Cagan's ratio to the inclusive money stock). The more narrowly conceived currency ratio can be equated with the product of two ratios, viz., the ratio of transactions-velocity of deposits to the transactions-velocity of currency and the value of currency transactions per dollar of deposit transactions. The difficulties encountered on the author's path suggest that the alter-

native approach may be usefully explored.

The literature has frequently emphasized the potentially volatile redistribution of deposits and the consequent serious obstruction to monetary policy. The chapter on the reserve ratio effectively eliminates such useless speculations. A solidly detailed investigation of all kinds of deposit redistributions (between national and nonnational banks, between member banks and nonmember banks, between classification of member banks, between time and demand accounts) establishes that these changes exert in general a minor influence on the banks' average reserve ratio. They explain consequently only a vanishing portion of the (inclusive) money stock's behavior. This result carries over to the exclusive money stock with the exception of the distribution between time and demand deposits. It should also be noted that the author's results bearing on the latter distribution are conditional on his choice of a currency ratio. Under the alternative approach noted before, the redistribution between demand and time deposits operates also through a channel independent of the reserve ratio on the money stock. The alternative approach assigns therefore a substantially larger effect to redistributions between time and demand accounts.

Reserve requirements shaped the banks' average reserve ratio much more importantly than deposit redistribution. It has been argued on occasion that changes in requirements only modify the "usable ratio," i.e., the ratio of reserve money in excess of requirements to total deposits, with no effect on the total ratio. Cagan investigates this issue carefully and finds that the usable ratio does not depend significantly on the legal ratios. In particular, he finds that after 1948 the usable ratio returned speedily to the previous level whenever requirements were changed. Before 1948 the usable ratio apparently absorbed a good portion of the legal changes. However, the author's interpretation of the observed movement is not sufficiently supported. Compensatory open-market policies accompanied every change in requirement ratios from 1948 to 1960. Compensation was usually very pronounced and quite immediate. This implies that changes in required reserves were matched by closely corresponding changes in total reserves. The immediate return to usable ratios thus reflects to a large extent such compensatory policies and yields little evidence with respect to the banks' adjustment to changes in legal ratios.

The usable reserve ratio forms the last explanatory component of the reserve ratio explored by Cagan. He wrestles first with explanations of its remarkable decline over the period studied. Market rates of interest move in the wrong direction and thus cannot explain the movement. A variety of institutional changes are mentioned by the author whose joint effect lowered the marginal benefit of usable reserves. His argument appears convincing. Nevertheless, one wonders whether the same or related changes did not simultaneously lower relevant marginal costs. Marginal costs of negotiating and transacting, or marginal costs of information bearing on loan customers, or marginal costs of portfolio readjustments may have fallen substantially since the early 1870's. Their reduction would also have lowered the usable ratio. Further research appears appropriate on this point.

The analysis of cyclic patterns carefully separates the effect of panics and severe depression. The effect of the latter is most likely negligible, whereas the

tional bank notes determined before 1914 the changes in the base. After 1914 the portfolio of Federal Reserve Banks loomed as an important determinant. The background of these source-components is illuminated, their behavior characterized and explained, and their relative importance indicated.

The cyclic patterns underlying the movement of the base are singularly instructive. Both Chart 7 on page 102 and Chart 24 on page 271 show a remarkable change in the cyclic behavior of the base around World War I. Before this period the growth of the base reached a peak in stage VII, about midway during the economic downswing. It rose sharply before and fell even more decisively in subsequent stages. After the war a peak was reached in stage III or IV; in the later phases of economic expansion and before the peak of activity. There followed a retardation through most of the economic decline until after stage VIII, or almost the end of the downswing. The charts also demonstrate that this substantial change in cyclic patterns cannot be attributed to the gold movements, but resulted from the behavior of the nongold sources of the base, which was essentially from the emergence of Federal Reserve credit. An adequate explanation of these observations thus leads us to the peculiar policy conception centered originally on bank borrowing and later on free reserves which has dominated the policy-makers' view since the 1920's. It can be shown that policies emanating from a "free reserve conception" generate the behavior of Federal Reserve credit so intriguingly described by the author's charts.

The role of the currency ratio has rarely been properly recognized in the literature. Here the author pioneers with his investigations of the secular and cyclic movements of this ratio. His efforts certainly contribute to the emergence of a reasonably satisfactory explanation of the longer-run behavior of the currency ratio. On the other hand, the cyclic evolutions of the ratio remain substantially unexplained. It is not clear whether the pattern conforms positively or inversely to the cycle. The author's choice of an inverse pattern is not convincing at this stage. The unsatisfactory state of affairs is also clearly revealed in the attempted explanations. The suggestions offered by Mitchell and Hawtrey yield very little, but neither can I find the author's suggestion of linking currency demand with a permanent expenditure concept very helpful. It may be useful to reconsider the author's framework guiding his search for explanations. The ratio is exhibited as a product of three factors: the ratio of currency to consumers' expenditures, income velocity, and the ratio of consumers' expenditures to income. The author attempts to explain the currency ratio in terms of the three new ratios. The underlying idea appears very sound indeed. The factoring decomposes the ratio into separate entities which permit a useful classification of a host of relevant substitution processes reflected by the currency ratio. But I would argue that the first and second factors substantially overlap and effectively prevent a useful separation. For precisely this reason Meltzer and I worked with the ratio of currency to check deposits (in lieu of Cagan's ratio to the inclusive money stock). The more narrowly conceived currency ratio can be equated with the product of two ratios, viz., the ratio of transactions-velocity of deposits to the transactions-velocity of currency and the value of currency transactions per dollar of deposit transactions. The difficulties encountered on the author's path suggest that the alter-

native approach may be usefully explored.

The literature has frequently emphasized the potentially volatile redistribution of deposits and the consequent serious obstruction to monetary policy. The chapter on the reserve ratio effectively eliminates such useless speculations. A solidly detailed investigation of all kinds of deposit redistributions (between national and nonnational banks, between member banks and nonmember banks, between classification of member banks, between time and demand accounts) establishes that these changes exert in general a minor influence on the banks' average reserve ratio. They explain consequently only a vanishing portion of the (inclusive) money stock's behavior. This result carries over to the exclusive money stock with the exception of the distribution between time and demand deposits. It should also be noted that the author's results bearing on the latter distribution are conditional on his choice of a currency ratio. Under the alternative approach noted before, the redistribution between demand and time deposits operates also through a channel independent of the reserve ratio on the money stock. The alternative approach assigns therefore a substantially larger effect to redistributions between time and demand accounts.

Reserve requirements shaped the banks' average reserve ratio much more importantly than deposit redistribution. It has been argued on occasion that changes in requirements only modify the "usable ratio," i.e., the ratio of reserve money in excess of requirements to total deposits, with no effect on the total ratio. Cagan investigates this issue carefully and finds that the usable ratio does not depend significantly on the legal ratios. In particular, he finds that after 1948 the usable ratio returned speedily to the previous level whenever requirements were changed. Before 1948 the usable ratio apparently absorbed a good portion of the legal changes. However, the author's interpretation of the observed movement is not sufficiently supported. Compensatory open-market policies accompanied every change in requirement ratios from 1948 to 1960. Compensation was usually very pronounced and quite immediate. This implies that changes in required reserves were matched by closely corresponding changes in total reserves. The immediate return to usable ratios thus reflects to a large extent such compensatory policies and yields little evidence with respect to the banks' adjustment to changes in legal ratios.

The usable reserve ratio forms the last explanatory component of the reserve ratio explored by Cagan. He wrestles first with explanations of its remarkable decline over the period studied. Market rates of interest move in the wrong direction and thus cannot explain the movement. A variety of institutional changes are mentioned by the author whose joint effect lowered the marginal benefit of usable reserves. His argument appears convincing. Nevertheless, one wonders whether the same or related changes did not simultaneously lower relevant marginal costs. Marginal costs of negotiating and transacting, or marginal costs of information bearing on loan customers, or marginal costs of portfolio readjustments may have fallen substantially since the early 1870's. Their reduction would also have lowered the usable ratio. Further research appears appropriate on this point.

The analysis of cyclic patterns carefully separates the effect of panics and severe depression. The effect of the latter is most likely negligible, whereas the

former induces a prolonged and slowly decaying response. The author acknowledges that interest rates do play a role. A concern with gross association permits, however, the discovery of their effects only for periods dominated by extreme variations in market rates. It is no surprise therefore than an effect of interest rates can only be discerned for the 1930's. The author concludes ultimately that interest rates cannot explain the cyclic changes in the usable ratio and associates these changes through loan demand with business conditions. His argument remains too vague to be convincing. The cyclic role of interest rates is probably submerged by their joint interaction with marginal costs of loan-extension and portfolio readjustment.

The detailed investigations of money-supply mechanisms are ultimately exploited for an evaluation of the lines of causation linking money, prices, and economic activity. If causation runs essentially from the economy to money, then it must be shown how the base, the currency, and reserve ratio are determined by economic evolution. The author thoroughly and convincingly argues that the secular patterns investigated are inconsistent with a causation from economic activity to money. The cyclic movements on the other hand are more ambiguous. They reveal a substantially greater dependence for money supply on economic activity and the causation from money to output and prices is difficult to separate. The author selects therefore a number of special events for a detailed study. Wartime inflations and severe depressions yield substantial evidence for the relevant causal operation of money. The author still concludes that most of the cyclic variations in the money stock are due to changing business conditions. This statement is quite consistent with a pronounced line of causation from money to business, but recent observations raise serious doubts concerning the author's contentions. We computed for each postwar cycle the ratio of (both exclusive and inclusive) money stock at the end of each half-cycle to the money stock at the beginning of the half-cycle. Similar ratios were computed for base and monetary multiplier. Rank correlations between money ratios, base, and multiplier ratios emphasize the dominant position of the base over half-cycles. The cyclic dependence of money on business would thus be mediated through the policy-makers' conception underlying the cyclic behavior of the base. One wonders how much dependence would remain should the conception miraculously change to generating a regular growth in the base. It is not unlikely that under these circumstances the cyclical dependence of the money stock on business would be seriously attenuated.

The book contains many excellent passages worthy of attention. There also remain serious unresolved issues, and the careful reader will be left on occasion with substantial doubts and open questions. It is however difficult to criticize the author severely for these inadequacies when we consider that systematic investigations of money-supply mechanisms re-emerged only recently. Since Cagan's first contribution to monetary theory, one has become accustomed to expect his efforts to yield a useful product of quality. Our expectation has not been in vain.

KARL BRUNNER

Ohio State University

Money, Interest, and Prices—An Integration of Monetary and Value Theory. 2nd ed. By DON PATINKIN. New York: Harper & Row, 1965. Pp. xxv, 708.

The reviewer of the second edition of an already-celebrated book has both a duty and an opportunity: a duty to report on the new material and an opportunity to present a revaluation of the book as a whole. No author could have been more helpful to a reviewer in the first task. Not only are the main changes announced in the Preface, but a detailed account of them is also given in a supplementary note. Two of the more important changes can be accepted as unqualified improvements. These are, first, the incorporation of the Gurley-Shaw distinction between "inside" and "outside" money into the analysis of issues concerned with the neutrality of money and other related matters in Chapter 12; secondly, the incorporation, in the particular context of the simplified money-and-goods model of Chapter 3, of the Archibald-Lipsey analysis of long-run equilibrium.

I have some reservations, however, concerning the most substantial revision of all. This is the elaboration in Chapters 5-7, based on the work of Baumol, Markowitz, and Tobin, of the microeconomic theory of money. Two alternative explanations of the demand for money are presented. In the first place, money is regarded as a consumer good, the holding of which gives subjective utility to the individual. In the second place, money is regarded as a producer's good, and money is held only as a means of achieving a more desirable basket of commodities over time. While there is much that is of value and of interest in these chapters, some of the analysis seems to raise more questions than it answers. For example, in the utility approach the holding of money is regarded as having utility for the individual by giving him a sense of security, of freedom from likely trouble and embarrassment. But should one stop there? The author himself goes a bit further in the Mathematical Appendix, where he treats of a model in which bonds as well as money have liquidity. But even this may not be going far enough. Any departure from the assumption that the consumption of commodities is the sole end of economic activity opens the door to all sorts of speculations. May not the individual obtain satisfaction from the mere holding of wealth as such, or from its acquisition? Why should the *liquidity* of assets be singled out as the one feature of wealth-holding that gives subjective satisfaction? Though very little that is definite or of operational significance can be said about these matters, they may nevertheless be of some analytical importance. For if it is true that individuals obtain subjective satisfaction from the holding and the acquisition of wealth, then this could lend some support for the Keynesian emphasis on the substitution between money and other assets, as against the substitution between money and commodities.

However, my main worry about these chapters is that they impair the unity and sweep of the book. For one thing, they are of a different order of difficulty from the rest of the text. The reader who comes to them feels like the novice mountaineer who has been congratulating himself on his progress up the mountain, only to find himself faced with a pitch of real severity. And it is not at all certain, nor would the author claim, that this is the only possible route to the summit. The student who on a first reading finds difficulty with the in-

tricate analysis of these chapters would be well-advised to pass on at once to Chapter 8 and come back to them later.

The book as a whole gains stature on renewed acquaintance. The main themes, that the real-balance effect is essential for the integration of monetary and value theory, that the dichotemization of the economic system into a market for goods in which relative prices are determined and a market for money in which absolute prices are determined is invalid, that Say's identity is appropriate for a barter economy but not for a monetary economy—all these by now familiar propositions and others have, in the reviewer's opinion, stood up well to the test of time and of the many assaults that have been made on them. But perhaps the strongest impression left by the book is of the author's integrity as a controversialist. Although a considerable part of the book is devoted to a critical analysis of the writings of others, a "mere debating point" is nowhere to be found in these pages. When referring in the Preface to the criticisms of the earlier edition that he has not been able to accept, Professor Patinkin writes: "My primary objective in such cases has been the necessary clarification of substantive issues, and not polemics. I cannot, however, pretend that I have not on occasion strayed across the indefinite line that separates these two." Let him, above all, be reassured on this score.

CHARLES KENNEDY

University of the West Indies

Traité d'économie bancaire. Vol. I, *Les institutions de banque.* By JACQUES BRANGER. Paris: Presses Universitaires de France, 1965. Pp. xii, 400. F 48.

Despite its ambitious title this is an elementary—if comprehensive—textbook on French financial institutions, written by a practitioner. The author, the president of a semipublic financial establishment, is publishing the text of a course he teaches at a school of engineering. A second volume will cover credit operations.

An improbable first chapter on the genesis of the credit economy and "the banker through the ages," featuring a parody of Kipling's poem, *If* (" . . . you'll be a Banker, my son!"), and a few oversimplified pages on the role of credit in the economy introduce the first part of the book, a "General History of Banking and Credit" from prehistory to the creation of the Bank of France. These 48 pages could advantageously have been dispensed with, inasmuch as the succeeding sections on the various credit establishments include lengthy paragraphs on their particular histories. Jacques Branger is obviously not a specialist and can only present a series of second-hand vignettes on Babylon, the Medicis, or the Bank of Amsterdam.

The second part, about two-thirds of the book, is a description of the major financial institutions in France. It also includes a chapter on the regulation of banks and financial intermediaries and another on the French franc area. After these two chapters and one on the Bank of France, Branger covers successively commercial banks and the private financial organizations, the semipublic intermediaries, and the public financial services. This second part is a handy catalogue, generally exhaustive (there is even a paragraph on C.O.D.

mail, although no mention is made of the GICEX consortium in export credit institutions), and the author can hardly be blamed for devoting more space to his own outfit than to any other, except the Bank of France. The exposition suffers from its separate treatment of institutions (this volume) and operations (volume two). The reader is not spared any detail on the board composition of the regional branches of the Bank of France, yet he has to wait for the second volume to be published to learn about the limits to rediscounting operations and the monetization of medium-term credit. The arrangement of the chapters could also have been improved upon; the chapter on the franc area, for instance, follows immediately that on the Bank of France while the role of the Treasury is discussed some 300 pages later.

The last part of the book devotes only six pages to a quantitative survey of the financial sector and a simplified flow of funds statement for the economy. It is unfortunate that this section is so limited. For one thing, the book badly needs figures on the relative importance of the various sectors and establishments, as this type of information cannot easily be ascertained from the text. It takes some hard research, for example, to find out that postal checking accounts represent more than 20 per cent of total demand deposits, and postal savings accounts almost 30 per cent of all time deposits. This section could have emphasized the major characteristics of French financial institutions, the importance of the public sector, the strong liquidity preference of the general public, the underdevelopment of the financial market, and the resulting use of short-term resources collected by the financial intermediaries for medium- and long-term uses. Branger could also have made use of the sources of information available through France's pioneering efforts in the integration of flow of funds statements with national accounts.

After a lame chapter on international development finance and another on the creation of new types of financial institutions in France (such as regional development corporations and mutual funds), the book ends with an odd assortment of quotations from various personalities, including the present heir to the French throne, on the future of banking, the inflationary consequences of French planning, and the need to stimulate saving.

In spite of its shortcomings and Branger's annoying remarks (sample: he suggests that the *Crédit Municipal,* because of its similar problems, is particularly competent to appraise the financial difficulties of its customers [p. 273]; or, "Should we recall that the postal checking accounts have been nicknamed 'Treasure of the Treasury'?" [p. 310]), the book will perform a valuable reference service for the reader interested in the institutional organization of French credit establishments. It is also to be hoped that textbook writers in this country will find there profitable suggestions for the organization of their subject matter. Isn't it time that students of money and banking acquaint themselves with other financial institutions than commercial banks and be offered a complete picture of the financing of an economy?

PATRICK B. DE FONTENAY
International Bank for Reconstruction and Development

Public Finance; Fiscal Policy

Essays in Fiscal Federalism. Edited by RICHARD A. MUSGRAVE. Washington, D.C.: The Brookings Institution, 1965. Pp. xvi, 301. $6.00.

The federal-state-local layering of governments in the United States and the relationships among the many semiautonomous state governments have long posed questions of substantial moment and perplexity to fiscal economists. Attempts to come to grips with these issues have resulted in a reasonably large body of literature, varying in degree of theoretical, empirical, and institutionally descriptive emphasis. Professor Musgrave's own contributions to what might be called federal and multiunit fiscalism utilize these three approaches, sometimes in amalgam and sometimes in pure form, and of course his studies are justifiably noteworthy. The present volume of five essays is yet another dimension of Musgrave's intellectual fecundity, insight, and enterprise, each of the pieces being based upon a doctoral dissertation written under his tutelage. That these five dissertations were filed within the short span of three years at Johns Hopkins and Princeton, whose outputs of economics Ph.D.'s are on a relatively small scale, is vivid testimony to Musgrave's effectiveness as a teacher and nurturer of scholarly activity. The volume, moreover, must be reassuring to the National Committee on Government Finance whose "seed money" has so conspicuously and handsomely paid off.

It is not intended that this merited praise of Musgrave should spread a shadow over the five young economists whose work the volume presents. Quite the contrary. Each of the essays displays a cast of research design, a faculty with empirical data, an inventiveness in hypothesis testing and a caution and refinement in interpretation of results that are sufficiently distinctive to allay the fear occasionally arising with dissertations that the imprint of the mentor is overriding. On this volume's demonstration the Musgravian imprint is delicate and subtle, as it should be. The essays offer a fortunate blend of the theoretical, institutional, and empirical, to be sure; but without doubt the work reported in these essays is truly independent and of a caliber infrequently found in the research of more mature economists. These essays, moreover, must be read if one is to appreciate the refreshing qualities of the research effort, and no statement of short compass could convey these qualities with a semblance of justice. It is possible to relate a few of the more tangible and salient results of the essays. This will indicate the primary focus of the individual studies; the logical inference that the volume contains other interesting and related points is quite proper.

Two of the essays are concerned predominantly with stabilization policy in a federal and multiunit political system. Stanley Engerman's study reveals a high conformity of postwar business cycles in the several states with those at the national level. But while the turning points are largely coincident, the amplitudes of the swings vary appreciably among the states. This suggests that stabilization policy should, in some instances, be regionally pin-pointed and, through a rudimentary application of input-output studies, Engerman shows that this would be an effective strategy. He contends, moreover, that regional

stabilization policy must be the responsibility of the federal government since the causes of regional differentials in, say, unemployment rates are national in character. These conclusions are based on an analysis of annual data; apparently seasonal adjustment problems precluded the use of quarterly series. Within this constraint, however, Engerman's arguments are well supported. Our stabilization experts can ill afford to overlook them, particularly when national employment is full or when regional pockets of heavy unemployment are known to exist.

The other essay on stabilization policy is by Robert Rafuse. He tests for the postwar period the validity of the so-called "perversity hypothesis"—that the behavior of state and local tax receipts and expenditures exaggerates rather than damps business cycles. The most prominent feature of postwar state and local outlays is their rapid and virtually continuous rise. This largely accounts for the destabilizing nature of receipts during downswings and of expenditures during upswings. When the effects of growth are eliminated, expenditures become stabilizing during both expansions and contractions. Receipts, while not yet countercyclically stabilizing, have tended to become less destabilizing with the passage of each cycle, reflecting the increasing income elasticity of tax yields as the relative shares of the various taxes in the aggregate of state and local receipts change through time. The *net* cyclical impact of these forces is that state and local fiscal behavior is highly countercontractionary but also proexpansionary. This proexpansionary aspect need not necessarily weigh against state and local governments since it is not wrong to abet upswings per se. Moreover, as Rafuse demonstrates, the net proexpansion fiscal impact tapers off in the later stages of the upswing, reinforcing the possibility that the total stabilization performance of state and local fiscal behavior is on the whole beneficent. These comments do not reflect the nuances of Rafuse's analysis; they indicate, however, that the "perversity hypothesis" is a gross oversimplification.

An estimate of the over-all incidence, by family income class, of federal, state, and local expenditures and taxes is the major consequence of Irwin Gillespie's study. Since the incidence of tax receipts has already received much attention, he rightly concentrates his efforts on measuring the impact of the various types of expenditures. His results unavoidably depend upon his assumptions concerning the allocation of particular expenditures; and while one may quarrel with some of these there is little prior work to rely on. Given his assumptions, Gillespie's method derives for 1960 the gain or loss by income class, as a percentage of income, owing to federal taxes, expenditures, and the net budget pattern, on the one hand, and owing to state and local taxes, outlays, and the net pattern, on the other, and finally combines these into an all-government impact. He finds, as a capstone measure, that the net pattern stemming from state and local government budgets is beneficial to income classes below $4,000-$5,000 i.e., pro-poor, and relatively neutral for all higher income brackets. The net pattern at the federal level, while somewhat less propoor, becomes anti-rich at incomes above $10,000, with neutrality maintained in the intermediate range. The all-government net budget pattern exhibits a

substantial pro-poor impact for the lowest income class, falling rapidly to neutrality at the $4,000-$5,000 income bracket, sustaining neutrality through the $7,500-$10,000 bracket, and becoming anti-rich at family incomes in excess of $10,000. In short and too simply, families with 1960 incomes greater than $10,000 were subsidizing, through federal and state and local fiscal processes, families whose 1960 incomes were less than $4,000. To most economists, these will be gratifying findings. Perhaps, had it been possible for Gillespie to utilize a breakdown of income classes beyond the $10,000 level, less comforting results would have been obtained.

One of the recent issues of tax equity—the treatment of state and local taxes for purposes of computing the federal individual income tax—is explored by Benjamin Bridges. Is it desirable, he inquires, to maintain the current practice of full deductibility of virtually all of state and local nonbusiness taxes, or is some other form of allowance, such as a credit against federal taxes, or even of complete "nonallowance" preferable? On equity grounds, Bridges recommends elimination of the deductibility provision, but suggests that as an aid to state governments it may be expedient to permit state personal income taxes to be credited against federal tax payments and that all other nonbusiness taxes should be nondeductible. In arriving at these proposals, Bridges examines on the basis of 1958 figures the prospective change in the federal tax take, the impact on state and local tax revenues, the effect on tax progression, the consequence for interstate income tax differentials, and other outcomes of seven hypothetical deduction and credit plans combined with several reaction patterns from state governments. Bridges' exploitation of the data is imaginative and painstaking. Aside from his general recommendations, one of the more provocative demonstrations is that with the present deductibility provisions interstate income tax differentials are quite small compared to the net income of the taxpayer; tax differentials are not apt to be a significant determinant of place of residency or employment.

The final essay is William Birdsall's attempt to specify the determinants of demand for public goods. He recognizes that he has probably failed to establish the factors underlying demand, as distinct from preferences: rational calculation of future benefits and costs of public goods is extremely difficult, and evidence suggests that these calculations are frequently not made. In the absence of this, no technique can capture the elements of demand. Yet, Birdsall seeks the determinants of preferences for public goods in a novel way. Voting results on 26 financial referenda in 55 New York communities are regressed against a sizable number of salient variables. Though it is not Birdsall's fault, the results are disappointing. With few exceptions, the a priori relationships between the various voting results and their presumed explanatory variables are not supported. This was true whether the explanatory variable in question was thought to be a "general" determinant of voting behavior or a "specific" determinant of the vote on a particular type of referendum. Nevertheless, a study of this sort needed to be done; it is to Birdsall's credit that he has pursued all potentially constructive possibilities in this pioneering effort.

The foregoing remarks suggest that the volume is extremely provocative. Many of the conclusions are real eye-openers; the data are masterfully ex-

ploited; the results are thoughtfully analyzed. The volume—and each of its essays—is a significant contribution to stabilization policy and fiscal economics.

M. O. CLEMENT

Dartmouth College

A Capital Budget Statement for the U.S. Government. By MAYNARD S. COMIEZ. Washington: The Brookings Institution, 1966. Pp. xv, 143. $6.00; paper, $2.50.

Federal finances include many and large transactions which in one respect or another extend well beyond a single year, e.g., loans, repayments, and outlays for construction. The budget, however, lumps such "capital" items with the "here today and gone tomorrow" outlays for current operations. True, budget presentation has been elaborated to include special accounts for some capital items. Yet the budget figures which seem to bear most directly upon decision making, and certainly those getting public attention, mix items for the shortest of the short run with figures for transactions which will not be fully liquidated before our grandchildren run the country.

Federal practice differs from that of business and many state and local governments. Must not federal procedures be defective? Failure to allow for (large) differences in the time element of economic actions must lead to avoidable errors. Numerous economists, myself included, have asserted that federal budgeting for capital transactions has weaknesses which could be reduced. Perhaps a separate capital budget would help. Lack of evidence about magnitudes, however, has hampered meaningful debate.

Now we have magnitudes for the years 1955-63, thanks to Dr. M. S. Comiez of the U.S. Bureau of the Budget. His study, one of the Brookings *Studies of Government Finance*, is in no sense "official," but it does not conclude with advocacy of sweeping change of present practice.

The book opens with a brief discussion of concepts, including reasons for doubting that "durability" and "tangibility" of results are a necessary test of "investment" by government. A short historical background closes the introduction. The 16-page chapter, "An Appraisal of Capital Budgeting," could stand by itself as an excellent summary of the theory and issues. The author rejects the argument that the federal government should have a capital budget just because businesses do; the fundamental objectives of business and government differ profoundly; some of the ends which we seek through government cannot be related to anything like the profit-maximizing which business accounting seeks to serve; the federal government's ability to borrow does not depend upon its balance sheet (assets) nor upon earning capacity in a sense comparable to that of a business firm. To apply "a concept of financial soundness for the private business world" (p. 19), he concludes, would be a backward step in getting a better comprehension of the respects in which the federal government and business are different types of agencies for achieving different types of ends.

Would the acquisition of long-lived assets be wiser if capital items were segregated from current expenditures? The author finds little reason to expect

improvement. Both accrual accounting and the explicit calculation and statement of depreciation may help in planning maintenance expenditure and replacement. Yet as regards the cost information needed for efficient management and control, "A cost-based budget system of accounting for individual projects and activities satisfies the need for such information without separate treatment and financing of capital outlays in the national budget" (p. 21). Would a capital budget help produce better public understanding of the difference between expenditures for current operations and capital outlays? The author concludes that a generally satisfactory alternative can be found in the expansion of special exhibits.

Would separate capital budgeting of capital facilitate better planning of capital outlays? If construction projects were associated with loan financing, would we not decide more wisely than today when the cost is a charge on current taxes? After examining pros and cons, the author says "No." The year-to-year demand for taxes would change very little. To relate capital spending to loan financing with either an explicit charge for depreciation or some provision for self-liquidation might help improve the balance between present and future generations—but not much. Conceivably, but not certainly, the use of loan financing rather than taxation might help to decide more wisely which investment projects to undertake.

Countercyclical fiscal policy would become much more difficult if loan financing and capital expenditure were associated closely. The amount of borrowing or debt repayment desirable for countercyclical policy has no necessary relation to the needs for new federal facilities or to depreciation on those in existence. Although loan financing of capital projects might encourage the substitution of user fees for taxes and in some cases bring better balance between present and future generations, the distribution of costs between users and nonusers might become less desirable. Finally, resource misallocation could be encouraged—capital projects of government might get more relative to either current account or private investment than justified; investments in "human capital" might be slighted if outlays on health and education were expensed while bricks and mortar were capitalized; items of current expense might be shifted to capital account to disguise the true total of operating costs.

The author concludes that a formal capital budget is not needed for the classification essential for planning government spending, for cost analysis, and for measuring federal finances as part of the national accounts. Efforts to improve present methods should continue, e.g., in allowing for depreciation and probable loan losses, but within the present type of unitary budget.

The analysis gives little attention to capital transactions of a more exclusively financial nature. The author does not, for example, discuss the significance of that current "growth phenomenon," the sale of financial assets (loans) to finance current spending, with the budget totals reflecting only the net difference. Nor does he deal with the problems of measuring federal assets for inclusion in estimates of national wealth.

Chapter 3 summarizes the empirical study. Five chapters then deal with major topics, including the definition of capital expenditures and receipts, cal-

culation of depreciation, treatment of various types of defense spending, and the development of a capital budget on a consolidated cash basis. Seven appendixes with 31 tables present greater detail on statistical sources and methods, and on findings.

"Civil capital" includes loans and investments, public works, changes in major commodity inventories, and major equipment and other physical assets, all limited to the federally owned. The receipts which are treated as offsets are loan repayments and proceeds from sales of property. The net excess of civil capital expenditures over offsetting receipts ranged from $1.4 (1957) to $7.0 (1959) billion.

The computation of depreciation and of loss on loans and inventories rests upon a variety of assumptions; but they appear reasonable. The 1955 figure was $1.2, that for 1963 $1.5, billion. In five out of nine years losses on commodity inventories were over 40 per cent of the total. If depreciation is limited to assets acquired after 1955, the totals drop to $700 million in 1963. National defense capital expenditures, including both public works and major military equipment (aircraft and ships), ranged from $8.0 (1961) to $10.3 (1958) billion.

Five models employ the administrative budget concept, two the consolidated cash budget. One of each group seems to the author clearly most appropriate. The first (Case II) adds net civil capital, as noted above, to the administrative budget and then deducts depreciation and losses on loans. How do the results compare with the official figures? Deficits are smaller in the six deficit years and surpluses are larger in two of the three surplus years; in no case is a deficit converted into a surplus. But some differences justify the author's characterizations as "significant"—the 1959 deficit of $12.4 falls to $6.7 billion. For the nine years the cumulative deficit drops from $31.5 to $12.2 billion. The two models which include certain national defense outlays as capital spending show cumulative surpluses of $20.8 and $26.4 billion, rather different from the $31.5 deficit in the official figures.

The second of the preferred models (Case VII) is essentially Case II based on the consolidated cash budget to take account of trust fund transactions and government-sponsored enterprises. Federal outlays for the interstate highway program are not included, however, because the highways are not federally owned. The taxes earmarked for highways are treated as capital receipts on the same basis as receipts from loan repayments. Surpluses become larger, deficits smaller, but in no year does one become the other. The cumulative deficit as reported officially, $22.1 billion, drops to $2.0 billion.

The author did not set himself the task of trying to judge how much, and in what ways, decisions might have been altered if capital budgeting had been used. As one looks backward, the difference in deficit or surplus in most years does not seem large in relation to other magnitudes. But who can be sure about the effect on decisions? Depreciation appears much smaller in total than casual impression might have led one to expect. If the federal government had a balance sheet, the net position would have appeared about $19 billion better in 1963 if capital budgeting (Case II) had been used over the nine years.

Perhaps among the problems of federal finance those of capital accounting

are of much lower order of priority than some of us had thought. This book leaves such an impression. Comiez has not attempted to cover all the issues. He has, however, provided estimates which will help in sharpening our analysis. Whether pleased or dissatisfied with present practices, all of us can now direct attention more fruitfully because measurement rests on a firmer foundation.

C. Lowell Harriss

Columbia University

Program Budgeting—Program Analysis and the Federal Budget. Edited by David Novick. Cambridge: Harvard University Press, 1965. Pp. xvi, 382. $6.50.

Very few volumes on public policy have been better timed than this one. On August 25, 1965, President Johnson announced at a news conference the introduction of "a very new and very revolutionary system of planning and programming and budgeting throughout the vast Federal Government." Fortuitously, David Novick and his associates had in manuscript a book on program budgeting, the product of a RAND-sponsored project that had been initiated about two years previously. Whereupon an abridged version of the manuscript was offered to the Government Printing Office and published in the early fall. The present volume, which contains three additional chapters, was published in December.

On October 12 the Bureau of the Budget, in implementation of the President's announcement, issued a bulletin (66-3) establishing the planning-programming-budget system (PPBS). In accordance with the October bulletin, 23 major federal departments and agencies are to prepare their 1968 budgets in program categories. An additional 17 agencies are "encouraged" to adopt PPBS.

The conceptual basis for program budgeting dates back to the Taft Commission on Economy and Efficiency in 1912. Applications started with the New York Bureau of Municipal Research in its budget for the borough of Richmond in the years 1913-15. In the federal government TVA and the Department of Agriculture developed a program budget format in the 1930's, as did the Navy Department in 1946. The Hoover Commission recommendations of 1949 and 1955, which urged the extension of program and performance concepts to other departments and agencies, were thus built on a record of existing but modest federal effort.

The immediate historical antecedents are, of course, RAND, Charles J. Hitch, and Secretary Robert McNamara. Beginning in 1961, the program budget, with accompanying techniques for military planning, became a major device to revolutionize the Department of Defense, integrate the services, centralize decision authority in the hands of the Secretary and his immediate staff, and impress the Congress. It must follow that what is good for Defense is good for the civilian agencies.

And it may all be true; the present volume is persuasive. Certainly no economist can object to the introduction of additional economic content into the

budgetary process. The task is to assure that narrow efficiency values do not dominate all other considerations.

As the authors point out, the traditional line-time budget classifies government expenditures (and obligations) solely in terms of inputs—the things that are bought. A program budget classifies in accordance with outputs and the resources necessary to yield a specified output. Resource costs are carefully measured to facilitate incremental analysis. The time dimension is extended beyond the immediate fiscal year, to permit a projection of the future consequences of present decisions. A program structure accommodates cost-effectiveness studies that may be used to evaluate alternative means of accomplishing objectives.

This sounds rather simple, and, in fact, is reasonably straightforward in some areas. The chapters by Novick on the Department of Defense and by Milton A. Margolis and Stephen M. Barro on the space program demonstrate this. But for civilian programs it is most difficult budgeting.

The chapters by John R. Meyer on transportation, Werner Z. Hirsch on education and natural resources, and Marvin Frankel on health are generally realistic in pointing to the problems. The central difficulty lies in the multipurpose nature of government activities. Transportation expenditures serve both national defense and civilian needs. The armed forces have large outlays for education and training. Expenditures for the abatement of air and water pollution serve both natural resources and health. Programs for medical research could be either education or health or both.

The authors of both the general chapters and the chapters on applications consistently point out that there is no single best classification but suggest that most of the difficulties can be resolved by a "translation grid" that will link existing classifications with new ones. It is not possible, as most of the authors recognize, to undertake an administrative reorganization in the interests of program budgeting. If a purely functional approach were simple, we would long ago have had federal departments of transportation, education, and water resources. In the absence of administrative reorganization a great many programs will cut across agency and departmental lines.

It follows that program budgeting becomes a technique, not for management at the operating level, but for the centralization of administrative authority. The technique is not, as Melvin Anshen would have us believe (pp. 15, 370), a "neutral instrument" with no "politics." It will upset existing budgetary decision patterns by transferring authority from bureaus to departments. It will strengthen the Executive Office of the President in dealing with both departments and agencies and in negotiating with the Congress. It will determine, for example, that school lunch programs should be viewed as education programs and not as health programs or as agricultural surplus disposal programs. Thus the education budget will appear to be larger or smaller in accordance with the classification that is adopted. Classification decisions of this sort, as Hirsch points out (p. 198), might amount to as much as $1.0 billion in an education budget of $3.6 billion. These are political decisions in any meaningful sense of the term.

Program budgeting will certainly be helpful in making decisions about alternative means of attaining specified and agreed-upon objectives. It will be of very little use in illuminating the values that must underlie choices among programs, as most of the authors agree. The best measurements of costs and outcomes will not reveal whether it is better to spend on highways than on health, or on education for suburban children rather than on education for children in slum areas. Program budgeting will not carry us very far toward the Pigovian ideal of distributing expenditures toward battleships and Poor Relief " . . . in such wise that the last shilling devoted to each of them yields the same return of satisfaction" (*A Study in Public Finance*, London 1951, p. 31).

As Arthur Smithies says, in his realistic chapter in this volume, "The idea of economic measurement has great attractiveness because of the analytic possibilities that it offers, especially to economists. But it is a perversion of human values to push it into areas in which it does not belong" (p. 51).

JESSE BURKHEAD

Syracuse University

Federal Budget Policy. By DAVID J. OTT and ATTIAT F. OTT. Washington, D.C.: The Brookings Institution, 1965. Pp. xiv, 150. $5.00; paper, $1.95.

When President Kennedy was having difficulty in 1963 arousing public enthusiasm for tax reduction to stimulate business, he is reported to have asked, in effect, "Where are all the people who have taken 'Eco 1' since the war?" Economists may not have done well in their educational efforts—or perhaps the job far exceeds our resources. In any case, public "discussion" of recent years demonstrates the need for more professional effort in explaining the economic aspects of public policy issues. This smallish book, one of the Brookings *Studies of Government Finance*, attempts, with considerable success, to present both facts and argument needed for nonprofessional understanding.

"Primarily educational," not a report on empirical research or new theoretical inquiry, this book ranges far beyond "policy" in the ordinary sense. College teachers of economics will find it useful even though much duplicates what appears in many textbooks. Students of government, sociology, and social studies—the "man of affairs" in business, government, or the professions —should also benefit, but not without some difficulty.

A concise summary of budget concepts—administrative, cash, and national income—precedes a 23-page description of the budget process from executive preparation through Congressional authorization to post audit. Both the factual account and the authors' evaluations suggest direct contact with the executive branch, giving a flavor of "telling from the inside." Ten groups of postwar improvements are identified, all in the executive branch. Congress gets credit for a few attempts to improve the handling of its part of the job, but for almost no accomplishment.

Tables, charts, and brief text summarize the record of federal taxes and spending from various dates to 1964. Actual receipts and expenditures often differ considerably from the estimates; the authors account (apologize?) for

disparities without pointing out the significance for public policy of our inability to count upon forecasts of federal finances.

Chapter 5 goes further than the "typical" elementary presentation of the (aggregative) effects of federal expenditures and taxes on employment and prices. After showing why the price level tends to rise before all but frictional unemployment disappears, the authors conclude that society may be able to get the desired combination of employment and price-level objectives only by "a weakening of union powers or a decrease in business monopoly power, or by stronger or weaker policies on the part of government to affect prices and wages" (p. 59). Discussion of the job-prices dilemma suffers from one large but unnecessary omission. Why not make clear that the man-hours of employment which any given aggregate dollar demand—say $700 billion—will buy depend upon the average wage rate per hour, more employment if the average cost of the employer is $3.50 than if it is $4? This relation bears upon the longer-run employment effects of increases in wage rates and the possible need for fiscal action.

The treatment of economic processes and relationships takes account of such variables as different lags and liquidity effects—but not monetary action. A tone of realism, the modesty of the claims about what can be counted upon, might well have received more cross-referencing later. The attempt to develop "refinements," I fear, is not entirely successful. For example, in explaining why the deficit or surplus is an inadequate measure of fiscal action, the authors (a) distinguish between intentions and results and (b) point out that the results which are realized combine those of automatic stabilization with those of discretionary policy. Many a reader who does not already understand will need more explanation (or none?).

The presentation of the economic aspects of alternative budget policies combines points long familiar to economists with some which represent more recent analysis. "There's more than meets the eye" in condensed discussions of the inadequacy, or worse, of annual balance as a guide, of the CED automatic stabilizing plan, and of formula flexibility. The first choice of the authors is a mixture of policies. For the longer run they prescribe discretionary policy which puts heavy reliance upon tax rate adjustments (probably at three- to five-year intervals); expenditures would be determined on the basis of the merits of each program. To the extent that fiscal action is to be used for moderating short-run fluctuations, reliance would be placed on automatic stabilizers. In a few pages the authors bring to the reader major fruits of masses of professional study.

Those of us with some experience in trying to explain "modern" fiscal policy to noneconomists know that there is no escaping queries and worries about the national debt. The authors explain the concept of public debt and the error in figures of federal obligations reaching $1 trillion or so. Less successful, however, are the sections dealing with burden of debt; a digest of recent journal debate on debt burden calls for considerable background. How many readers in the intended audience will comprehend what the authors say about points which professional economists may not yet have stopped debating?

The final chapter, "Determining the Need for Federal Spending," deals

with considerations other than those of employment, price levels, and growth. When many points of diverse nature appear—while others which one believes should appear are missing—a reviewer cannot comment in detail. The authors allot three pages to evaluating the forces which influence the division of government spending between federal and state-local. Three topics—spending on public goods, spending to affect income distribution, and spending on semipublic goods—each receive two pages. A section, "Some Common Fallacies in Judging Federal Spending," makes some good points well. The chapter as a whole, however, suffers from oversimplification or, rather, overambition. Much of what appears will be helpful for the intended audience. The net result, however, is disappointing; the subject is too vast for a few pages.

Four short appendixes include nine tables of statistics, two discussions of derivation of concepts, and an excellent bibliography.

The book's weaknesses result primarily from an attempt to do more in terms of topics covered and depth of analysis than proved do-able. But having just criticized overambition, I reveal the hounding complexity of the problem by adding that the total result suffers from the self-imposed exclusion of monetary policy. The merits are more impressive. They result from a combination of professional competence and willingness to work to "let the people know." The total effectiveness is enhanced by another combination—an evident determination to be judicious where controversy is involved, while taking positions and supporting them.

This is a book of which one can say, "The wider the readership, the better the prospects of American society."

C. Lowell Harriss

Columbia University

Financing State and Local Governments. By James A. Maxwell. Washington: The Brookings Institution, 1965. Pp. xvii, 276. $5.00.

At a moment in fiscal history when state and local spending on goods and services gives indication of outpacing federal spending, *Financing State and Local Governments,* by James A. Maxwell, is a well-timed and exceedingly thorough analysis of the facts, problems, and policy implications of financing lower levels of government in the United States in the 1960's. The volume is the tenth publication in the Brookings Institution series of *Studies of Government Finance.*

Maxwell places his subject in historical perspective. In doing so, he illustrates the significant diversity in state systems of taxation; at the same time his work illustrates the historical status quo which has existed in local public finance as manifested by the dominance of the highly inequitable, administratively cumbersome, and fiscally ineffective property tax.

His subject matter is organized sequentially along the following lines: The stage for his major task is set by an historical chronology of intergovernmental fiscal relationships under the federal system. Next he reviews the pattern of expenditures, relative revenue generating efforts, and patterns of tax preference in state and local units of government. The role of intergovernmental transfers receives a thorough treatment. Two chapters are devoted to a factual

review and comparative analysis of state individual income, sales, and other taxes; as might be expected, most of the discussion of local taxation is concerned with the property tax and the manner in which it might be rehabilitated; other subject areas are the state and local debt picture, the earmarked-revenue concept, capital budgets, and future trends in state and local finance in the United States. A wealth of factual data is incorporated into a series of appendix tables. These are invaluable reference sources, particularly the tables which present comparative data by state on expenditure and revenue patterns.

This reviewer is in full accord with Maxwell's analysis of and recommendations on the state-local system of finance, with one major exception. Maxwell and I hold diametrically opposed positions concerning the equity, the administrative feasibility, the fiscal efficacy, and the compatibility of the local property tax with the public finance requirements of an urban-industrial society.

Maxwell asks (p. 129), "should a renewed effort at reform be made? Is the property tax worth refurbishment?" He replies in the affirmative primarily in the belief that "local governments should have a major source of revenue of their own." Here we part company. My position is that the property tax should be gradually phased out of the modern local revenue system. The tax is simply incompatible with the fiscal needs of an urban-industrial society.

In appraising the economic and equity effects of the property tax, Maxwell concludes (p. 133) that "the property tax cannot be awarded a high position." Yet he manifests an element of fiscal fatalism when he observes (p. 133), "But no alternative *local* taxes are in sight."

Most students of public finance believe that the property tax is highly regressive when related to the income position of the taxpayer. Maxwell, on the other hand, refers (pp. 131-32) to Musgrave's 1954 data (p. 398) to Margaret G. Reid's *Housing and Income* (1962), and (pp. 298-308) to the Survey Research Center, University of Michigan, *Income and Welfare in the United States* (1962), and concludes that the "regressivity belief" is of questionable validity. He suggests, further, that improved assessment procedures since 1954 have moved Musgrave's figures on the pattern of property tax regressivity toward proportionality (P. 131).

My own empirical studies of the pattern of distribution of property tax burdens in California indicate a high degree of regressivity. For example, a sample of 1,300 completed questionnaires of 5,000 households in a California community of 20,000 persons revealed that the family units with $2,000 annual income had annual property tax burdens ranging from 10.0 to 45.0 per cent of household income. The median burdens in the $2,000 and $10,000 income groups were 15.0 and 5.0 per cent, respectively. Musgrave's 1954 data suggest effective rates of 4.2 and 3.2 per cent for these income groups in the United States. Clearly, the 1954 data do not provide the factual framework within which to appraise the equity of the local property tax in the decade of the 1960's. The conclusion that the local property tax trends toward proportionality merits large scale empirical analysis.

One further aspect of Maxwell's analysis of property taxation should be mentioned. In discussing the property tax base he argues (pp. 133-34) that the inclusion of both intangible "representative" property, i.e., stocks, bonds,

mortgages, bank deposits, etc., and the real property on which the representative property rests, is clearly duplicative. Here we have the familiar "double taxation" argument. Maxwell's analysis is in keeping with the conventional wisdom, but it abstracts from such realities as the separation of ownership and control and the relatively independent legal, economic, and social status of the corporate entity, per se, in modern society.

Let me hasten to add that the volume is an exceedingly valuable contribution to the field of public finance. I recommend that the book be used as a second required text in general courses in government finance. It fills a long-deficient reference need for students, scholars, and practitioners at a time when the problems of state and local finance are on the ascent.

GERHARD N. ROSTVOLD

Claremont, California

The Public Economy of Urban Communities. Papers Presented at the Second Conference on Urban Public Expenditures, Held February 21-22, 1964, under the Sponsorship of the Committee on Urban Economics of Resources for the Future, Inc. Edited by JULIUS MARGOLIS. Washington: Resources for the Future, Inc.; Johns Hopkins Press, distributor, 1965. Pp. xvii, 264. $5.00.

In his introduction to this volume of papers Professor Margolis sees the thread that ties them together in the nature and evolution of the "new field of public economics." Public economics is "A full-fledged economic analysis of government as a producer of goods and services and as the sovereign setting the rules of economic and social organization." If one is inclined to define public finance in terms of the contents of the standard American text book, then Margolis is indeed justified in his call for recognition of a new "field." But the case is far less clear when viewed in the light of the work of Pigou, the scholars of continental Europe, and in this country such people as Musgrave, Samuelson, Buchanan, Harberger, Shoup, among others, whose work surely involves "public economics," even if we have always called it "public finance." The latter term may well be misleading, but even more confusion seems inherent in the effort to move away from it toward the definition of a new subdiscipline which must include most of what economists generally think of as public finance while merely adding a few analytical tools and areas of analysis to the traditional ones.

Irrespective, however, of whether one prefers public economics or public finance, at least three of the papers contained in *The Public Economy of Urban Communities* appear, at best, to have only a very remote relevance for either public economy or urban communities. Professor Rothenberg builds a formal model of political behavior out of the notions of demand (from citizens) for and supply (by legislators) of "political support." Apart from an interesting analysis of types and compositions of political coalitions, however, his paper offers little that seems to promise new insights for understanding the public sector. Granted that "long-run welfare criteria" would be handy to have, Harvey Leibenstein, despite a valiant effort, gets hung up, not unexpectedly, on the intergeneration problem. Since unborn generations clearly have no

discernible preference functions, we have in his paper some ably put questions, but hardly the empirical basis for welfare decisions that he seeks. The third in this group of contributions, "Majority Voting and Alternative Forms of Public Enterprise," by Benjamin Ward, contains nothing, beyond some introductory remarks, that is specific to *public* enterprises. It focuses, rather, on decision-making by variously constituted boards of directors through alternative voting procedures.

The other eight essays are both less ambitious and more relevant to the subject matter suggested by the book's title. Professors Buchanan and Tullock develop an interesting analysis of public and private adjustments to the existence of reciprocal external economies associated with a variety of activities that may be undertaken either privately or collectively or both privately and collectively. Their showing that public provision of a good the consumption of which involves major external economies may not, indeed is unlikely to, yield Pareto-optimal results is not surprising. Their analysis, however, constitutes an important contribution to the theory of public finance.

James Q. Wilson and Edward C. Banfield examine the hypothesis that, in elections authorizing expenditures for various local public projects, voters tend to behave in a manner suggesting the pursuit of self-interest. They find that in the larger cities low-income renters "almost invariably support all expenditure proposals," but that for higher-income groups self-interest appears to be replaced by a "public-regarding" attitude, particularly among Anglo-Saxons and Jews. Their results, based on voting records and population characteristics by precincts, seem more than sufficiently promising to warrant further research along similar lines.

Otto A. Davis sets up a model of "political influences" on public school expenditures per pupil in a six-county area of western Pennsylvania. He then proceeds to a presentation of the results of multiple regression analysis in which the statistically significant predictor variables turn out to be the proportion of the electorate owning property, per capita market value of taxable property, and the proportions of children attending public and nonpublic schools. Whether or not these variables represent political influences may be debatable, but there appears to me to be little call for Davis's conclusion "that economists attempting to understand this [allocation] process would do well to discard at least a part of their notions of demand and supply and, hence, the implicit implication that a pricing mechanism is the allocation device in the public sector." Surely some economists, if not Davis, are familiar with the efforts of those of us who have done just that, including Werner Hirsch, Jerry Miner, and myself, among others.

The paper by Professor Weisbrod is drawn from his *External Benefits of Public Education*. It presents a crude but interesting effort to test the hypothesis that benefit spillovers associated with local expenditures for education lead to reduced educational outlays and a shift in financial responsibility to higher levels of government.

Allen V. Kneese, in a succinct, well-reasoned paper, offers an excellent treatment of the economic issues in decisions regarding water quality control. He makes a convincing case for the need for a great deal more effort on the

part of economists than has been forthcoming in one of the most important public policy areas facing industrial communities everywhere.

Using the case-study approach, Nathaniel Lichfield succeeds in clarifying the issues involved in the employment of benefit-cost analysis in choosing among alternative plans for urban development or redevelopment. Problems of measurement of benefits and costs and identification of repercussions of alternative projects are well illustrated, as is the importance of detailed specification of "instrumental objectives."

Far too large a part of the paper by Benjamin Chinitz and Charles M. Tiebout is devoted to a restatement of Musgrave's three-part budget system and his classification of public goods as social-want and merit-want satisfying goods. This taxonomic scheme contributes little or nothing to an understanding of "The Role of Cost-Benefit Analysis in the Public Sector of Metropolitan Areas." It is no wonder, then, that the theoretical analysis falls down at an early stage and the authors conclude only that the application of cost-benefit analysis "is not at all easy, even at the conceptual level." And while the brief discussion of "cost-benefit analysis and the allocation of transportation" points up well the economic issues involved in metropolitan transportation problems, it tells us very little about the role of cost-benefit analysis in the solution of these problems.

Finally, Professor Strotz's "Urban Transportation Parables" are spelled out in the form of mathematical statements (not all of which I can follow, I readily confess). They lead to such conclusions as "Thus, population density should tend to fall off the further land is from the city center . . ." and ". . . a stretch of road should be taxed, subsidized, or be exactly self-financing depending on whether returns to scale of the congestion function are adverse, favorable, or constant. . . ." These conclusions hardly seem new, but there may be useful reassurance to be found in the elegance of the demonstration of their validity.

Clearly it has been impossible to do justice here to eleven papers covering an extremely wide range of topics. I find it difficult to identify a theme or unifying thread running through the book, and the title hardly seems descriptive of its contents. The specialist in public finance will find several of the contributions well worth reading, while others are best skipped, and many other readers will discover, perhaps to their surprise, that they have long been engaged in research in "The Public Economy of Urban Communities." On the basis of my own tastes I would suggest that the benefit-cost ratio will probably exceed 1 for those who confine their reading to the papers by Buchanan and Tullock, Wilson and Banfield, Kneese, Weisbrod, and Lichfield.

HARVEY E. BRAZER

University of Michigan

International Economics

International Monetary Relations—Theory, History, and Policy. By LELAND B. YEAGER. New York: Harper & Row, 1966. Pp. viii, 504. $10.50.

Professor Yeager provides us with a long and generally excellent book on

"international finance." International finance is, to some of us, empirically the most pertinent portion of "international economics." (Even in matters with a "commercial policy" context, the issue can be largely "financial," as in the question of optimum currency areas, touched on lightly by Yeager, pp. 98-99, 487.) Recent reviews of the theory of "international trade" make apparent the embarrassingly precious quality of much of that theory and the considerable extent to which it has moved in effect toward objectives other than explication of the world. Yeager not only has chosen the more substantial subarea but has devoted his exposition to elucidating the subject rather than to confounding us with ingenuity in doodling.

The volume is in two parts of equal length: "theory" and "history and policy."

After an opening chapter which provides a summary primer of trade theory and is virtually unutilized later (thus illustrating the considerable gulf between "trade" and "finance") and a second chapter which surveys selected techniques and technicalities of the foreign exchange market, the first major topic is the balance of payments. Alternative approaches to defining and measuring equilibrium are sketched; Yeager's orientation on the mechanics of this problem centers on the admittedly difficult notions of "independently motivated" and "compensatory" transactions (pp. 46-47), but he well emphasizes the status of individual economic units and their connection with the central bank in interpreting the meaning and significance of a payments deficit (pp. 51-53). The remainder (10 chapters) of Part I is devoted to the theory of balance-of-payments adjustment under pegged and fluctuating exhange rates and under direct controls, along with repercussions of adjustment on the terms of trade and the price level and the nature, importance, and possible control of speculation. The three chapters on speculation and related matters constitute one-quarter of the first part.

In Part II, there is a history of monetary standards before World War II (4 chapters); wartime experience and planning and the IMF (2 chapters); postwar evolution to convertibility and case studies of England, France, Germany, and Canada (5 chapters); dollar shortage and glut (1 chapter); and existing "liquidity" arrangements and reform proposals (3 chapters). In these hard-headed discussions, some profane—and perhaps some near-sacred—toes are firmly trod upon.

The volume is large: almost 500 oversized, double-columned pages, including seven end-of-chapter appendices comprising 28 pages of small print. There is no consolidated reading list, but the footnote-citation items would constitute an impressive (but not exhaustive) and well-selected bibliography. Often the discussion is commentary, assessment, and elaboration of the literature (always sensible and frequently subtle) more than systematic, comprehensive exposition; it is almost as much high-level critique and monograph as textbook. It probably warrants comparison with Meade's major opus more than with the lesser books—useful in their ways—by Henning, Wasserman-Hultman-Zsoldos, and Ward.

For all of the bulk, the saneness, and the erudition of the work, it is lacking in extensive, detailed presentations on a few key matters and conspicuous

items of literature, e.g., measurements of payments imbalances (there is no explicit reference to relevant works of Lederer, Høst-Madsen, or Lary and little to Gardner), the Hicks differential growth model (briefly noted, pp. 459-60), stability conditions with initial payments imbalance (p. 158, n.), the Domar analysis of international investment and balance-of-payments stages (not mentioned). Again, one may be dubious, at least from the student's point of view, that justice can be done to some expositions of internal-external equilibria (e.g., Mundell, Hemming-Corden) and of devaluation without a bit of geometry; or to the absorption approach without employing income equations; or to the mechanics of the foreign-trade multiplier without considerable algebra or "period" tables. In addition, there are, quite inevitably, a few places where one may have some doubts on judgment or emphasis, and infrequently the exposition is a little austere or not as simple as it could be (e.g., some of the algebraic derivations, while systematic, are admittedly tedious and not always the easiest presentation).

But while the discussion may occasionally slip between the two stools of "monograph" and "textbook," such difficulties are greatly overbalanced by the impressive strengths of the book. Along with its unusually wide scope and utilization of the literature, the volume abounds in solid and sometimes quite advanced theoretical analysis in Part I (e.g., an unconventional and useful introduction to monetary systems, pp. 54-59; excellent incorporation of corrective cash-balance effects into adjustment with fixed rates, pp. 54-56, 64-67, 147-48; and elaboration of simple multiplier theory to include the role of money and the export response to domestic income, pp. 76-78) and knowledgeable, lucid, and effective historical surveys and policy interpretations in Part II. And Yeager's conclusions on such matters as "destabilizing" speculation (unlikey with fluctuating rates and well-ordered internal economies; unlikely to be serious with fluctuating rates even with internal disorder; and virtually inevitable and often serious with pegged-but-adjustable rates), the Canadian experience (it does not justify the typical banker and governmental predilection for pegged rates), and the rationality and efficacy of the IMF arrangement (he finds it generally ineffectual and unpromising if not actually appalling) are gratifying to a growing number of us.

The book is written with a solemnity presumably appropriate to international finance.

WILLIAM R. ALLEN

University of California, Los Angeles

Primary Comodities in International Trade, By J. W. F. ROWE. New York: Cambridge University Press, 1965. Pp. xi, 224. $6.50; paper, $2.45.

One could be forgiven for assuming, prima facie, that Mr. Rowe had followed current fashion in producing a book of statistical tables on international trade. But in actual fact he has not done so; and for this alone we owe him our thanks. What Rowe, in effect, has endeavored to do is to produce a *textbook* on a topic which, as he correctly points out, has had little attention devoted to it recently.

Rowe's definition of what are primary commodities follows from his

definition of primary industries as "those industries which supply foodstuffs and raw materials by agriculture or mining in the form in which they are first exchanged internationally." He limits his discussion of those primary industries and their products by defining his study as being related to *transocean trade*, and not to international trade as we know it. This means, for example, that important regional trade such as intra-European trade is omitted from consideration. Nevertheless we are left with a list of some twenty principal primary commodities which can be regarded as a satisfactory one. The actual list is as follows: Crude Petroleum, Meat, Wheat and Flour, Fats and Oils, Cotton, Coffee, Copper, Wool, Sugar, Rubber, Dairy Products, Tobacco, Rice, Maize, Tea, Cocoa, Tin, Jute and Hard Fibers, Zinc, and Lead. As can be seen, all the old favorites are there!

Briefly, the book is arranged as follows. In Part 1, after clearing away the definitional problems, Rowe presents data on the importance of the 20 given commodities in recent international trade and the main exporters of those commodities. There then follows an excellent, if necessarily sketchy, account of the structure of production in the 20 primary industries. The type of information provided is necessary, I feel, for a full understanding of the position of primary commodities in international trade.

Part II completes the general picture, as it were, since it is concerned with demand and the determination of prices. Following the procedure adopted in Part I, we are given data on the principal markets for primary commodities. Before the important discussion on the pricing process we are given an elementary, and largely unnecessary, account of the "physical process of marketing" the commodities. The chapter on prices, however, makes up for this and it contains a lot of sound common sense. It discusses, mainly, the development and importance of the role played by the "merchant speculator" in establishing and maintaining world prices; as a result there is discussion of the development of future markets. Merchant speculators were given an essentially free hand in determining prices up to 1939 and from about the mid-1950's onwards. In the intervening period the war and its aftermath resulted in large-scale government purchases and sales of primary commodities; so that governments largely determined prices. The appendix to "the determination of prices" chapter attempts to compare the pricing process under the merchant speculator regime with that under government bulk trading. It is not surprising that Rowe reaches no firm conclusions. But most would sympathize with his view that political, social, and other noneconomic motives are likely to govern the determination of price under government bulk-trading schemes; so that less homage is paid to the "equilibrium price" than is likely to be the case under the private enterprise regime.

Part III is historical and largely statistical, being, in effect, a description of the trade in, and the prices of, the primary commodities from 1918 to the present day. This discussion, which at first sight seems unnecessary, is seen, on a rereading, to be extremely valuable in understanding the very important issues raised in Part IV where commodity control schemes are scrutinized.

I regard Part IV, and in particular the chapters on "Control Schemes since 1945" and on "The Mechanics of Commodity Control," as the best part of the

book. This part makes interesting and instructive reading. Furthermore, despite covering some old and familiar ground, it will give stimulation to further theoretical research, especially on the structure of control schemes.

I have an objection, however, to the material in the Appendix to the chapter—which in itself is really excellent—on "The Mechanism of Commodity Control." My objection is that, despite his statement to the contrary, Rowe has not been rigorous in his treatment of the comparison of the crop-equalization scheme (i.e. producers' scheme) and the merchant regime. When considering a bumper crop of a commodity and the best theoretical policy for producers to adopt, i.e., whether to introduce their own crop-holding scheme or take what they can get from the merchant speculator, it is possible to make more definite statements than Rowe does by the use of some simple algebra or arithmetic. I am sure the Appendix would gain from a more rigorous presentation of the ideas it now contains. To give one very simple example, if we let: x be the percentage rise in the crop in the bumper year over the "normal" year; m be the merchant's margin in per cent; Π_B be profit for producers in the bumper year; and Π_0 be their profits in a normal year; it can be shown that (letting x and m be fractions):

$$\text{if} \quad x \lesseqgtr \left[\frac{1}{1-m} - 1\right], \qquad \pi_B \lesseqgtr \pi_0.$$

So as a starting point one can say how profit will compare under the merchant regime in a bumper year with that in a normal year, and one can then develop the whole topic from here.

There can be no doubt that with one or two reservations Rowe has produced an admirable book that is well written and, contrary to his own fears about this, well organized. It will serve the needs most certainly of the undergraduate and possibly those of the general reader. Although Rowe has not written a *Principles* as such, a remark once made by Sir John Hicks is well worth recalling: "The ideal which any writer of *Principles* ought to set before himself is that of the classical poet: 'What oft was thought but ne'er so well expressed.'" Such is the case with Rowe's book: and it will take a great effort on the part of anyone to achieve success who sets himself the task of improving on this book.

J. L. Ford

University of Manchester.

The Canadian Dollar 1948-1962. By Paul Wonnacott. Toronto: University of Toronto Press, 1965. Pp. xviii, 339. $6.50.

This book consists of two substantially independent parts. Part I was published as *The Canadian Dollar 1948-1958* and was reviewed in this journal (September 1962, *52*, 874-75). The present review is restricted to Part II, which discusses the events and policies from 1958 until 1962 when the Canadian government abandoned the system of flexible foreign exchange rates.

Professor Wonnacott chose to concentrate his attention on the economic policies of this second period rather than on the functioning of the mechanism of international adjustment, which was the principal interest of Part I. This choice was made because the mechanism of adjustment continued to function smoothly, and it was the bizarre economic policies of the period that caused the collapse of the system of exchange. High interest rates and a major lengthening of the maturity of the public debt during the depression of the late 1950's were the more painfully inappropriate because the resulting inflow of capital was immediately transferred by the flexible exchange rate mechanism through a current account deficit in the balance of payments. The effect of the budgetary deficit on employment was less with a flexible than it would have been with fixed rate for analogous reasons. The exchange system itself presented real as distinct from imaginary policy problems only in 1961 when the Government interfered directly by announcing their determination that the rate should rise and by purchasing U.S. dollars. They were then met with too much success for their taste. "The least ambiguous lesson of the recent Canadian experience is that a combination of ill-advised and ill-administered policies, occurring at an unhappily inopportune time, is sufficient to create a crisis under a flexible exchange rate sytem" (page 200).

The study also includes description and commentary on other more specific policies. Policics to reduce foreign control of Canadian industry may have affected the composition and the volume of capital inflows. The complicated scheme to induce the expansion and rationalization of automobile production in Canada by bilateral tariff abolition with the United States and subsidy is admirably analyzed.

Thus, Part II of Wonnacott's book is essentially a brief interpretative history of economic policy in Canada in 1958-62. His arguments are persuasive. This reviewer, for one, shares most of his views. Nevertheless, it must be granted that a priori reasoning plays a very large role in these questions. There are very few data to confirm the analyses. To what extent did the ambivalent xenophobia perceptible in many quarters affect capital flows? Of what significance was the technology accompanying imported capital? Did capital inflows raise the share of labor in national income? These and many other questions need to be answered.

H. C. EASTMAN

University of Toronto

L'économie internationale depuis 1950. By JEAN WEILLER. Paris: Presses Universitaires de France, 1965. Pp. 250. F 18.

This is a puzzling book. It is exceedingly difficult to read. It lacks the lucidity that one is accustomed to expect from French writers. The substance is apparently deep, though evasive.

Professor Weiller belongs to the "Perroux school," the ambition of which is to bypass the "classical" theoretical tradition, by substituting for it a set of new concepts, supposedly more adequate to the real world. The author's anticlassical and institutional bent leads to constant sniping at the orthodox frame

of reference. According to the author, the classical static theories hardly help to explain international economic developments, which result, essentially, from the governments "structural" or "policy preferences."

The book is divided into four parts, dealing respectively with economic policies in Europe and international relations in the fifties, European integration and cooperation, negotiations in the 'sixties between industrialised countries, and international strategies between unequally developed countries.

Starting his analysis from the termination of the Marshall Plan period, the author discusses anew the then fashionable thesis of "structural disequilibrium," and the policies that have led to its disappearance. The nonautomatic and nonorthodox aspects of the policies are constantly being stressed. In the case of the German miracle, the author goes so far as to contend its causal relation with the 1948 monetary reform and the ensuing classical policies. He argues that the rapid and orderly economic growth ought to be imputed partly to the Allies' dirigiste policies before 1948 (!), to aid, and to a favorable industrial structure.

The second part, devoted to "cooperation" and "integration," is mainly concerned with the Common Market, of which the structural aspects and problems are again stressed, as well as the need for joint economic planning. The European Economic Community is said to have originated from a too *laissez-faire* attitude of other international organizations, mainly the OEEC. The Rome Treaty is considered as a compromise between classical free trade principles and the will to "plan." The author's preference for structural planning, of the Monnet type, at an international level remains a basic theme throughout the book.

In the third part, the significance of tariff barriers to international relations is toned down (p. 125). While structural obstacles to the Kennedy-round are mentioned (p. 132), the author maintains that lowering tariffs would not necessarily bring about the most desirable structural shifts in production and trade.

In the chapter on "the role of intergovernmental negotiations in strengthening the international monetary order," Weiller expresses his skepticism towards the restoration of international automatic balancing mechanisms. He would rather rely on the coordination of mutual assistance (p. 160).

The author is most at ease in dealing with the problems of underdeveloped countries. Here, indeed, he can lean on a good deal of recent unorthodox literature. The central problem is that of adapting international specialization to the requirements of domestic planned development. The Heckscher-Ohlin "law" is rejected and flexible discretionary protection to sustain and stimulate planned development is defended. The author pretends that a reasonable rate of gowth (5 per cent) would not be possible without selective controls of imports (p. 217).

Weiller's attitude is that of a sociologist. His purpose is to explain the behavior of governments. From an economist's viewpoint, his weakness is to show too much tolerance for irrationality, or to look for some rationality in irrational behavior. He accepts "policy preferences" as given and hardly discriminates between them. As an idealist, he purports to substitute *coopération généralisée* to *laissez passer* as a leading utopia.

This book needs an antidote for French readers, but Rueff's could be too strong.

ROGER DEHEM

Laval University
Quebec, Canada

International Finance. By RICHARD WARD. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. vi, 213. $7.95.

This country's balance-of-payments ills, together with the growing debate on the need for international monetary reform, have generated a great deal of interest in international financial relations among students of business finance and economics. However, very few up-to-date textbooks on the subject are presently available. Professor Ward's comprehensive book thus fills a void and is to be welcomed. It is a largely descriptive and elementary text, written in an easily readable, straightforward style. In the words of the author, "it concerns the international payments mechanism and its economic function." His purpose, Ward states in the preface, is "not to differentiate and mystify the subject, but rather relate it to domestic institutions." In the concluding sentence of the last chapter the reader is warned that the international monetary system described in the book will not be the system operating in the next decade —a prediction not shared by the present reviewer. But, however the system evolves, study of the book will be helpful for a better understanding of the considerations that are likely to dominate the continuing debate on international monetary reform.

The volume begins with a discussion of the measurement of the balance of payments. In preparing this chapter, the author unfortunately could not take advantage of the Bernstein Committee's report, published after completion of the book. The conceptual apparatus used here by the author may not be easily comprehended by the uninitiated student. Explaining the concept of balance-of-payments disequilibrium, he states: "if there is a surplus or a deficit, the country's autonomous outflows are not equal to inflows, and the balance of payments is in disequilibrium." Teachers may feel that this and related observations require more elaborate treatment than provided by Ward. Incidentally, he states in the following paragraph that banks sell their dollar balances to foreign governments (not mentioning central banks), and states that the United States holds its monetary reserve in the form of gold (only subsequently drawing attention to the fact that certain foreign currency holdings of our monetary authorities also serve this purpose). The chapter concludes with a very useful description of the major components of our balance of payments.

In the second chapter, on international investment, Ward defines direct investment abroad rather loosely as "the purchase of capital goods in one country by residents of another country." Monetary capital flows, he says, "become real capital flows through the import surplus which accompanies the inflow of capital." The chapter contains a good explanatory survey of the motivations of U.S. direct investment abroad, discusses portfolio investments, and provides an outline of the major international and U.S. government institutions through which international investment is channeled.

A commendable exposition of the components of short-term capital flows

follows in the third chapter, including a bird's-eye view of the Euro-dollar market. This section is particularly valuable because there are very few such brief surveys available. The present reviewer would take exception to the statement that commercial paper generally carries a maturity of nine months and that exporters sell their accepted bills in the open market through a dealer —except on rare occasions. Nor would he agree with the statement in the following chapter that direct lending is not the most important part of the banks' international role. Also questionable is the author's contention that commercial banking, through its influence on the international system, plays a fundamental role in determining the size of the balance of payments. But this chapter provides an excellent thumbnail sketch of the institutional side of international banking, information not easily come by elsewhere.

In Chapter 5 the author succeeds rather well in clarifying the mysteries of foreign exchange markets and rates. The following chapter, dealing with exchange rates and including brief descriptions of the International Monetary Fund and the Bank for International Settlements, is also very useful. The forward exchange market is treated in a separate chapter, and here again the author has done a praiseworthy job in explaining the many complexities of the subject in a manner that should have considerable appeal to students of finance.

The role of gold in the international payments mechanism is given an appropriate amount of attention, with discussion of both the London gold pool and the various reform proposals aimed at expanding the use of gold in official reserves. Ward makes some generalizations that may not appear acceptable to all teachers of international finance. Thus he suggests that transfers of currency in settlement of international transactions "occur through private capital flows unless the government intervenes in the exchange market"; and he observes that "if a currency other than the dollar is acquired in international transactions, the country acquiring the currency has the right to convert it into dollars." But whether one wants to take exception to such broad statements is largely a matter of personal preference. Again, students will find it most helpful to have in a nutshell a simple exposition of recent reform plans bearing on the role and price of gold.

Two chapters dealing with the interrelationships between the money supply and foreign transactions and between national income and the balance of payments provide a clear and to-the-point explanation of the linkage between the domestic economy and international payments and the channels through which monetary policy affects international financial relations. For the student who wants to be *au courrant* on international monetary reform, a concluding chapter on international reserves explains in straightforward language the major proposals made in recent years for improving the international monetary mechanism.

The volume contains useful selected bibliographies at the end of each chapter. Even though this is not a book for the specialist or for the analyst seeking a sophisticated discussion, students can learn a great deal from it about international finance.

FRED H. KLOPSTOCK

Federal Reserve Bank of New York

Foreign Investments in India. By MICHAEL KIDRON. New York: Oxford University Press, 1965. Pp. xvi, 368. $8.00.

There has been a great deal of discussion in the United States on the desirability of foreign private investment in India. This, however, is the first book that I know that looks into the entire subject in a historical, political, and economic sense—as distinguished from a study of individual cases of foreign private investment.

The author begins with a historical presentation of the role of foreign capital in India under the British empire, the effect of independence upon foreign investment, and the changing character of that investment following independence. He then looks at the changing Indian attitude and policies of the Indian government toward foreign private investment—and the shifts in attitudes and policy under the changing economic and political circumstances, both within India itself and among potential investing countries. This is followed by a discussion of the position of the foreign sector in India today and the reasons for private foreign investment within India and its character. He concludes with an attempt to estimate the benefits and costs of private investment in India.

What comes out most strikingly from this analysis is the bargaining character of the foreign investment process within India. Essentially the author describes a process of the Indian government bargaining with foreign equity investors—large private firms from the United States, the United Kingdom and other countries—or governments, especially from the USSR and East European countries, which are interested in constructing plants or collaborating with the Indian government or private firms. The parties in this bargaining process are the Government of India; Indian private industrial firms which may be collaborating with the large foreign firms; foreigh firms which are interested in entering the Indian market for a variety of reasons, only *one* of which may be maximization of profits in the relatively short run; and foreign governments, which may be interested in supporting investment by their nationals or constructing plants within India for a wide variety of political and economic reasons.

The Government of India's interest in encouraging foreign investment is a product of a mixture of motivations: to industrialize in accordance with the Plans; to save its own foreign exchange resources in the process of industrialization; and to gain access to modern technology and eventual freedom from foreign control of this technology. However, the government wishes to gain these without surrendering control of key sectors of the economy or of economic policy to foreign influences. There is also a wide variety of ideological influences at stake, with the government's belief in a "socialistic pattern of society," and the strong nationalist suspicion of foreign investment. Michael Kidron does an excellent analysis of the shifts in government policy and the relatively low importance of ideologic elements in the policy mixture. This is almost a definitive analysis of the pragmatic character of Indian government policy in an important area, in response to circumstances, most notably the recurrent shortages of foreign exchange. It is also clear that the Government of India does not hold a strong bargaining hand in negotiating with potential foreign investing firms, especially in areas of new technology. The government

is interested in comparatively short-term gains and desires them rapidly; the foreign firm is able to forego some of its *potential* short-run gains for longer-term benefits, although the *actual* gains in fact turn out to be high in the short term. One of the very interesting sections of the book shows that, for the large private foreign firms which have invested in India, the economic activities of their Indian subsidiary are only a small portion of their total business; but in the Indian economy these same subsidiaries play a major industrial role.

Private foreign investment has been especially important in "the newer, technologically complex, and patent-protected industries . . . such as the electrical and chemical industries. . . . However, where massive investment is required . . . foreign firms are unlikely to venture more than knowhow. Steel-making, heavy transport equipment . . . for example, have all been left largely to local capital, in these cases through state financing, with foreign firms participating as technical advisers." But while the government's bargaining position is not strong, it is the government that determines into which sectors to permit private foreign firms; which firms to encourage; how many foreign firms to encourage in an industry or a sector; and from which country the foreign collaboration should be selected if there is a choice.

Private foreign firms have done well for themselves financially in India, and if they exercise restraint in terms of the size of the return they expect, and in dealing with Indian government and business, they can continue to do well. They are interested in the longer-term prospects of a potentially very large Indian market, and to get into this market they must do so at an early stage. They are especially interested if they can get in with a relatively small net investment of their own financial capital, as distinguished from technical know-how, or as distinguished from equipment that can be transferred from other enterprises elsewhere; in that way they can make a rapid recovery of their investment. If they can establish a collaboration agreement with an Indian firm or management group, they will be assisted and protected in their relations with the Indian government, as well as against unfavorable Indian business reaction to the investment. Private Indian firms today generally welcome collaboration as a way of getting foreign exchange and technology; but may oppose it, as they have in the past, if they regard foreign investment as a competitive threat.

Finally, foreign governments have been interested in investing in Indian industry, either by encouraging private investors or themselves constructing plants, as a way of maintaining or increasing their influence within India or expanding sales of their own exports. In the industrial field the Russian and East European countries have made large investments in the public sector, and are apparently willing to invest in the private as well as the public sectors of the economy; British and German firms, with the support of their governments, have built steel plants in the public sector; the U.S. government however has assisted only marginally, if at all, in public-sector manufacturing enterprises, and U.S. private firms have in recent years not invested or collaborated with public-sector enterprises. (This is a self-imposed constraint, on largely ideological grounds, that in my opinion is a mistake. It deprives the United States of impact upon very important sectors of the economy.) The Government of India has used potential Soviet and East European investment

as a bargaining instrument in order to avoid becoming overly dependent on Western investment; where Western firms have been unwilling to invest at terms considered desirable, the government has gone to the Communist countries. (However, Kidron makes it clear that the government has in most cases given the West first option, as for the Bokaro steel plant, or for certain pharmaceutical plants.)

In his conclusions the author asks what India has received from the foreign investment. He raises serious questions whether this investment has in fact provided India with the technological independence that the government desires; and whether India has saved, and will continue to save, foreign exchange as a result of this foreign investment. In both cases he argues that India has not achieved these aims: the technology supplied has been offered on terms that will continue India's dependence on foreign sources of technology, and India has not saved foreign exchange by the investment. These conclusions should be of great interest to Indian officials, as well as to Western officials since they run counter to present policy. However, he does not indicate what the alternatives are for India, given the sources of the technology that India wants and India's relatively poor bargaining position.

I have one main criticism of the book. The author in general seems to believe that India has lost "control" of its industrial sector to foreign investors, and this belief pervades the analysis. However, at times he is embarrassed or doubtful of his own use of the term "control," which he recognizes is very difficult to define, and qualifies it. I would agree with his qualifications and much less with the belief. The Government of India makes the final decisions, and has turned down private investment when the price has been too high, as reportedly in the recent Bechtel fertilizer negotiations; it has successfully put a great deal of pressure on the foreign oil companies for lower prices, and on other foreign companies to expand. While the government has many times modified its policies in order to encourage private investment, whether foreign or domestic, it has, as the author himself points out, controlled the terms of entry and limited the entry into various major sectors, quite apart from the general economic controls after entry. I personally prefer to look at foreign investment in India in the framework of a bargaining analysis, similar to that described earlier, in which the government makes the final decision to accept or reject the investment, in which the various sides have strengths and weaknesses—rather than as one in which either side, the government or the private foreign investors, has or seeks complete control.

This is a very stimulating book. I would consider it a very important book for anyone looking at the Indian economy or at the formation of economic policy within India. I would recommend it also to a broader audience than simply economists or those interested in India. It should be read by U.S. and other Western officials encouraging private investment in India or elsewhere, and by potential private foreign investors, since it presents the best analysis I know of the environment in which they will be functioning. Finally, it should be read by Indian officials dealing with foreign investors, since it takes the first over-all objective look at their efforts, . . . and questions the results.

George Rosen

The RAND Corporation

Business Organization; Managerial Economics; Marketing; Accounting

Studies in Mathematical and Managerial Economics. Vol. III, *Management Goals and Accounting for Control.* By YUJI IJIRI. Chicago: Rand McNally & Co.; Amsterdam: North-Holland Publishing Co., 1965. Pp. xvii, 191.

This is Volume III in the series *Studies in Mathematical and Managerial Economics,* edited by Henri Theil. The series applies quantitative methods to the problems of mathematical economics and business management. This book offers analytical (quantitative) approaches to the problems of setting management goals and accounting for control, where the information supplied is based on the intended use of the information. Recognizing the necessity of laying a firm foundation the author develops the basic explanations of the mathematical tools that he desires to use, the approaches to decision-making using these mathematical tools, and then explains how an understanding of the decision process can influence the accounting techniques. The book is of relevance to the theoretical economist interested in the theory of the firm, the managerial economist developing practical approaches to decision-making, as well as the teacher of accounting who wants to study a unique approach to the problems of accounting for control.

Chapter 1 describes the motivation of the book and introduces the mathematics that are used. The mathematical prerequisites are described by the author to be elementary linear algebra and linear programming. This reviewer warns the reader that "elementary" is used in the same sense that a mathematician would use the term. Most readers will find the book to be a challenge.

In Chapter 2 the simple linear breakeven model is explained and then extended to a "piecewise linear" breakeven. Nonlinear functions are thus avoided, but at somewhat of a cost (the cost is noted by the author). The ideas and uses of a generalized inverse (every matrix has a generalized inverse) are used to obtain a solution to "goal programming" problems, a type of linear programming.

Chapter 3 considers complications in the setting of goals (for example, the setting of multiple goals and incompatible goals are reviewed). In Chapter 4 the quantitative feedback (gathering of information) is utilized as a tool for facilitating the goal attainment of the organization. A method of charting the goals of an organization and the feedback generated by the accounting processes is developed. Managerial uses of this "goal indicator chart" are discussed.

In Chapter 5 the double-entry bookkeeping system is restructured by establishing a measurement vector for each asset and equity account. Each component of the vector represents a different characteristic of the account. The balance sheet that results not only shows the relevant dollar measures but also various physical units. Chapter 6 is an application of the previous chapters. The basis of the applications is a series of plays of a business game (The Carnegie Tech Management Game). One objective is to illustrate the fact that information useful to management can be supplied by the accounting system that employs the mathematical models developed earlier in the book.

In closing the author states:

> The advent of the electronic computer, a mathematics tool, and the development of new mathematics along with numerous management applications in operations research and elsewhere open further prospects for future developments. It is our belief that serious research will show that accounting is neither in conflict with these developments nor isolated from them.

Management Goals and Accounting for Control is an original contribution to the literature of economics and accounting. The process of evaluating and digesting this book will continue for many years. Some books can be readily placed on a value scale, but this reviewer shrinks from passing final judgment at this time on this volume. However, there are several definite statements that can be made. This is an intelligent and sophisticated effort to relate mathematics, economics, and accounting and is worthy of the study of scholars. Anyone working in the broad area of the theory of the firm should read it.

HAROLD BIERMAN, JR.

Cornell University

Industrial Organization; Government and Business; Industry Studies

Economics of Research and Development. Edited by R. A. TYBOUT. Columbus: Ohio State University Press, 1965. Pp. xx, 458. $7.50.

This latest piece of evidence of burgeoning "research on research" is a collection of essays first presented at a conference in October, 1962. Reflecting the diversity of investigations going on in this area, the papers were variously written by economists, psychologists, sociologists, political scientists, industrial engineers, historians, and even an anthropologist. As usual in collections of this sort, the papers are uneven in quality, and regrettably the overall average leaves much to be desired.

Perhaps the best way to convey the flavor and text of this book is to run down the list of titles of its (seven) Parts, briefly indicating the nature of the contents of each. Part I, the History of Science, contains two papers, one on the role of the state in support of basic science since the seventeenth century, the other an historical review of the increasing specialization of scientific research and the organizational problems of coping with this development. Part II, Industrial Research and Development, contains three papers dealing mainly with the economic determinants of the level, and difference in level, of industrial R and D. A fourth paper, by the anthropologist, is a study of the impact of developments in routine engineering knowledge on productivity change; as a by-product, the paper also reports interesting information on the process of diffusion of new knowledge.

Part III, Internal Organization, has three papers dealing with the organization of research groups. One is an appraisal of the relevance of personality types for the problem of assigning research activities to scientists. The second is an examination of the influence of cooperation on research and quasi-research activities. In the nature of things, it should be of interest chiefly to administrators of research in industry. The third of the papers in Part III is a

study of the process of idea flow and project selection in industrial laboratories.

Three papers also comprise Part IV, on Public Policy and R and D. One is an effort to develop a tool for classifying the social costs and benefits of R and D—in a manner reminiscent of Sir John Clapham's "empty boxes." Then follow two papers dealing with the role of government in the allocation of R and D resources.

Part V, Military Research and Development, contains two economic discussions of R and D problems in the military sector. One is concerned with the proper conduct of R and D projects characterized by great uncertainty with respect to performance, reliability, and cost; as such, its applicability ranges well beyond the military end of R and D. The other is a description of the interrelations between the federal government and contractor firms in the acquisition of new weapons systems.

Part VI, International Collaboration, deals with two experiments in international collaboration in R and D, two papers discussing Euratom and a third the Mutual Weapons Development Program of NATO. Although hard pressed by some others, this group of papers is probably the least edifying in the entire book. Finally, in Part VII, we come to problems of R and D as they relate to the developing economies. One of the more interesting papers of the book is the first of this group, which is concerned with the implications for technology of the resource positions in which today's underdeveloped economies find themselves vis à vis the developed ones. In it, the editor of the book sets forth the thesis that "economic development in the new nations might well result in the long term coexistence of cottage industry and automated process manufacturing, omitting in the middle Western-style skilled labor manufacturing." The other paper is a plea for considerably increased research on social systems. This includes research on peace; on cultural, spiritual, and religious values, and the nature of man; on political power and processes (to relieve suspense, the author of this piece is Kenneth Boulding); and not necessarily least, on the population problem and technology suitable for the resources of underdeveloped areas.

Readers of this *Review* will probably find their professional interest stirred most by the papers in Part II and the first paper of Part VII. Since the contents of Part II fall somewhat within the special competence of this reviewer, let me close this review with a few of the remarks typical of book reviews. The paper by Jesse Markham is another in a series of efforts to link up the conduct of industrial R and D with industry concentration (oligopoly). How long efforts to maintain this Schumpeterian thesis will continue, it is hard to say. But there is mounting statistical evidence against the thesis, and none at all of which I am aware in its favor. In fact, the next paper, by Yale Brozen, reports on a very weak correlation between industry R and D performance and the degree of industry concentration. Although Brozen's data were rather old, his results have since been repeatedly confirmed by recent data.[1] About the only

[1] See D. Hamberg, "Size of Firm, Oligopoly, and Research: The Evidence," *Can. Jour. Econ.*, Feb. 1964; and F. M. Scherer, "Firm Size, Market Structure, Opportunity, and the Output of Patented Inventions," this *Review*, Dec. 1965; and the references in these articles to earlier corroberative studies.

defense left the supporters of Schumpeter in this matter would appear to be the one allegedly erected by Gustav Cassel in the course of an American lecture tour. "What," he was asked, "if the facts don't support your theory?" "Well, then," Cassel exclaimed, "the facts must be wrong." D. HAMBERG

University of Buffalo

The New Deal and the Problem of Monopoly—A Study in Economic Ambivalence. By ELLIS W. HAWLEY. Princeton: Princeton University Press, 1966. Pp. xv, 525. $10.00.

Professor Hawley has provided a definitive history of national industrial policy in the United States between 1933 and 1939. His footnotes indicate the intensity and wide range of his study. His immediate interpretations are well founded, enlightening, and without bias. His book will become an invaluable secondary source.

He sees the widespread disillusionment with the industrial system in 1933 as generating demands for political action along three different lines (which still persist). One group would have relied on antitrust policy to restore competition (and flexibility of prices), thus achieving a reorganization of the industrial system while at the same time implementing democratic values. Another group favored over-all economic planning, while a third urged government sponsorship of cartels and business planning.

The National Industrial Recovery Act began by giving some comfort to each of these groups. But in administration it developed almost a sustained stalemate, and by the time it was struck down by the Supreme Court it had few friends. In fact its main achievement was to fortify private monopolistic arrangements for a time. In its wake came a policy of government sponsorships of cartels, restricted, however, to a few industries (petroleum, coal, and transportation) and to the distribution trades. Each of these was in a position to conceal its drive for profitable restrictions by reference to the conservation of natural resources, national defense, protection of the small man, and the like. But over-all government planning was no longer politically feasible. Following the depression of 1947 antitrust policy was reinvigorated under Thurman Arnold, but its implementation was cumbrous, and it was not unifomrly applicable. Consequently it failed as an anticyclical weapon or as a means to economic expansion and faded away in the nationalistic glare of approaching war. In the end the policy of the Roosevelt administration was one of counterorganization, or creating monopoly power in areas previously unorganized (labor, agriculture).

The change from time to time in the relative weight of each of the three views on government policy always left some room for each of them. Policy throughout the period was, therefore, a bundle of inconsistencies. These changes were due in part to changes in economic conditions. Depression revived the argument about rigid prices and gave renewed vigor to pressure for more active antitrust policy. But it also enlivened arguments for planning. Such planning as occurred was partial and, as indicated above, under shelter of special circumstances.

The persistent pursuit of policies made up mutually conflicting elements calls for explanation. Hawley blames a popular demand, on the one hand, to

be allowed to continue to enjoy the material goods and services provided by an industrial order with a high degree of collective organization and, on the other hand for the maintenance of a system as free as possible from centralized control and adjusting itself automatically to market forces while at the same time preserving human dignity. But the survival of this ambivalence itself requires explanation.

The period of Hawley's study is too short to help greatly in explaining this survival. He doubts, however, that the experience of the thirties will reduce confusion in future policy even though economic conditions have changed. Monopoly and rigid prices are no longer regarded as major causes of depressions. Inflation, labor power, and the wage-price spiral are of more concern, but point toward monetary and fiscal policy. Nevertheless the economic power of monopoly is still feared and its contribution to the waste of resources and inequality of incomes criticized. Even so, current policy, like that of the New Deal years, is still a maze of conflicting cross currents. In this respect there is, seemingly, no trend. Future policy is expected to "remain confused and contradictory . . . programs designed to combat monopoly will still be intermingled with those designed to promote it." This conclusion looks less bold, however, in view of the exclusion of "revolution, or drastic changes in techniques, attitudes, values and institutions." In fact it almost comes to saying that in the absence of the major influences making for change there will be no change.

The pessimistic conclusion that confusion and ambivalence are likely to continue presumably rests partly on Hawley's view that the experience of the thirties, if relevant, suggests "the limits of logical analysis and the pitfalls inherent in broad theoretical approaches" as well as the necessity for making due allowance for the intellectual heritage, current trends of opinion, and the realities of pressure group politics. It is unfortunately true that economic analysis has largely failed to specify the causes of industrial concentration or to measure its consequences in terms of either the efficiency of the use of resources or the distribution of incomes. Elaboration of the monopoly concept into a family of imperfections or kinds of monopolistic competition has not helped. If economists had been more successful, perhaps some of the arguments of the three pressure groups upon which Hawley's analysis is based might have been undermined. But, as he indicates, the intellectual heritage, current trends of opinion, and like factors also contribute to the formation of public policy. Although economic analysis might reduce their influence, it cannot eliminate it. But in this respect does social policy regarding the industrial system differ greatly from social policy in many other socioeconomic fields? Problems become a matter of political concern precisely when the need for compromise among objectives becomes a pressing need.

Everyone interested in industrial organization and control needs Hawley's work and will enjoy his lucid presentation of a complex problem.

ARTHUR ROBERT BURNS

Columbia University

Inter-Economy Comparisons: A Case Study—A Comparative Study of Industrial Development, Currency Devaluation, and Inflation. By LEONARD A. DOYLE. Berkeley and Los Angeles: University of California Press, 1965. Pp. xiii, 232. $6.00

Even the full title of this volume does not suggest its contents. It is not concerned with intercountry comparisons of national product, industrial structure, prices, or exchange rates. Rather, the study aims at comparing the capital and operating costs of two cement plants, one the cement plant of the Indonesian Government at Gresik in East Java and the other the Cushenberry plant of the Permanente Cement Corporation in Southern California. Yet the title is not altogether misleading since the comparisons do involve some consideration of the effects of inflation and exchange rate variation; presumably the attempt to determine the contribution of the Gresik plant to the economy of Indonesia justifies the reference to "industrial development."

There has been much speculation and little systematic enquiry regarding the ability of the less developed countries to absorb modern technology. This study focuses on two large plants of similar design and vintage (both came into operation in 1957) in order to explore some of the issues, especially those involved in making cost comparisons. But while developments in Indonesia towards the end of the 1950's were such as to enliven the investigation, Professor Doyle's painstaking cost-accounting approach has to hurdle three difficult obstacles, viz. rapid price inflation, government ownership, and a complex and unstable exchange rate situation. A plant in some more placid setting would have reduced the number of variables and greatly simplified the task of arriving at meaningful conclusions.

The greater part of the book (five chapters out of eight) consists of a discussion of the principles underlying the author's detailed computations of investment and operating costs, with special attention devoted to such subjects as the measurement of depreciation and of interest charges in a socialist economy suffering from rapid inflation. Ordinary business accounting concepts are adapted to the special circumstances surrounding the comparisons, and much space is devoted to justifying these departures from conventional practice.

Despite Doyle's careful examination and application of accounting precepts, the reader who is interested in what he can learn from the figures will often be at a loss, the more so as the author fails to summarize and interpret his findings. While the costs of the Gresik plant incurred in rupiahs are perfectly definite in that currency, the rupiah value of imports depends of course on the exchange rate used. Not only did the rate change; at no time did the official rate, which Doyle generally uses, come very close to measuring the real purchasing power of the rupiah vis-à-vis the dollar. In these circumstances not only the U.S.-Indonesian comparisons of capital and operating costs are indefinite, but even comparisons of relative costs are affected by the rate because of imports. This reviewer found it difficult to know what to make of many of the calculations. Or, for that matter, of such statements as: "Whatever one's views of the theoretical procedure necessary to compare investment in a country with an overvalued currency, the practical solution must be to accept the official rate for statistical purposes."

To the extent that the author provides comparable information in physical units, the results are of greater interest. We learn, for instance, that the Gresik plant employed almost 5 times as many workers (and paid them only 6 per cent as much, at the official rate); yet Cushenberry's output was over one-and-a-half times as high. Moreover, investment in the Gresik plant, no matter how measured, substantially exceeded the cost of the Cushenberry installation. The reasons behind these differences are interesting. The Indonesian firm had to provide housing, a school, a fleet of buses, recreational facilities, etc. for its workers; and electric power, docking, oil storage and rail facilities, and even a paper bag factory for its operations. The warehouse had to be larger than in the United States so that an adequate supply of spare parts could always be kept on hand, and the machine shop also had to be more elaborate to make or repair equipment which in the United States could be made or serviced by outside firms. Clearly, a very much greater input of capital and labor is required to produce cement in a country still lacking the social and other overhead capital which in industrialized countries is taken for granted and shared broadly. The larger Gresik labor force was in part also attributable to special factors: the Gresik plant is a state enterprise, unemployment is widespread, the wage level is low in relation to the value of the product, the wage spread is narrow for workers at different levels, etc.

Doyle tries to assess the importance of the Gresik plant to the Indonesian economy, and concludes that for all practical purposes the net benefit is the saving in foreign exchange on cement imports ("the real costs represented by managerial and operating personnel and by domestic materials and supplies can be regarded as zero"). In 1959 the foreign exchange saving amounted to around $4 million. In the same year the marginal contribution to Indonesia's foreign exchange position of a ton of cement was at least $16, after allowing for all dollar costs, including depreciation on a replacement basis of the dollar portion of the investment. In the circumstances, as he points out, a policy of keeping the plant short of the foreign exchange needed for expansion and efficient operation would be shortsighted.

Doyle has some provocative points to make, of which the following is an example:

> In selecting industrial projects in a developing economy it may be desirable to favor those for which the ratio of local currency to foreign exchange is relatively low. In spite of the rather general belief that newly independent nations require enormous sums of foreign exchange for capital investment, the fact is that the chief limitation on industrial capital formation is often local currency rather than foreign exchange. This is particularly true of countries, such as Indonesia, that elect state enterprises rather than private foreign investment (p. 31).

Neither this argument nor a number of the others are developed as fully as they might be, however, and supporting references to the literature are almost entirely lacking.

In a book that attempts to cover engineering, accounting, and economic considerations, careful organization is especially important. Many readers will

justifiably feel that the author could well have given more thought to the arrangement of his material.

WILLIAM I. ABRAHAM

New York University

The Economics of Regulation—Theory and Practice in the Transportation and Public Utility Industries. By CHARLES F. PHILLIPS, JR. Homewood, Ill.: Richard D. Irwin, 1965. Pp. xvi, 783. $10.00, college price.

One advantage of a dilatory review of a book which has already become a standard text is that it may serve to convert the heathen. To this end, let us first appreciate a passage which is typical of most of the book:

> In general, commissions have acted only in response to complaints or when a company has filed for a rate increase. Few investigations have been started on their own initiative. . . . Most of the commissions have been compelled to devote their time to the determination of the rate level, leaving little time to spend in investigating the rate structure. But even when the rate level has changed, the commissions have tended to approve across-the-board increases, thereby permitting all rates in the structure to move together and preserving the existing pattern of differentials. . . . (p. 389).

This passage is not quoted because it is especially wise or especially felicitous, but because it is representative. It projects truth and common sense. Anyone familiar with the field of public utility regulation would agree with most of it. Most would agree with all of it. But, aside from Emery Troxel, not many economists have thought to spell out the point so carefully.

In addition, Charles Phillips is conspicuous for his homework even in a field which abounds in homework. The sources are here in abundance: old and new, theoretical and institutional. After endless exposure to Interstate Commerce Comm. v. Alabama Midland Ry. Co., 168 U.S. 144 (1897), I was still pleased to meet someone who had obviously gone all the way back to the decision, and who could summarize the special frame of reference so crisply:

> . . . when evidence was presented to the Court, it was shown that the river—the water competition shown in the evidence before the ICC—was dry half of the year, that it was but three feet deep, and that the presence of overhanging trees made the use of boats impossible (p. 111).

But, like all homework, this takes time. And the opportunity cost can be considerable. For example, if there is one question upon which economists have been agreed almost since the profession began, it is the economics of the back haul. If there is also one issue on which the Interstate Commerce Commission has covered itself with shame, it is this same issue of the back haul. Phillips quotes one of the least worthy of the Interstate Commerce Commission's exercises, as reported at 4 MCC 189 (1938):

> . . . the traffic of one truck operator may preponderate in one direction, whereas that of a competing operator may preponderate in the other. As between operators, therefore, the application of the "out-of-pocket" cost method of making rates might well result in a break-down of the rate structure in both directions.

This is a perfect example of the kind of economic hobgoblin with which the Interstate Commerce Commission cannot contend, for the excellent reason that it exists only at Twelfth Street and Constitution Avenue. But Phillips refuses to exorcise it. Instead he comments:

> Compared with the railroads, therefore, where an unbalanced movement of traffic in one direction is frequently encountered by all carriers, a particular motor carrier may have a back-haul problem in one direction, while another carrier faces a similar situation in the opposite direction. To allow both motor carriers to reduce their back-haul rates to out-of-pocket costs would not be justified ... (p. 381).

Generalizing from the Interstate Commerce Commission and from Phillips, one would have to conclude that load factor has nothing to do with costs, that unemployment of factors should be protected against employment, and that a principal object of regulation is to produce waste in the place of efficiency. This perversity appears only rarely in Phillips—much less rarely than in Interstate Commerce Commission decisions—but in an economic text it should not appear at all.

Which brings us to the central question: what books have a comparative advantage in which markets?

1. Phillips should be in every reputable library. The style is intelligible to all and condescends to none. The factual statements are remarkably accurate. The research and scholarship embodied in the book are formidable.

2. When considering Phillips as a text, the first question must be: why is a government regulation course in the curriculum? If this can be answered, the second question might be: can any one book be both theoretical enough, and factual enough, to carry the entire load in such a course? If the response to this is negative, as I suspect it must be, the question is: does Phillips earn high marks on the factual material? The answer to this is a most emphatic "yes."

3. Finally, then who does carry the load of theory? Perhaps some economists would modestly nominate themselves, in their classrooms, for this honor. But if they seriously believe that they can convey economic theory by their own unaided lectures, they probably have not been reading their examinations very carefully. And, for all its virtues as a compendium and as a careful, scholarly, and dispassionate summary of how things really are, the Phillips book will require them to lengthen their reach to pull its institutional material closer to standard analysis.

The trouble with microeconomics is that it is so cluttered with facts. This is the nature of the beast. No amount of free-lance theorizing can alter it. *The Economics of Regulation* should be praised for adding to the pre-existing stockpile of facts. But it cannot be praised for pursuing a sometimes uneasy course between economics and other disciplines.

The Phillips book already occupies a secure niche in economics, as indeed it should. But its educational value would be greater, and its niche more secure, with more rigorous economic and financial analysis.

James R. Nelson

Amherst College

Labor Economics

Hoffa and the Teamsters—A Study of Union Power. By RALPH JAMES AND ESTELLE JAMES. Princeton: D. Van Nostrand Co., 1965. Pp. xviii, 430. $6.95.

The ease with which David Beck, former president of the Teamsters' Union, was toppled from power under the onslaught of the McClellan Committee led many a superficial observer of the labor movement to assume that James Hoffa would follow the path of his predecessor when McClellan's sleuths turned their attention to him. Yet, paradoxically, Hoffa's power within his organization seemed to thrive and increase under the repeated onslaughts of the Committee and the former Attorney General of the United States, Robert Kennedy. The expulsion of the teamsters from the house of labor, seemed to have little impact on the teamsters' fortunes under Hoffa's leadership. They continued to gain both in membership and economic improvements for their constituents.

This seeming paradox is studied in depth by Ralph and Estelle James. They have given us a penetrating story of a man, an institution, and its impact on the larger society within which both leader and organization function. The writers were granted a unique opportunity by Hoffa himself to spend some 90 days as part of his entourage observing him in action at close quarters. Ralph James was introduced as an egghead member of the staff at all private conferences. The resulting account of their observation is a tribute to their scholarship and integrity, and incidentally a treatise on how to do scholarly research without being seduced by a sponsor. This is no small achievement for the social scientist who is confronted by the problem of keeping his channels of inquiry open despite the knowledge by the sponsor that the findings reported may be something less adulatory than the typical institutional *Festschrift.*

The Jameses have avoided two traps— the temptation to yield to the sensational journalese of the inside dopester on the one hand, and on the other to reduce the colorful actors in a richly human drama to the role of abstract beings in the grip of abstract forces.

What emerges is the following: (1) a psychological study in depth of the complex personality of James Hoffa; (2) a dynamic study of the nature of negotiation and administration of collective agreements by an intuitive expert; (3) a first-rate economic analysis of the impact of the Teamsters' Union upon the fortunes of its constituents and the economy at large.

A much more complex personality emerges from the pages of the James's book than the simple-minded thug portrayed in the newspapers at the time of the McClellan hearings. Hoffa emerges, ruthless to be sure, but nevertheless sincerely concerned about the welfare of his men. Endowed with a brilliant mind, but one not given to abstraction, he is a master craftsman, drawing upon the contents of a file-cabinet brain in which are compartmentalized the immeasurably complex details of all teamster agreements.

What makes Jimmy tick? The Jameses express their complex frustration in attempting to understand him. He is a man who brooks no opposition in carrying out policy, yet maintains on his payroll outspoken oppositionists to his policies. He is a complete puritan in his personal habits, eschewing smoking,

drinking, and sexual escapades, yet tolerating around himself men whose incompetence is exceeded only by their bawdy lechery.

Completely contemptuous of labor history, he is the labor counterpart of the Senior Henry Ford to whom history was bunk. Yet he is utterly captivated by Farrel Dobbs, the intellectual Trotskyite, whom he acknowledges as his mentor in changing him from a young punk to a superstrategist with a new concept of the strength of the teamsters' organization.

Hoffa has built upon the strategic organizational concepts laid down by Farrel Dobbs. It was Dobbs, with the aid of the Dunne brothers, who understood the strategic importance of the over-the-road truck driver in making the Teamsters the keystone of the Twin Cities' labor movement, to whom all other unions looked for aid.

The Jameses give a detailed picture of the way Hoffa has used this strategic concept to convert a collection of independent local baronies held tenuously together by per capita payments to old Dan Tobin, former national president, into a nationally integrated organization with national agreements. They credit Hoffa with mastery of the intricately detailed economies of the trucking industry. The Jameses indicate that, through collective bargaining, Hoffa has been able to do more for his members than other leaders of more respectable stamp, but possessing less strategic power.

The authors indicate that Hoffa's manipulation of the pension funds may entail a future cost when many members will blame future administrators for not paying the pension which the members think they have gained. On the other hand, they reject the moral evaluation of Hoffa by Senator McClellan as the labor movement's worst enemy.

Hoffa does emerge as a power-loving despot, but then what leader of an organization doesn't love power for power's sake? The Jameses are inclined to believe that Hoffa's reputation stems more from the early days of Detroit's violence in which he gave as good as he got.

What makes the James's study so refreshing is the manner in which their problem dominated their techniques. This study is an academic revival of the John R. Commons' institutional tradition in which scholars were dominated by the interest evoked by their problem rather than the status stemming from a pyrotechnical display of powerful tools subjected to trivial problems.

Their work is an indispensable tool for the student of labor or transportation.

William Gomberg

University of Pennsylvania

Labor Migration and Economic Growth—A Case Study of Puerto Rico. By Stanley L. Friedlander. Cambridge, Mass.: The M.I.T. Press, 1965. Pp. ix, 181. $6.00.

This book is based on the author's doctoral dissertation. Its subject is the extent to which migration from Puerto Rico to the United States has aided the economic development of the island's economy, and its goal is to demonstrate that large-scale emigration from Puerto Rico to the United States was crucial in raising per capita incomes in Puerto Rico and in supporting the rapid rate of economic growth the island has experienced since 1945. Chapter 1 is intro-

ductory, and in Chapter 2 the author presents a theoretical analysis of the effects of emigration on economic development. The remaining five chapters deal specifically with the Puerto Rican experience; the last chapter summarizes the study and suggests applications of the Puerto Rican experience to the problems of other underdeveloped areas.

Chapter 2 occupies 32 pages, and I feel it should have been shortened by about 90 per cent. The author admittedly derives much of the analysis from an article by R. R. Nelson in this *Review* (December, 1956). The essential points are that in overpopulated areas population growth responds very rapidly to economic development because both fertility and (especially) mortality respond (positively and negatively, respectively) to rising income levels. Thus since increases in national income are not matched by increases in per capita income, but possibly by decreases, either substantial investment of foreign capital and/or large-scale emigration are necessary to promote increases in per capita income and, consequently, saving, which eventually steer the developing economy toward self-sustained economic growth.

Not only has the logical framework of Chapter 2 been worked out elsewhere, but the terminology and symbolism used by the author seem to confuse, rather than to clarify, the theory. For instance, in equations (2.1) through (2.3) on page 13, the author states:

(2.1) $Y = f(K)$

and

(2.2) $S = \Delta K$ (by definition),

where K is the capital stock, S is saving, and Y is national income. He then substitutes (2.2) into (2.1), obtaining

(2.3) $Y = f(S)$.

But if (2.3) is true, it does not seem to follow from (2.1) and (2.2). Subsquently it is stated that "the rate of growth of output is a function of the change in capital formation . . ." and

(2.5) $d(Y/Y) = h(d(S/P))$ *sic!*

where S/P is per capita saving. Much of the remainder of Chapter 2 is equally confusing.

In Chapter 3, the author discusses emigration and population growth in Puerto Rico; there is a summary of economic and demographic conditions as of 1940, and aspects of demographic changes and migration since then. Between 1945 and 1962, nearly 600,000 Puerto Ricans emigrated to the United States; this constituted 85 per cent of the total emigration during the twentieth century. That this was a substantial number of people can be seen by comparing the figure with the 1960 population of Puerto Rico—2,349,544. The author emphasizes that the figures probably understate the importance of migration to the population growth of the island, since emigrants tended to be in the most fertile age groups and would have produced many children who, as it turned out, were born on the mainland. (Emigrants in the 14-34 age groups

numbered 346,107, compared with a population of 681,434 in the age group on the island in 1960.)

Subsequently (p. 57) the author discusses his calculation of the amount of Puerto Rican emigration from 1940 through 1960. The estimate—577,994—is based on a comparison of the "expected" population (derived from base-year population, birth, and death rates) with the "actual" population over the period observed. He compares his estimate with "migration statistics" and notes almost perfect correspondence; but the author does not state the source of the "migration statistics," and one suspects they were based on the same sort of estimates as the author's, especially since data from other sources (probably annual estimates of net departures) presented in Appendix A suggest that migration over the same period amounted to 600,500. While the discrepancy is not large, the author's vagueness in describing the accuracy and sources of his data is annoying and unnecessary.

In Chapters 4 and 5 the author discusses the causes and effects of the skill mix of the migration stream. The principal hypothesis tested is "that the occupational earnings differentials would stimulate the movement of a larger proportion of unskilled to skilled laborers than the existing ratio in the economy after a minimum income per capita is attained" (p. 75).

The author notes that the labor force in Puerto Rico grew slowly, or shrank, during the period of high postwar emigration (depending upon which dates are compared). This would seem to be caused mainly by the typically high labor force participation rate in the age groups constituting the bulk of the migration stream. The author sets himself the task of showing that despite the high labor force participation of the emigrants, migration caused Puerto Rican per capita income to *increase* because the quality of the labor force was improved. The author relies on two sources of data to estimate the impact of the migration on the skill mix of the Puerto Rican labor force. One is a comparison of the 1950 and 1960 occupational distribution of Puerto Ricans in the United States with the occupational distribution in Puerto Rico. He correctly recognizes that the occupation titles of jobs held by Puerto Ricans in the United States may well be different than those that would have been held by the migrants if they resided in Puerto Rico; he further suggests that the occupational mix of the migrants in the United States is more highly skilled than it would have been in Puerto Rico. I am inclined to agree. The comparisons imply that the migrant group in the United States contains a much higher proportion of operatives and service workers, other than domestic, and the same or lower proportions of other occupations (including professional and technical; farmers and farm managers; owners, managers, and officials; clerical and sales; craftsmen and foremen; domestic service; and laborers) than does the labor force of Puerto Rico. Thus, if the migrants were removed to Puerto Rico and placed in their United States occupational categories, the proportions of these other occupations in Puerto Rico would be unchanged or reduced. Whether this suggests, as the author asserts, that the migration improved the skill mix of the Puerto Rican labor force, seems unclear to me. On the basis of the 1950 and 1960 censuses, it seems more likely

that migration increased the proportions of workers at both extremes of the occupational distribution in Puerto Rico.

Data from airport surveys conducted since 1957, however, suggest that perhaps half of the experienced workers in the migration stream were from agriculture. The author uses these data to support his hypothesis that migrants substantially upgraded their occupational status upon emigrating to the United States, and that the average occupational and skill level of Puerto Ricans emigrated has been lower than the average of those who remained. However, this leaves the author in the difficult position of having to reconcile his conclusions about the skill mix with data showing that in both 1950 and 1960, in practically all age groups, the educational level of migrants was higher than the average educational level of the population of Puerto Rico. He rationalizes the apparent contradiction by asserting that "the educational level of the migrants is not significantly higher than the remaining population in terms of qualifying for more advanced occupations," and that "the migrants may have a higher education than the people in Puerto Rico but a lower education than the people in the United States" (pp. 102-3). I find both of these arguments unconvincing. It is the effect of emigration on the average quality of the Puerto Rican labor force that is the issue; it appears that no matter which way one views the educational data, the emigrant Puerto Ricans had no less formal education than those who did not emigrate.

Perhaps one source of the apparent contradiction between the occupational mix and educational mix of the migrants is found in data reported in Chapter 5. Information in the airports' surveys of Puerto Rican emigrants suggest that, in all years from 1957 through 1961, slightly over one-half of the passengers interviewed were not in the labor force. (This group included students.) It is possible that one of the causes of migration was the difficulty of relatively able and well-trained persons in Puerto Rico to obtain employment in relatively skilled jobs at home. Thus, persons who were not in the labor force in Puerto Rico, or who were forced by lack of job opportunities to take low-paying, unskilled jobs, found opportunities for the employment of their talents more favorable in the United States. A possible source of such interregional differences in the ease of entry into the types of jobs the migrants have found on the mainland may be the relatively very high minimum wage wages rates imposed in Puerto Rico. Minimum wages in Puerto Rico and some of their effects on the growth of employment opportunities have been discussed by Lloyd Reynolds in the March, 1965 issue of this *Review*.

In Chapter 5, the author discusses the causes of the skill mix of the migration stream and cites evidence that the principal cause was "[differences] in the sizes of the absolute [earnings] differentials (in percentage terms) among occupations" (p. 119). Unfortunately, the author is not able to compare directly the alternative wages of migrants in Puerto Rico and in the United States, as data are available only on earnings in occupational categories; they are not classified by the national origin of the workers employed in them. Thus, comparisons are not made between identical occupations in Puerto Rico and the United States, but between occupations which the author feels Puerto

Rican migrants have been most likely to leave in Puerto Rico and to enter in the United States.

Earnings of farm workers in Puerto Rico are compared with earnings of custodians in New York, and so on. In 1952, the earliest year in which comparisons between weekly earnings are made, the differences ranged from a low of $30.45 between office workers in the United States and clerical and sales workers in Puerto Rico and a high of $51.10 between craftsmen in the two areas. In 1962, the same occupations also displayed the lowest and highest differences—$45.82 and $76.60, respectively. The author theorizes that the reason farm workers constitute the largest single occupational group in the migration stream despite the fact that their earnings gain (the difference between median weekly earnings of custodial workers in the United States and farm workers in Puerto Rico was $72.60 in 1962) would appear to be somewhat less than that of craftsmen, is that their *relative* gain (the proportionate increase over their Puerto Rican alternatives) is greatest. I find this rationalization unpersuasive, both because the differences in earnings gains do not seem large enough to explain the occupational mix of the migration and also because equally plausible alternative hypotheses are either rejected almost out of hand or not even considered.

The author finds that "The migrants did not respond to the general rate of unemployment for all Puerto Ricans in the United States, nor did they respond to unemployment rates by occupations as they did to earnings differentials by occupation." His evidence is based mainly on a comparison of the differences in aggregate and occupational unemployment rates between Puerto Rico and the United States for 1960 and 1950. It is true that for most occupations, but not in the aggregate, the unemployment rate, as reported in the United States *Decennial Census of Population*, was higher in the United States than in Puerto Rico. This was true even for operatives, who constitute about half the Puerto Rican labor force in the United States. Unfortunately, it is dangerous to infer much from a comparison of decennial census unemployment rates, as it is widely recognized that the number of marginal workers was typically underreported; the underreporting seems to have been very great in Puerto Rico, where the census unemployment figure for 1950 was just over 5 per cent while the Puerto Rico Bureau of Labor Statistics reported a rate of 14.2 per cent for men and 11.8 per cent for women. The Puerto Rican official statistics are thought to be more reliable than those collected by the Census Bureau. (See, for instance, A. J. Jaffe, *People, Jobs, and Economic Development*, Glencoe, Ill., 1959, pp. 275-76.) I have not examined the 1960 data for accuracy, but I suspect that, while reporting in the decennial census has improved, important errors still remain.

An alternative hypothesis not considered by the author is noted above; high minimum wage rates may have restricted the entrance of relatively well-trained but inexperienced workers from entering the more skilled occupations. (On this point, see also M. W. Reder, "Wage Structure and Structural Unemployment," *Rev. Econ. Studies*, 1964, *31* (4), p. 316n.). It is also likely that the occupational mix of the migration is partly a function of the age of the migrants. Persons most likely to move *cet par.*) tend to move while they are

young. Thus, the more highly skilled members of the Puerto Rican labor force may well be those who, for personal reasons, have elected not to emigrate. (See, for instance, Gary S. Becker, *Human Capital,* New York, 1964, pp. 50-51.) Even in the short run, the young have fewer commitments, economically, sociologically, and psychologically, than the old.

Chapter 6 contains an analysis of the effects of labor quality on the growth of output and productivity. The author states that "falling average productivity implies falling marginal productivity of capital. Apparently investment in Puerto Rico has not decreased in response to the declining marginal productivity of capital. . . . In any event rising real wages did not seem to retard investment on the island." Not only is the argument marred by the logic involved in the average-marginal statement, but no convincing empirical evidence is presented to support the author's assertion regarding the effect of rising wages on investment. Finally, the author moves from his assertion that emigration, by improving the quality mix of the Puerto Rican labor force, raised income per capita, to the conclusion that "emigration, therefore, played an important indirect role in stimulating capital formation by allowing gains in output to be translated into gains in income per capita, which stimulated-savings per capita throughout the economy" (p. 140). This appears to be an important part of the analysis of Puerto Rican emigration, since if this process were to occur elsewhere, it would, in the terminology of the book, assist underdeveloped countries to achieve self-sustained economic growth. Unfortunately, the author fails to note that net foreign investment (mostly by the United States) in Puerto Rico appears to account for a substantial proportion of capital formation on the island. According to the *Statistical Yearbook of Puerto Rico,* net foreign investment amounted to more than 85 per cent of the level of gross domestic investment from fiscal 1957 through fiscal 1961. (I am indebted to my colleague, Walther Michael, for discussion of this point.)

The chapter concludes with some estimates of production functions for the Puerto Rican economy in an attempt to estimate the influence of education and improved labor force quality on Puerto Rican economic growth.

While this book deals with an important subject, I cannot recommend it as required reading for those wishing to learn something about the role ef emigration in aiding overpopulated and underdeveloped countries to achieve self-sustained economic growth. It is clear that Puerto Rico's nearly unique connection with the United States has overwhelmed most evidence that can be gained regarding the influence of emigration on domestically financed capital formation. It does seem plausible that, given the rate of capital formation in Puerto Rico, emigration raised the productivity and the wages of both emigrants and those remaining on the island. Moreover, it does not seem implausible that if emigration had been smaller and perhaps, minimum wage rates less rigorously applied, capital formation would have been still greater than the large amounts that were in fact achieved.

BELTON M. FLEISHER

Ohio State University

Fringe Benefits: Wages or Social Obligation? An Analysis with Historical Perspectives from Paid Vacations. By DONNA ALLEN. Ithaca, N.Y.: Cornell University, New York State School of Industrial and Labor Relations, 1964. Pp. ix, 273. $4.75; paper, $2.50.

This book is an analysis of the development of fringe benefits for wage workers in U.S. industry. Miss Allen illustrates her analysis with the history of one type of fringe benefit: annual vacations.

Miss Allen's thesis is that many (though not all) fringe benefits have undergone an evolution similar to that of vacations. In the first phase, management accepts a "social obligation," such as providing an annual rest period for its employees. The second, or union, phase has three stages: first, the unions insist that the company include this fringe benefit in the contract; second, they proceed to liberalize it; third, they work to eliminate the management qualifications and restrictions upon the right to the social benefits. From this it is but a short step to the position that the fringe benefit is in fact a form of deferred wages. In this interpretation, vacation pay is money earned by the worker during his period of employment, and put aside for him by the employer.

Miss Allen believes that this trend has most unfortunate consequences. In the first place, the "deferred wages" doctrine has enabled employer-dominated state unemployment compensation boards to deprive employees of their unemployment benefits while receiving vacation pay, severance pay or similar benefits. She feels that if the deferred-wages doctrine were taken to mean that these benefits were simply funds owed by the company, then the worker would *not* be deprived of his unemployment benefits. However, she feels that this doctrine permits a biased interpretation—that the money is owed specifically for time not worked.

Her second, more fundamental, objection to the deferred wage doctrine is that, when we regard payments into vacation, pension, or unemployment funds as part of the employee's wage, we remove the company's social obligation to provide for its employees.

Miss Allen views the postwar period as one in which the political climate has been unfavorable to the labor movement's efforts to secure social legislation, but in which the economic situation has favored the more powerful trade unions in their struggle to wrest these social benefits directly from employers. Thus, it seems unfortunate to her that the unions have been undermining the view that employers have a social obligation to their workers by insisting that not only does the worker have a right to these fringe benefits, but that these benefits are in fact a form of wages for services rendered.

However, it is not necessary to share Miss Allen's sympathetic view of corporate paternalism in order to appreciate this book. The history of the annual holiday in the United States from 1900-1960 is presented at length. Not only are the quantitative gains in the spread of vacations chronicled in some detail, but a very interesting analysis is given of the changing employer and employee attitudes towards this custom.

JOHN D. OWEN

The Johns Hopkins University

TITLES OF NEW BOOKS

General Economics; Methodology

BACH, G. L. Economics—an introduction to analysis and policy. 5th ed. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xv, 728. $8.95. Teacher's manual to accompany text. Pp. ii, 174. First ed. reviewed in 1954 by E. D. Hawkins.

BONGARD, W. Nationalökonomie, wohin. Realtypen des wirtschaftlichen Verhaltens. Cologne: Westdeutscher Verlag, 1965. Pp. 132. DM 16.80.

CAIRNCROSS, A. Introduction to economics. 4th ed. London: Butterworth, 1966. Pp. xii, 665.

CARELL, E. Allgemeine Volkswirtschaftslehre. Heidelberg: Quelle & Meyer, 1966. Pp. 555. DM 28.

CLIFFORD VAUGHN, F. AND M., comp. Glossary of economics—including Soviet terminology—in English/American—French—German—Russian. New York, London and Amsterdam: Elsevier, 1966. Pp. 201. $12.

COPELAND, M. A. Toward full employment in our free enterprise economy. The Millar Lectures no. 7. New York: Fordham Univ. Press, 1966. Pp. xiii, 80. $3.

COURTHÉOUX, J-P. La répartition des activités économiques. Paris: Centre de Recherche d'Urbanisme, 1966. Pp. viii, 274. F 15.

DYE, H. S., MOORE, J. R. AND HOLLY, J. F. Economics: principles, problems, and perspectives. Boston: Allyn and Bacon, 1965 (4th printing). Pp. xii, 627. $8.50.

FELS, R. An introduction to economics—the challenge to the American economy. 2nd ed. Boston: Allyn and Bacon, 1966. Pp. xvi, 698. $8.50. First ed. reviewed in 1961 by K. A. Knopf.

GNANADOSS, B. A critique of economic reasoning. Madras: Fenn Thompson & Co., 1966. Pp. ix, 158. Rs 15.

GRAYBEAL, D. M. Can't we all be rich? New York: Friendship Press, 1966. Pp. 158. Paper, $1.75.

HAVENS, R. M., HENDERSON, J. S AND CRAMER, D. L. Economics: principles of income, prices, and growth. New York: Macmillan; London: Collier-Macmillan 1966. Pp. xvi, 623.

JOSEPH, M. L., SEEBER, N. C. AND BACH, G. L. Economic analysis and policy—background readings for current issues. 2nd ed. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xv, 576. Text ed., $3.95.

JOSEPH, M. L., SEEBER, N. C. WITH BACH, G. L. Workbook in economics. 3rd ed. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xii, 244. Text ed., $2.95.

LUMSDEN, K., ATTIYEH, R. AND BACH, G. L. Microeconomics—a programmed book. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. ix, 179. Text ed., $2.95.

MCCONNELL, C. R. Economics—principles, problems, and policies. 3rd ed. New York: McGraw-Hill, 1966. Pp. xxv, 792. $8.95.

MONTGOMERY, J. D. AND SMITHIES, A. Public policy. Vol. 14. Cambridge: Grad. School of Pub. Admin., Harvard Univ.; Harvard Univ. Press, distrib., 1965. Pp. 455.

REYNOLDS, L. G. Economics: a general introduction. Rev. ed. Homewood, Ill.: Irwin, 1966. Pp. xxi, 964. $8.75. First ed. reviewed in 1963 by L. E. Fouraker.

SHERMAN, H. J. Elementary aggregate economics. New York: Appleton-Century-Crofts, 1966. Pp. xvi, 249. Paper, $2.95.

SMITH, R. S. AND DE VYVER, F. T., eds. Economic systems and public policy: essays in honor of Calvin Bryce Hoover. Durham, N.C.: Duke Univ. Press, 1966. Pp. xix, 274. $10.

TOBIN, J. National economic policy—essays. New Haven and London: Yale Univ. Pre 1966. Pp. x, 211. $5; paper, $1.75.

Seminario: "La investigación económica y social en las facultades de ciencias económic chilenas." Inst. Econ. pub. no. 80. Santiago: Inst. de Econ., Univ. de Chile, 1965. Pp. 172.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

BOUQUEREL, F. Les études de marchés. Que sais je?: Le point des connaissances actuell no. 1219. Paris: Presses Univ. de France, 1966. Pp. 128.

BROWN, M. On the theory and measurement of technological change. Cambridge: Car bridge Univ. Press, 1966. Pp. xii, 214. $8.50.

CHRISTALLER, W. Central places in Southern Germany. Transl. from Die Zentralen Orte Süddeutschland by C. W. Baskin. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. 23 $9.95.

COLLERY, A. National income and employment analysis. New York and London: Wiley, 196 Pp. xiv, 154. Paper, $1.95

DOWD, D. F. Thorstein Veblen. New York: Washington Square Press, 1966. Pp. xvii, 2C $3.95.

GÄFGEN, G. Grundlagen der Wirtschaftspolitik. Cologne: Kiepenheuer & Witsch, 1966. Pp. 51 DM 32; paper, DM 19.80.

HEINEN, E. Das Kapital in der betriebswirtschaftlichen Kostentheorie. Möglichkeiten ur Grenzen eines produktions- und kostentheoretischen Analyse des Kapitalverbrauchs. D Betriebswirtschaft in Forschung und Praxis vol. 2. Wiesbaden: Betriebswirtschaftlich Verlag Gabler, 1966. Pp. 91. DM 11.50.

HIRSCH, H. Alfred Marshalls Beitrag zur modernen Theorie der Unternehmung. Sozialwi senschaftliche Abhandlungen no. 10. Berlin: Duncker & Humblot, 1965. Pp. 228. DM 3

INAGAKI, M. Efficient, inefficient and critical growth. Pub. no. 33. Rotterdam: Netherlan Econ. Inst., 1966. Pp. 16.

KHALDOÛN, I. Les textes sociologiques et économiques de la Mouqaddima, 1375-1379. Class fied, transl. and annotated by G-H. Bousquet. Paris: Ed. Marcel Rivière, 1965. Pp. 18 F 16.

MATHUR, G. Planning for steady growth. New York: Augustus M. Kelley, 1965. Pp. xi 386. $12.50.

MUELLER, M. B., ed. Readings in macroeconomics. New York: Holt, Rinehart and Winsto 1966. Pp. xiii, 402. Paper, $5.95.

POPESCU, O. Ensayos de doctrinas económicas argentinas. 2nd ed. Buenos Aires: Ed. Depalm 1965. Pp. 153.

SHACKLE, G. L. S. The nature of economic thought—selected papers 1955-1964. New Yor and Cambridge: Cambridge Univ. Press, 1966. Pp. xiv, 322. $9.50.

SHAPIRO, E. Macroeconomic analysis. New York: Harcourt, Brace & World, 1966. Pp. x 618. $8.95.

STIGLER, G. J. The theory of price. 3rd ed. New York: Macmillan; London: Collier-Ma millan, 1966. Pp. viii, 355. $6.95.

STIGLITZ, J. E., ed. The collected scientific papers of Paul A. Samuelson. 2 vols. Cambridg Mass. and London: M.I.T. Press, 1966. Pp. xi, 771; xi, 1039. $12.50 each.

WEIZSÄCKER, C. C. Zur ökonomischen Theorie des technischen Fortschritts. Göttingen Vandenhoeck & Ruprecht, 1966. Pp. 104. DM 13.

ZANNETOS, Z. S. The theory of oil tankship rates—an economic analysis of tankship opera tions. Cambridge: M.I.T. Press, 1966. Pp. xv, 336. $10.

Economic History; Economic Development; National Economies

ABOYADE, O. Foundations of an African economy. New York and London: Praeger, 1966. Pp. xvi, 366. $15.

ALLEN, J. B. The company town in the American West. Norman: Univ. of Oklahoma Press, 1966. Pp. xvii, 205. $5.95.

BALDWIN, R. E. Economic development and growth. New York and London: Wiley, 1966. Pp. xi, 133. $1.95.

BASS, L. W. The management of technical programs—with special reference to the needs of developing countries. New York and London: Praeger, 1966. Pp. 154. $8.50.

BEHRENS, W. E. Die Bedeutung staatlicher Entwicklungsplanung für die wirtschaftliche Entwicklung unterentwickelter Länder. Volkswirtschaftliche Schriften no. 94. Berlin: Duncker & Humblot, 1966. Pp. 165. DM 26.60.

BHAGWATI, J. The economics of underdeveloped countries. New York: McGraw-Hill, 1966. Pp. 254. $4.95; paper, $2.45.

BIRMINGHAM, W. AND FORD, A. G., eds. Planning and growth in rich and poor countries. New York and London: Praeger, 1966. Pp. 267. $7.50.

BOLINO, A. C. The development of the American economy. 2nd ed. Columbus, Ohio: Merrill Books, 1966. Pp. xvi, 636. $8.75. First ed. reviewed in 1961 by R. B. Sheridan.

BOXER, A. H., ed. Aspects of the Australian economy. New York and London: Melbourne Univ. Press, 1966. Pp. 107. $3.50; paper, $1.75.

CARACCIOLO, A. Le port franc d'Ancone—croissance et impasse d'un milieu marchand au XVIII^e siècle, no. 19. Paris: S.E.V.P.E.N., 1965. Pp. 306.

CHAMBERS, J. D. AND MINGAY, G. E. The agricultural revolution—1750-1880. London: B. T. Batsford Ltd., 1966. Pp. ix, 222. $9.50.

CHANDRASEKHAR, S. American aid and India's economic development, 1951-64. New York and London: Praeger, 1966. Pp. 225. $7.

CIPOLLA, C. M. Guns, sails and empires: technological innovation and the early phases of European expansion 1400-1700. New York: Pantheon Books, 1965. Pp. 192. $5.

CLARK, R., WITH SOPER, T. AND WILLIAM, P. Aid in Uganda—programmes and policies. London: Overseas Develop. Inst., 1966. Pp. 101. 15s.

COBEN, S. AND HILL, F. G., eds. American economic history—essays in interpretation. Philadelphia: Lippincott, 1966. Pp. xi, 618. Paper, $3.95.

CURRIE, L. Accelerating development: the necessity and the means. New York and London: McGraw-Hill, 1966. Pp. xiv, 255. $7.50.

DAUMARD, A. Maisons de Paris et propriétaires parisiens au XIX^e siècle, 1809-1880. [Paris:] Ed. Cujas, 1965. Pp. 284.

DEMARIA, G. Eclissi dell'economia borghese. Padua: CEDAM, 1965. Pp. xiv, 640.

DIAMOND, S. The reputation of the American businessman. New York: Harper & Row, 1966. Pp. 209. Paper, $1.45. First published by Harvard Univ. Press, 1955.

EHRMAN, L. Opportunities for investment in Chile—a program for encouragement of private industry. New York and London: Praeger, 1966. Pp. xxix, 169. $12.50.

ESTALL, R. C. New England—a study in industrial adjustment. New York: Praeger, 1966. Pp. xv, 296. $7.50.

FELDMAN, G. D. Army industry and labor in Germany 1914-1918. New Jersey: Princeton Univ. Press, 1966. Pp. xvi, 572. $12.50.

FREYSSINET, J. Le concept de sous-développement. Sér. écon. du développement, no. 1. Paris: Mouton & Cie, 1966. Pp. xv, 368.

FRIEDMANN, J. Regional development policy: a case study of Venezuela. Cambridge, Mass. and London: M.I.T. Press, 1966. Pp. xvi, 279. $8.50.

HAZARD, J. L. Michigan's commerce and commercial policy study. East Lansing: Inst. Inter-

nat. Bus. Management Stud., Grad. School Bus. Admin., Michigan State Univ., 1965. P xx, 200. $7.50.

HENDERSON, P. D., ed. Economic growth in Britain. London: Weidenfeld & Nicholson, 196 Pp. 296.

HERRICK, B. H. Urban migration and economic development in Chile. Monogrs. in eco no. 6. Cambridge, Mass. and London: M.I.T. Press, 1965. Pp. x, 126. $5.

KILBY, P. African enterprise: the Nigerian bread industry. Hoover Inst. stud. no. 8. Sta ford: Hoover Inst. on War, Revolution, and Peace, Stanford Univ., 1965. Pp. x, 11 $3.50; paper, $2.50.

KIRDAR, U. The structure of United Nations economic-aid to underdeveloped countries. Th Hague: M. Nijhoff, 1966. Pp. 361.

KNIGHT, O., ed. I protest—selected disquisitions of E. W. Scripps. Madison and Londo Univ. of Wisconsin Press, 1966. Pp. xv, 799. $12.50.

LANDES, D. S., ed. The rise of capitalism. New York: Macmillan; London: Collier-Mac millan, 1966. Pp. vii, 150.

LEWIS, W. A. Development planning—the essentials of economic policy. New York: Harpe & Row, 1966. Pp. 278. $4.95.

LÜTGE, F. Deutsche Sozial- und Wirtschaftsgeschichte. Enzyklopädie der Rechts- un Staatswissenschaft. New York and Berlin: Springer-Verlag, 1966. Pp. xviii, 644. DM 59.

MCLAUGHLIN, R. U. Foreign investment and development in Liberia. New York and Lon don: Praeger, 1966. Pp. xiii, 217. $15.

MEIER, R. L. Science and economic development: new patterns of living. 2nd ed. Cam bridge, Mass. and London: M.I.T. Press, 1966. Pp. xxi, 273. Paper, $2.45. First pub. i 1956 and reviewed in 1957 by P. A. Baran.

MONTGOMERY, J. D. AND SIFFIN, W. J., eds. Approaches to development—politics, admin istration and change. New York and London: McGraw-Hill, 1966. Pp. xiii, 299. $8.50.

MOYES, A. Volunteers in development. London: The Overseas Develop. Inst., 1966. Pp. 141 8s. 6d.

MUSCAT, R. J. Development strategy in Thailand—a study of economic growth. New Yor and London: Praeger, 1966. Pp. xvi, 310. $15.

NIDER, J. On the contracts of merchants. Transl. from *De Contractibus Mercatorum* b C. H. Reeves. Norman: Univ. of Oklahoma Press, 1966. Pp. xiii, 77. $3.75.

NORTH, D. C. Growth and welfare in the American past—a new economic history. Engle wood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xiv, 198. $5.95; paper, $2.95.

RAO, Y. V. L. Communication and development—a study of two Indian villages. Minne apolis: Univ. of Minnesota Press, 1966. Pp. 145. $4.50.

REINING, C. C. The Zande scheme—an anthropological case study of economic developmen in Africa. Northwestern Univ. African stud. no. 17. Evanston: Northwestern Univ. Press 1966. Pp. xxi, 255. $7.50.

RIST, L. Die Weltbank vor einigen neuen Aspekten der Entwicklungsprobleme. (Printed as a manuscript.) Kieler Vorträge. N.F. 39. Kiel: Universität, Inst. für Weltwirtschaft 1965. Pp. 19. DM 2.50.

ROBOCK, S. H. AND SOLOMON, L. M., eds. International development 1965. Dobbs Ferry N.Y.: Oceana, 1966. Pp. viii, 197. $6.

SCIGLIANO, R. AND FOX, G. H. Technical assistance in Vietnam—the Michigan State University experience. New York and London: Praeger, 1966. Pp. 92. $8.50.

SUPPLE, B. E., ed. The experience of economic growth—case studies in economic history. New York: Random House, 1963. Pp. x, 458, xi. Paper, $3.75.

TURIN, L. Combat pour le développement. Paris: Ed. Ouvrières, 1965. Pp. 311. F 18.30.

UHLIG, C. Das Problem der "Social Costs" in der Entwicklungspolitik. Ökonomische Studien, vol. 11. Stuttgart: Gustav Fischer; New York: Intercontinental Medical Book Corp., distrib., 1966. Pp. vii, 178. $7.

VELLA, G. F. Storia ed economia nella questione meridionale italiana. Milan: Dott. A. Giuffrè, 1966. Pp. 468. L. 4000.

WHEELWRIGHT, E. L. Industrialization in Malaysia. New York: Cambridge Univ. Press; Melbourne: Melbourne Univ. Press, 1966. Pp. xvi, 153. $8.50.

WISH, J. R. Economics development in Latin America—an annotated bibliography. New York and London: Praeger, 1965. Pp. xvi, 144. $8.

WOODMAN, H. D., ed. and comp. Slavery and the Southern economy—sources and readings. New York: Harcourt, Brace & World, 1966. Pp. x, 261. Paper, $3.95.

YAKEMTCHOUK, R. Assistance économique et pénétration industrielle des pays de l'Est en Afrique. Coll. Etudes Econ., no. 2. Leopoldville, Congo: IRES, Univ. Lovanium, 1966. Pp. 104.

Asian Development Bank act. Hearings. 89th Congress, 2nd session, Jan. 26-Feb. 2, 1966. House Com. on Banking and Currency. Subcom. on Internat. Fin. Washington: Supt. Docs., 1966. Pp. 157.

Economic and social development plans—centrally-planned economies, developed market economies. *Dag Hammarskjold Library.* New York: United Nations, 1966. Pp. 59.

15 years and 150,000 skills—an anniversary review of the United Nations Expanded Programme of Technical Assistance. U.N. pub. sales no. 65:1.18. New York: United Nations, 1965 Pp. 139.

New Mexico's economy in 1965. Annual summary issue of *New Mexico Business.* Albuquerque: Bur. of Bus. Res., Univ. of New Mexico, 1966. Pp. 74. $1.

Output, employment, and productivity in the United States after 1800. Vol. 30, Studies in income and wealth, Conference on Research in Income and Wealth. New York: Nat. Bur. Econ. Research; Columbia Univ. Press, distrib., 1966. Pp. xiv, 660. $12.50.

Statistical Methods; Econometrics; Social Accounting

BISHOP, W. D. Kentucky county income, 1963—with comparative data, 1956-63. Lexington: Bur. Bus. Research, College of Commerce, Univ. of Kentucky, 1965. Pp. vii, 31. Paper, single copies free.

FISHER, F. M. The identification problem in econometrics. New York: McGraw-Hill, 1966. Pp. xi, 203. $8.95.

FOURASTIÉ, J. Les formules d'indices de prix—calculs numériques et commentaires théoriques. Paris: Librairie Armand Colin, 1966. Pp. x, 197. F 9.

FRISCH, R., WITH NATAF, A Maxima and minima—theory and economic applications. Chicago: Rand McNally; Dordrecht: D. Reidel, 1966. Pp. xii, 176. $8.50.

KAPLAN, L. J. Elementary statistics for economics and business. New York, Toronto and London: Pitman, 1966. Pp xv, 297. $7.50.

LEONTIEF, W. Input-output economics. New York: Oxford Univ. Press, 1966. Pp. ix, 257, $8.50.

MALLABER, K. A., ed. Conference on librarian-statistician relations in the field of economic statistics. Papers and proceedings of a conference sponsored by the Library Assn. and the Royal Stat. Soc., July 5, 1965. London: Library Assn., 1966. Pp. 138. 40s.; 30s. to members of Library Assn.

SPROWLS, R. C. Computers—a programming problem approach. New York and London: Harper & Row, 1966 Pp. x, 388. $8.50.

SZCZEPANIK, E. F. National accounting as a tool of economic planning. Inst. Soc. Stud., ser. minor, vol. 6. The Hague, London and Paris: Mouton & Co., 1965. Pp. 61. *f* 6.

VIET, J. Input-output— essai de présentation documentaire du système de W. Leontief. Pub. sér. D, Méthodes et Techniques no. 2. Paris: Mouton, 1966. Pp. 143.

WOLD, H. O. A., ed. Bibliography on time series and stochastic processes—an international team project. Cambridge: M.I.T. Press, 1966. Pp. xv, 516. $18.

Cahiers du séminaire d'économétrie. No. 8, Les programmes d'expansion. Paris: Centre Nat. Recherche Sci. Pp. 100. F 25.

Contributions by M. Verhulst, G. Faiveley, R. Froment, J. Zighera, J. Paelinck, and E. Malinvaud.

Problems of input-output tables and analysis. Stud. in methods ser. F, no. 14. New York: United Nations, 1966. Pp. 157.

Statistical yearbook 1965. 17th issue. U.N. Dept. Econ. and Soc. Affairs, sales no. 66.SVII.1. In English and French. New York: Pub. Svce., U.N., 1966. Pp. 747. $15; paper, $11.

Study week on the econometric approach to development planning, October 7-13, 1963. Pontificia Acad. Sci. Acripta Varia, pub. no. 28. Chicago: Rand McNally; Amsterdam: North-Holland Pub. Co., 1965. Pp. xxxviii, 1259.

Economic Systems; Planning and Reform; Cooperation

AKZIN, B. AND DROR, Y. Israel—high-pressure planning. Nat. planning ser. no. 5. New York: Syracuse Univ. Press, 1966. Pp. xxxii, 90. Paper, $2.95.

BARAN, P. A. AND SWEEZY, P. M. Monopoly capital—an essay on the American economic and social order. New York: Monthly Review Press, 1966. Pp. ix, 402. $8.75.

BRECH, R. Planning prosperity—a synoptic model for growth. New York: Humanities Press, 1966. Pp. viii, 189. $5. First pub. in 1964 by Darton, Longman & Todd, Ltd.

BRONSTEIN, M. AND FUSFELD, D. R., eds. The Soviet economy—a book of readings. Rev. ed. Homewood, Ill.: Irwin, 1966. Pp. ix, 389. Paper, $4.95.

CAMPBELL, R. W. Soviet economic power—its organization, growth, and challenge. 2nd ed. Boston: Houghton Mifflin, 1966. Pp. xii, 184. Paper, $2.75. First pub. in 1960.

EATON, J. Political economy—a Marxist textbook. New York: International, 1966. Pp. 253. $5; paper, $1.95. First pub. in 1963 by John Eaton.

HAGEN, E. E. AND WHITE, S. F. T. Great Britain—quiet revolution in planning. Nat. planning ser. no. 6. New York. Syracuse Univ. Press, 1966. Pp. xxv, 180. Paper, $3.95.

HANSON, A. H. The process of planning—a study of India's five-year plans, 1950-1964. New York and London: Oxford Univ. Press, for Royal Inst. Internat. Affairs, 1966. Pp. xiii, 560. $16.80.

HEIDE, H. Die langfristige Wirtschaftsplanung in Schweden. Forschungsberichte des Instituts für Weltwirtschaft an der Univ. Kiel no. 73. Tübingen: J.C.B. Mohr (Paul Siebeck), 1965. Pp. viii, 147. DM 28.

KLEIN, S. The road divides—economic aspects of the Sino-Soviet dispute. Hong Kong: Internat. Studies Group, 1966. Pp. ix, 178. $7.50.

The author discusses the Soviet Union's aid to and trade with China during the 1950's, the reasons for the "break" in 1960, and its economic consequences for China. He attributes an unusually large part of China's development to Soviet aid, reviews a long history of Sino-Soviet conflict and expects the current estrangement to continue and even intensify, and thinks that China is still in the "Great Slide Backward." Numerous photographs and typographical errors.

KRONROD, Y. A. Zakoni Politicheskoi Zkonomii Sotasializma (Laws of socialist political economy). Moscow: "Muisl" Press, 1966. Pp. 580. Rbl. 2.25.

LAPALOMBARA, J. Italy—the politics of planning. Nat. planning ser. no. 7. New York: Syracuse Univ. Press, 1966. Pp. xvii, 184. Paper, $3.95.

LECHT, L. A. Goals, priorities, and dollars—the next decade. New York: Free Press; London: Collier-Macmillan, 1966. Pp. xiv, 365. $6.95; paper, $2.95.

MELNYK, Z. L. Soviet capital formation: Ukraine, 1928/29-1932. Munich: Ukrainian Free Univ. Press, 1965. Pp. xxvi, 182.

This book is "one of the first Western studies in the field of regional economics of the Soviet Union." The author finds that "in the course of the first Five-year plan period (1928/29-1932), five billion rubles in capital funds ($4.34 billion, at the current official rate of exchange), or almost thirty per cent of total revenue collected by the central USSR government in Ukraine, were withdrawn from Ukraine and spent elsewhere in the Soviet Union."

MICHEL, H. Zur Beeinflussung des Wachstumsprozesses einer Volkswirtschaft durch staatliche Massnahmen. Berlin: Vahlen, 1965. Pp. 109. DM 16.

NORDBOTTEN, S. Long-range planning, progress- and cost-reporting in the Central Bureau of Statistics of Norway. Artikler no. 16. Oslo: H. Aschehoug & Co., 1966. Pp. 6. NKr. 4.

POLLARD, S. D. How capitalism can succeed. Harrisburg, Pa.: The Stackpole Co., 1966. Pp. 254. $4.95.

SHAFER, R. J. Mexico—mutual adjustment planning. Nat. plan. ser. no. 4. New York: Syracuse Univ. Press, 1966. Pp. 214.

Business Fluctuations

BRECH, R. Britain 1984: a report prepared for Unilever—an experiment in the economic history of the future. New York: Humanities Press, 1966. Pp. xii, 124. $3. First pub. in 1963 by Darton, Longman & Todd, Ltd.

BURNS, A. F. The management of prosperity. 1965 Benjamin F. Fairless Memorial Lectures. New York: Carnegie Inst. of Technology; Columbia Univ. Press, distrib., 1966. Pp. 70. $3.50.

BUTLER, W. F. AND KAVESH, R. A., eds. How business economists forecast. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xiv, 540. $15.95.

PERRY, G. L. Unemployment, money wage rates, and inflation. M.I.T. monogr. in econ. no. 7. Cambridge, Mass. and London: M.I.T. Press, 1966. Pp. xv, 143. $6.

THALBERG, B. A trade cycle analysis—extensions of the Goodwin model. Lund econ. stud. no. 2. Lund: Studentlitteratur, 1966. Pp. 126.

THEIL, H. WITH BEERENS, G. A. C., DE LEEUW, C. G. AND TILANUS, C. B. Applied economic forecasting. Stud. in mathematical and managerial econ., vol. 4. Chicago: Rand; Amsterdam: North-Holland Pub. Co., 1966. Pp. xxv, 474. $14.

WOY, J. B. Business trends and forecasting—information sources. Management info. guide no. 9. Detroit: Gale Res. Co., 1965. Pp. 152. $8.75.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

BASU, S. K. Theory and practice of development banking—a study in the Asian context. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. viii, 235 $9.50.

BEARD, T. R. U.S. Treasury advance refunding June 1960-July 1964. Washington: Board of Governors, Fed. Res. System, *s.d.* Pp. 65. 50¢.

BENAVIE, A. Intermediaries and monetary policy. Mich. bus. reports no. 48. Ann Arbor: Univ. of Michigan, 1965. Pp. 93.

BOSMAN, H. W. J. Monetary and financial aspects of the problems of developing countries. Inst. Soc. Stud., ser. minor, vol. 10. The Hague, London and Paris: Mouton & Co., 1965. Pp. 45. *f* 5.

DEPRANO, M. E. Self-correcting problems in money, banking, and monetary theory. Boston: Allyn and Bacon, 1966. Pp. 104. $3.25. Tentative text list.

DE ROOVER, R. The rise and decline of the Medici Bank, 1397-1494. New York: Norton, 1966. Pp. xviii, 500. Paper, $2.95. Reviewed in 1964 by H. A. Miskimin.

GOLDFELD, S. M. Commercial bank behavior and economic activity—a structural study of monetary policy in the postwar United States. Contributions econ. analysis no. 43. Amsterdam: North-Holland, 1966. Pp. 212. $7.50.

HOLGUÍN, J. F. Evolución de las instituciones financieras en Colombia. Mexico: Centro de Estud. Mon. Latinoamericanos (CEMLA), 1966. Pp. 256.

JANOCHA, P. Intervention und Kooperation dur Zentralbanken auf den Devisenmärkten nach dem Zweiten Weltkrieg. Kieler stud. Inst. f. Weltwirtschaft, Univ. Kiel, no. 74. Tübingen: J.C.B. Mohr (Paul Siebeck), 1966. Pp. vii, 149. DM 32.

JOHRI, C. K. Monetary policy in a developing economy. A study of the policies of the

Reserve Bank of India and their effect on the operation of the banking system, 1956-65. Calcutta: World Press, 1965. Pp. xv, 283. Rs 20.

LANGER, W. Sicherung der Geldwertstabilität. Eine Aufgabe der Konjunktur-, Strukur- und Wachstumspolitik. Institut für Weltwirtschaft, Univ. Kiel, N.F. 44. Kiel: Universität Kiel, 1966. Pp. 20. DM 3.

LEPONIEMI, A. On the demand and supply of money: the evidence from the quarterly time series in the United States, the United Kingdom and Finland 1949-1962. Econ. stud. no. 27. Helsinki: Finnish Econ. Assn., 1966. Pp. 162.

MENA, A. O. Discurso pronunciado en la Septima Reunión de Gobernadores del Banco Interamericano de Desarrollo . . . México, 25-29 de abril de 1966. Mexico: Dirección de Prensa, Memoria, Bibliotecas y Publicaciones, 1966. Pp. 11.

———, Discurso pronunciado en la Sesión Inaugural de la XXXII Convención Nacional Bancaria . . . México, 24 de marzo de 1966. Mexico: Dirección Prensa, Memoria, de Bibliotecas y Publicaciones, 1966. Pp. 26.

MORELLE, W., HESSER, L. AND MELICHAR, E. Merchant and dealer credit in agriculture—data from the 1960 survey of agriculture. Washington: Board of Governors, Fed. Res. System, 1966. Pp. 70, A-40. $1.

MUSGRAVE, R. A. Finanztheorie. Tübingen: J. C. B. Mohr (Paul Siebeck), 1966. Pp. xx, 537. DM 53; paper, DM 48.

OLAKANPO, J. O. Central banking in the Commonwealth (a comparative study of monetary problems and techniques). Calcutta: Bookland Private Ltd., 1965. Pp. xvi, 331. 30s.

POOLE, W. A. Sources and uses of capital funds—a pilot study of New Zealand, 1955-56 to 1961-62. Tech. memo. no. 10. Wellington: New Zealand Inst. Econ. Research, 1965. Pp. 152.

RAMOS, A. M. Q. A history of money and banking in Argentina . . . a thesis. Rio Piedras, Puerto Rico: Univ. of Puerto Rico, 1965. Pp. 346.

TILLY, R. Financial institutions and industrialization in the Rhineland—1815-1870. Madison and London: Univ. of Wisconsin Press, 1966. Pp. viii, 197. $6.50.

This book has a dual purpose. "First, it employs the historical experience of an important industrializing area—the Rhineland province of Prussia—to illuminate the role of financial institutions in the process of economic development. Second, its examination of government monetary and banking policies provides new information (on) the role of the state in Prussian industrial development."

SAUSE, G. G. Money, banking, and economic activity. Boston: D. C. Heath, 1966. Pp. xiv, 507. $8.75.

TURNER, W. E. Stable money—a conservative answer to business cycles. Austin, Tex.: W. E. Turner; Texas Mailing & Printing Co., 1966. Pp. xxxi, 547. $8.95.

WOLF, H. A. Monetary and fiscal policy. Columbus, Ohio: Merrill Books, 1966. Pp. xviii, 478. $9.25.

ZOLOTAS, X. Current monetary and economic developments in Greece. Papers and lectures no. 19. Athens: Bank of Greece, 1966 Pp. 25.

The Federal Reserve portfolio—statements by individual economists. Subcom. on econ. progress. Econ. Joint Com., 89th Cong., 2nd sess. Washington: Supt. Docs., 1966. Pp. 176

International banking. New York: American Inst. Banking, 1966. Pp. 246.

Recent Federal Reserve action and economic policy coordination. Hearings . . . Econ. Joint Com., 89th Congress, 1st sess., Dec. 13-16, 1965. Washington: Supt. Docs., 1966. 2 pts.

Public Finance; Fiscal Policy

ANTON, T. J. The politics of state expenditure in Illinois. Urbana and London: Univ. of Illinois Press, 1966. Pp. viii, 286. $6.75.

BELLSTEDT, C. Die Steuer als Instrument der Politik—Eine vergleichende Untersuchung der Steuerpolitik in den USA und Deutschland. Berlin: Duncker & Humblot, 1966. Pp. 469. DM 68.

BOYLE, L. Equalisation and the future of local government finance. Edinburgh and London: Oliver & Boyd, 1966. Pp. xix, 142. 30s.

COLM, G. AND WAGNER, P. Federal budget projections. A report of the Nat. Planning Assn. and the Brookings Inst. Washington: Brookings Inst., 1966. Pp. xx, 194. Paper, $2.50.

CONLON, C. F. and others. State and local taxes on business. Symposium conducted by Tax Inst. Amer., Oct. 28-30, 1964. New Jersey: Princeton, 1965. Pp. 352.

DAL PANE, L. La finanza Toscana dagli inizi del secolo XVIII alla caduta del Granducato. Milan: Banca Commerciale Italiana, 1965. Pp. 759.

GANDHI, V. P. Tax burden on Indian agriculture. Cambridge: Law School, Harvard Univ. 1966. Pp. xix, 240.

HEILBRUN, J. Real estate taxes and urban housing. New York and London: Columbia Univ. Press, 1966. Pp. 195. $8.50.

HINRICHS, H. H. A general theory of tax structure change during economic development. Cambridge: Law School, Harvard Univ., 1966. Pp. xvi, 154.

ITO, H. Studies in public finance and taxation. Econ. ser. no. 38. Tokyo: Sci Council of Japan, Div. Econ., Commerce & Bus. Admin., 1966. Pp. 113.

KLEINSORGE, P. L., ed. Public finance and welfare—essays in honor of C. Ward Macy. Eugene: Univ. of Oregon Books, 1966. Pp. vii, 305. $7.50.

LYNCH, D. W. The development of state and local debt in Kentucky: 1890-1962. Lexington: Univ. of Kentucky, College of Commerce, 1966. Pp. xi, 209. Single copies free upon request while supply lasts.

MARSHALL, A. W. Cost/benefit analysis in health. Santa Monica, Calif.: Rand, 1965. Pp. 18.

NORMANTON, E. L. The accountability and audit of governments—a comparative study. New York: Praeger; Manchester: Univ. of Manchester Press, 1966. Pp. xx, 452. $11.

PARETO, V. Manuel d'économie politique. Travaux de droit, d'économie, de sociologie et de sciences politiques no. 45. Geneva: Librairie Droz, 1966. Pp. 695.

PETERS, G. H. Cost-benefit analysis and public expenditure. London: Inst. Econ. Affairs, 1966. Pp. 44. 7s. 6d.

POPHALE, G. L. A quarter century of direct taxation in India 1939-1964—a historical-cum-critical survey. Bombay: IMC Econ. Research and Training Foundation, 1966. Pp. xliv, 475. $5.

PORTER, R. J. Factors influencing the cost of state and local borrowing in Kentucky. Lexington: Bur. Bus. Research, Univ. of Kentucky, 1965. Pp. x, 196. Single copies free.

SABINE, B. E. V. A history of income tax. New York: Humanities Press, 1966. Pp. 288. $6.75.

SCHERER, J. AND PAPKE, J. A., eds. Public finance and fiscal policy—selected readings. Boston: Houghton Mifflin, 1966. Pp. xii, 590. Paper, $5.25.

SCHNITTGER, L. Besteuerung und wirtschaftliche Entwicklung in Ostafrika. Afrika-Studien no. 8. Berlin and New York: Springer, 1966. Pp. xv, 193. DM 31.

WEIDENBAUM, M. L. The allocation of government funds. Dept. Econ. working paper. St. Louis: Washington Univ. Press, [1966]. Pp. 20.

WILLIAMS, A. H. Tax policy—can surveys help? London: Political and Econ. Planning, 1966. Pp. 40.

WOLOZIN, H., ed. The economics of air pollution—a symposium. New York: Norton, 1966. Pp. 318. $5; paper, $2.95.

A better balance in federal taxes on business. New York: Com. for Econ. Develop., 1966. Pp. 37. Paper, 75¢.

Etude comparative des dépenses et des recettes des pouvoirs publics des pays du Benelux, 1961-1965. Brussels: L'Union Econ. Benelux, 1965. Pp. 90. Bfr. 140.

Financing state government. St. Paul: Minnesota Legislative Res. Com., 1966. Pp. 11.

Fiscal policy for economic growth in Latin America—papers and proceedings of a confer-

ence held in Santiago, Chile, December 1962. Baltimore: Johns Hopkins Press, for T Joint Tax Program, 1965. Pp. x, 462. $12.

Foreign tax policies and economic growth—a conference report of the National Bureau Economic Research and the Brookings Inst. New York and London: Nat. Bur. Ecc Research; Columbia Univ. Press, distrib., 1966. Pp. xii, 482. $10; paper, $3.50.

Progressiviteten i skattesystemet, 1960. (Statistical tax incidence investigation, 1960). Wi English summary. Samfunnsøkonomiske stud. no. 15. Oslo: Central Bur. of Stat., 196 Pp. 95. NKr. 7.

Statistical yearbook of the Tel-Aviv-Yafo municipality 1965. Tel-Aviv: Dept. of Research Stat., Municipal Corp. of Tel-Aviv-Yafo, 1965. Pp. xlix, 435.

Statistics of income—1962. Corporation income tax returns with accounting periods end July 1962-June 1963. Pub. no. 16(3-66). Washington: Supt. Docs, 1966. Pp. v, 42 Paper, $2.25.

Statistics of income . . . 1963: preliminary corporation income tax returns with accounti periods ended July 1963-June 1964. Treasury Dept., Internal Revenue Svce., pub. n 159(4-66). Washington: Supt. Does., 1966. Pp. 38 30¢.

A study of tax burden by income class in the Philippines. Conducted by the Joint Leg Exec. Tax Comm. with the Office of Stat. Coordination and Standards, Nat. Econ. Counc Manila: Philippines Joint Leg.-Exec. Tax Comm., 1964. Pp. 122.

Taxation of foreign income—by United States and other countries. Symposium conduct by Tax Inst. Amer., Dec. 2-3, 1965. Princeton: Tax Inst. Amer., 1966. Pp. xii, 340. $12.5 Contributions by various authors.

International Economics

Avramovic, D. and others. Economic growth and external debt. Baltimore: Johns Hopkir Press, 1966. Pp. xiii, 207. $6. First pub. in 1964.

Byé, M. Relations économiques internationales. 2nd ed. Paris: Dalloz, 1965. Pp. 92 F 42.80.

Collier, D. S. and Glaser, K., eds. Western integration and the future of Eastern Europ Foundation for Foreign Affairs ser. no. 9. Chicago: Henry Regnery Co., 1964. Pp. xii 207. $6.

Coppock, J. O. Atlantic agricultural unity: is it possible? New York, Toronto and London McGraw-Hill, 1966. Pp. xvi, 238. $7.50.

Curzon, G. Multilateral commercial diplomacy: the General Agreement on Tariffs an Trade and its impact on national commercial policies and techniques. New York: Praege 1965. Pp. xii, 367. $12.50.

Dell, S. A Latin American common market? New York and London: Oxford Univ. Pres for Royal Inst. of Internat. Affairs, 1966. Pp. ix, 336. $8.80.

Díaz Alejandro, C. F. Exchange-rate devaluation in a semi-industrialized country—th experience of Argentina 1955-1961. Monogrs. in econ. no. 5. Cambridge, Mass. and London: M.I.T. Press, 1965. Pp. xiv, 210. $7.50.

Edwards, C. D. Trade regulations overseas—the national laws. Dobbs Ferry, N.Y.: Oceana 1966. Pp. xvi, 752. $25.

Ford, J. L. The Ohlin-Heckscher theory of the basis and effects of commodity trade. Asi monogr. no. 6. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. viii, 88. $5.75

Galbraith, V. L. World trade in transition. Washington, D.C.: Pub. Affairs Press, 1965 Pp. vi, 104. Paper, $3.25.

Gilbert, M. Problems of the international monetary system. Essays in internat. fin., no. 53 Princeton: Internat. Fin. Sec., Princeton Univ., 1966. Pp. 25. Single copies free.

Hamilton, W. B., Robinson, K. and Goodwin, C. D. W., eds. A decade of the Commonwealth, 1955-1964. Duke Univ. Comonwealth-Stud. Center pub. no. 25. Durham: Duke Univ. Press, 1966. Pp. xx, 567. $12.50.

HELLWIG, F. Die politische Tragweite der europäischen Wirtschaftsintegration. Kieler Vorträge, 45. Kiel: Inst. für Weltwirtschaft an der Universität Kiel, 1966. [DM 3.20.]

INGRAM, J. C. International economic problems. New York, London and Sydney: Wiley, 1966. Pp. x, 180. $1.95.

JENSEN, F. B. AND WALTER, I., eds. Readings in international economic relations. New York: Ronald, 1966. Pp. ix, 528. $6.50.

KINDLEBERGER, C. P. Europe and the dollar. Cambridge, Mass. and London: M.I.T. Press, 1966. Pp. x, 297. $7.50.

———. International short-term capital movements. New York: A. M. Kelley, 1965. Pp. 262.

LAMBRINIDIS, J. S. The structure, function, and law of a free trade area. New York and London: Praeger, 1966. Pp. 308. $12.50.

LIEFTINCK, P. External debt and debt-bearing capacity of developing countries. Essays in internat. fin., no. 51. Princeton: Internat. Fin. Sec., Dept. of Econ., Princeton Univ., 1966. Pp. 28.

LINNEMANN, H. An econometric study of international trade flows. Amsterdam: North-Holland, 1966. Pp. xiii, 234. $7.80.

LIPFERT, H. Einführung in die Währungspolitik. (2., neubearbeitete Aufl.) Beck'sche Schwarze Reihe vol. 26. Munich: Beck, 1965. Pp. xi, 287. DM 11.80.

LOVASY, G. Las propuestas de financiamiento compensatorio—examen y evaluación de los diversos planes. Mexico: Centro de Estud. Mon. Latinoamericanos (CEMLA), 1966. Pp. 69.

MACHLUP, F. International monetary systems and the free market economy. Reprints in internat. fin. no. 3. Princeton: Internat. Fin. Sec., Dept. of Econ., Princeton Univ., 1966. Pp 1-22.

———. Involuntary foreign lending. Stockholm: Almqvist & Wiksell, 1965. Pp. 129.

———. Real adjustment, compensatory correction, and financing of imbalances in international payments. Reprints in internat. fin. no. 2. Princeton: Internat. Fin. Sec., Dept. of Econ., Princeton Univ., 1965. Pp. 185-213.

MCKINNON, R. I. AND OATES, W. E. The implications of international economic integration for monetary, fiscal, and exchange-rate policy. Stud. in internat. fin. no. 16. Princeton: Internat. Fin. Sec., Princeton Univ., 1966. Pp. 35. $1.

MIKESELL, R. F. Public international lending for development. New York: Random House, 1966. Pp. x, 244. $4.95; paper, $2.45.

"The purpose of this book is to explore the major policies and operational problems of public international lending agencies engaged in promoting social and economic progress in the developing countries." The author looks at international lending before World War II, public lending after, discusses each of the major lending agencies, and then, in a long chapter, takes up some special problems of public lending and economic development.

———. Public foreign capital for private enterprise in developing countries. Essays in internat. fin. no. 52 Princeton: Internat. Fin. Sec., Princeton Univ., 1966. Pp. 29. Single copies free.

POLK, J., MEISTER, I. W. AND VEIT, L. A. U.S. production abroad and the balance of payments: a survey of corporate investment experience. New York: Nat. Indus. Conf. Board, 1966 Pp. 200. Paper, $17.50. NICB Assocs. and Govt. agencies, $3.50. Students and teachers, $1 (Mailed only to school address.)

This report "is based on an intensive examination of the corporate balance of payments of a hundred of this nation's leading manufacturing enterprises. Fully half of these were explored in detail through lengthy interviews with the chief executives responsible for international finance. The report presents a pioneer study of company accounts from the standpoint of their foreign exchange implications and at the same time an examination of the national balance of payments in the light of the investment activities of these companies."

ROBERTSON, D. J. AND HUNTER, L. C., eds. The British balance of payments. Edinburgh: Oliver & Boyd, 1966. Pp. vii, 187. 30s.

Contributions by various authors.

ROLFE, S. E. Gold and world power. New York: Harper & Row, 1966. Pp. viii, 276. $6.95.

SACHS, I. Foreign trade and economic development of underdeveloped countries. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. viii, 136. $6.50.

SPICER, K. A. Samaritan state? . . . external aid in Canada's foreign policy. Toronto: Univ. of Toronto Press, 1966. Pp. xiv, 272. $7.50.

STRACKBEIN, O. R. American enterprise and foreign trade. Washington, D.C.: Public Affairs Press, 1965. Pp. iv, 193. Paper, $2.50.

TRIFFIN, R. The world money maze—national currencies in international payments. New Haven and London: Yale Univ. Press, 1966. Pp. xii, 585. $12.50.

UREN, P. E., ed. East-West trade—a symposium. Toronto: Canadian Inst. Internat. Affairs, 1966. Pp. 181. $3; paper, $2.

VERNON, R., ed. How Latin America views the U.S. investor. New York and London: Praeger, in coop. with Harvard Univ. Grad. School of Bus. Admin., 1966. Pp. x, 117. $10.

WALSH, A. E. AND PAXTON, J. Trade in the common market countries. London: A. E. Walsh & Partners Ltd., 1965. Pp. 111. £10.10.

WELLS, S. Trade policies for Britain—a study in alternatives. New York and London: Oxford Univ. Press, 1966. Pp. x, 137. Paper, $2.

WIONCZEK, M. S., ed. Latin American economic integration—experiences and prospects. New York and London: Praeger, 1966. Pp. xiii, 310. $7.50.

WOOLLEY, H. B. Measuring transactions between world areas. New York: Nat. Bur. Econ. Res.; Columbia Univ. Press, distrib., 1966. Pp. xviii, 157. $7.50.

Classification of commodities by industrial origin—relationship of the standard international trade classification to the international standard industrial classification. New York: United Nations Stat. Office, 1966. Pp. 72.

Los instrumentos del Mercado Común Centroamericano. Ser. econ. and fin., no. 415. San José, Costa Rica: Escuela Superior de Admin. Pública, América Central, 1965. Pp. viii, 142. $1.50.

The market economy in Western European integration. 7th Flemish Econ. Cong., Louvain, 8 and 9 May, 1965. Louvain: Neuwelaerts, 1965. Pp. xiii, 417. Bfr. 500.

Los problemas monetarios internacionales y los paises en desarrollo. Informe del Grupo de Expertos (UNCTAD). Mexico: Centro de Estud. Mon. Latinoamericanos (CEMLA), 1966. Pp. 74.

Proposed economic assistance programs, FY 1967. Summary presentation to the Congress. Agency for Internat. Develop. Washington: Supt. Docs., 1966. Pp. vii, 239. 75¢.

Business Finance; Investment and Security Markets; Insurance

ARCHER, S. H. AND D'AMBROSIO, W. F. WITH SHARPE, W. F. Business finance: theory and management. New York: Macmillan, 1966. Pp. xvi, 569. $8.50.

BIERMAN, H., JR. AND SMIDT, S. The capital budgeting decision—economic analysis and financing of investment projects. 2nd ed. New York: Macmillan; London: Collier-Macmillan, 1966. Pp. xii, 420. $6.95. First pub. in 1960.

BOCK, B. AND FARKAS, J. Balance sheet on acquisitions and disposals. Stud. in bus. econ. no. 92. New York: Nat. Indus. Conf. Board, 1966. Pp. 30. $1; $5, non-associates.

FREUND, W. C. Investment fundamentals. New York: American Bankers Assn., 1966. Pp. 152.

LERNER, E. M. AND CARLETON, W. T. A theory of financial analysis. New York: Harcourt, Brace & World, 1966. Pp. xii, 281. $7.95.

MAYER, R. R. Financial analysis of investment alternatives. Boston: Allyn and Bacon, 1966. Pp. 93. Paper, $2.75.

ROBBINS, S. The securities markets—operations and issues. New York: Free Press; London: Collier-Macmillan, 1966. Pp. xvi, 303. $7.95.

WESTON, J. F., ed. Financial management in the 1960s: new challenges and responsibilities. Readings from *Fortune*. New York: Rinehart and Winston, 1966. Pp. 303.

WOLF, H. A. AND RICHARDSON, L., eds. Readings in finance. New York: Appleton-Century-

Business Organization; Managerial Economics; Marketing; Accounting

BARKSDALE, H. C. AND WEILBACHER, W. M., eds. Marketing research—selected readings with analytical commentaries. New York: Ronald, 1966. Pp. ix, 696. $7.50.

BELL, M. L. Marketing concepts and strategy. Boston: Houghton Mifflin, 1966. Pp. xii, 686. $8.75.

BLAIR, T. L. V. Africa: a market profile. New York and London: Praeger, 1966. Pp. 272. $12.50.

BLOCH-LAINÉ, F. AND PERROUX, F. L'entreprise et l'économie du xx^{e} siècle. Vol. 2, La formation des décisions et l'entreprise. Paris: Presses Univ. de France, 1966. Pp. 346. F 20.

CHAMBERS, R. J. Accounting, evaluation and economic behavior. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xii, 388. $7.50.

CONSTANTIN, J. A. Principles of logistics management—a functional analysis of physical distribution systems. New York: Appleton-Century-Crofts, 1966. Pp. xxii, 623. $9.50.

DALE, E. AND MICHELON, L. C. Modern management methods. Cleveland: World Pub. Co., 1966. Pp. x, 211. $4.95.

DAVIDSON, W. R. AND DOODY, A. F. Retailing management. 3rd ed. New York: Ronald, 1966. Pp. x, 905. $9.50. First pub. in 1951.

DAVIS, K. R. Marketing management. New York: Ronald, 1966. Pp. xii, 894. $9.

EDWARDS, R. S. AND TOWNSEND, H., eds. Business growth. New York: St. Martin's Press; London: Macmillan, 1966. Pp. xxiv, 410. $8.

GARRETT, T. M. Business ethics. New York: Appleton-Century-Crofts, 1966. Pp. xiii, 210. Paper, $2.50.

GENTRY, D. L. AND TAFF, C. A. Elements of business enterprise. 2nd ed. New York: Ronald, 1966. Pp. vi, 624. $8; workbook, $2.50. First pub. in 1961.

GREEN, P. E. AND TULL, D. S. Research for marketing decisions. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. viii, 532. $9.95.

GRÉGOIRE, R. Business management. New York: United Nations, 1966. Pp. 220. Paper, $2.50.

GRETHER, E. T. Marketing and public policy. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. 120. Paper, $1.75.

HAWKINS, H. C. G. Wholesale and retail trade in Tanganyika. New York and London: Praeger, 1966. Pp. 178. $17.50.

HAX, H. Die Koordination von Entscheidungen. Ein Beitrag zur betriebswirtschaftlichen Organisationslehre. Schriftenreihe Annales Univ. Saraviensis no. 17. Cologne: Heymann, 1965. Pp. 269. DM 22.

KISH, J. L., JR. AND MORRIS, J. Microfilm in business. New York: Ronald, 1966. Pp. viii, 163. $7.50.

KJAER-HANSEN, M., [ed.] Readings in Danish theory of marketing. Copenhagen: Einar Harcks; Amsterdam: North-Holland Pub. Co., 1966. Pp. 325.

LESOURNE, J. Du bon usage de l'étude économique dans l'entreprise. Paris: Dunod, 1966. Pp. viii, 173.

MANSFIELD, E., ed. Managerial economics and operations research—a nonmathematical introduction. New York: Norton, 1966. Pp. x, 244. $5.95.

MASLOW, A. H. Eupsychian management: a journal. Homewood, Ill.: Irwin, 1965. Pp. xvi, 277. Paper, $4.65.

MASON, E. S., ed The corporation in modern society. New York: Atheneum, 1966. Pp. xv, 335. Paper, $1.95. First pub. in 1959 by Harvard Univ. Press.

MEIER, R. C. AND NEWELL, W. T. Inventory simulation program ISP2, no. 1. Seattle: Univ. of Washington, Grad. School Bus. Admin., 1966. Pp. 33. 75¢.

WALLACH, F. Beförderung und Aufstieg in der Unternehmung. Winterthur: Verl. Hans Shellenberg, *s.d.* Pp. xx, 235. DM 24.

WASSON, C. R. Managerial economics—text and cases. New York: Appleton-Century-Crofts, 1966. Pp. vii, 472. $6.50.

ZIEGLER, R. J. Business policies and decision making. New York: Appleton-Century-Crofts, 1966. Pp. xi, 260. Paper, $2.95.

L'entreprise—ses techniques et son gouvernement. Pt. 4, La croissance de l'entreprise et le profit. *Econ. Appliquée,* July-Sept. 1965. Paris: Presses Univ. de France, 1965. Pp. 209. F 18.

Facts on Europe—economic and labor information for the U.S. businessman. New York: Indust. Relations Counselors, Inc., 1966. Pp. 118. $8.50.

Technology and the American economy. U.S. nat. comm. on technology, automation, and econ. progress. Washington: Supt. Docs., 1966. Pp. 115.

Industrial Organization; Government and Business; Industry Studies

BAIN, J. S. International differences in industrial structure—eight nations in the 1950s. Stud. in comparative econ., no. 6. New Haven and London: Yale Univ. Press, 1966. Pp. xiv, 209. $5; paper, $1.45.

BJERKHOLT, O. Økonomiske konsekvenser av nedrustning i Norge. (Economic consequences of disarmament in Norway.) With English summary. Article no. 17. Oslo: Central Bur. of Stat., 1966. Pp. 25. NKr. 4.

BLOCH-LAINÉ, F. AND PERROUX, F. L'entreprise et l'économie du xx^e siècle. Vol. 1, L'entreprise et son environnement. Paris: Presses Univ. de France, 1966. Pp. 328. F 18.

BOCK, B. AND FARKAS, J. Concentration in manufacturing—industry summaries. Stud. in bus. econ. no. 91. New York: Nat. Indus. Conf. Board, 1966. Pp. 81, $2.50; $12.50, non-associates.

BOLTON, R. E., ed. Defense purchases and regional growth. Washington, D.C.: Brookings Inst., 1966. Pp. xiv, 189. Paper, $2.50.

COLBY, C. C. North Atlantic arena—water transport in the world order. Carbondale: Southern Illinois Univ. Press, 1966. Pp. xiv, 253. $7.50.

DUE, J. F. The intercity electric railway industry in Canada. Can. stud. in econ. no. 18. Toronto: University of Toronto Press, 1966. Pp. x, 118. $3.95.

FRANK, H. J. Crude oil prices in the Middle East—a study in oligopolistic price behavior. New York and London: Praeger, 1966. Pp. xi, 209. $15.

HERRIGER, H.-P. Die Subventionierung der deutschen Filmwirtschaft. Cologne: Westdeutscher Verlag, 1966. Pp. 56. DM 26.50.

HUNT, H. G. Industrial economics. New York and Oxford: Pergamon Press, 1965. Pp. viii, 225. Paper, $3.95.

JEWKES, J., CHAMBERS, P. AND ROBBINS, L. C. Economics, business and government. Addresses given at a dinner on 13 January 1966, to mark the IEA's 10th year. Occas paper no. 8. London: Inst. Econ. Affairs, 1966. Pp. 32. 5s.

KLEINER, J. Die Textilindustrie in Israel. Mit einer englischen Zusammenfassung: The textile industry in Israel. Basel: Kyklos-Verlag, 1966. Pp. viii, 168. DM 22.

LANSING, J. B. Transportation and economic policy. New York: Free Press; London: Collier-Macmillan, 1966. Pp. xv, 409. $7.95.

LAVE, L. B. Technological change—its conception and measurement. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. 228.

LEIVO, V. Optimization of location and size of automobile dealerships. Pub. no. 28. Helsinki: Helsinki Res. Inst. for Bus. Econ., 1965. Pp. 122.

LOCKLIN, D. P. Economics of transportation. 6th ed. Homewood, Ill.: Irwin, 1966. Pp. xii, 882. $9.50.

MARSCHAK, T. A. The role of project histories in the study of R&D. Santa Monica, Calif.: Rand, 1965. Pp. 119.

MILLER, K. E. AND SAYRE, W. Missouri business trends for retailing, wholesaling and selected services. Stud. no. 9. Columbia: Research Center, School of Bus. and Public Admin., Univ. of Missouri, 1966. Pp. vi, 82. $1.75.

NEHRT, L. C. International marketing of nuclear power plants. Soc. sci. ser. no. 22. Bloomington and London: Indiana Univ. Press, 1966. Pp. xi, 405. $6.

OWEN, W. The metropolitan transportation problem. Rev. ed. Washington: Brookings Inst., 1966. Pp. xiii, 266. $6. First pub. in 1956 and reviewed in 1957 by R W. Harbeson.

ROWLEY, C. K. The British Monopolies Commission. London: Allen & Unwin Ltd., 1966. Pp. 394. 50s.

SAMPSON, R. J. AND FARRIS, M. T. Domestic transportation—practice, theory and policy. Boston: Houghton Mifflin, 1966. Pp. xvi, 464. $7.95.

SANMANN, H. Seeverkehrsmärkte. Grundlegung einer Ökonomik des Seeverkehrs. Verkehrswissenschaftliche no. 1. Aus dem Onst. für Verkehrswissenschaft der Univ. Hamburg. Göttingen: Vandenhoeck & Ruprecht, 1965. Pp. 413. DM 48.

SCOTT, J. C. AND ROCKEFEELER, E. S., eds. Antitrust and trade regulation today—selected analyses. 2nd ed. Washington: Bur. Nat. Affairs, 1966. Pp. 241.

SHAPERO, A., HOWELL, R. P. AND TOMBAUGH, J. R. The structure and dynamics of the defense R & D industry—the Los Angeles and Boston complexes. Menlo Park, Calif.: Stanford Research Inst., 1965. Pp. x, 125. $4.75.

SHEPHERD, W. G. AND GIES, T. G., eds. Utility regulation—new directions in theory and policy. New York: Random House, 1966. Pp. xii, 284. Paper, $2.95.

TARBELL, I. M. The history of the Standard Oil Company. Briefer version ed. by D. M. Chalmers. New York: Harper & Row, 1966. Pp. xxv, 227. Paper, $2.45.

This book originally appeared as a series of articles in *McClure's*, from 1902 to 1904, and was published in book form, in two volumes, by Macmillan in 1904. In this edition, the text has been cut approximately in half and the appendices eliminated. (From publisher's note.)

TYBOUT, R. A., ed. Economics of research and development. Ohio State Univ. Conf. on Econ. of Research and Develop. Columbus: Ohio State Univ. Press, 1965. Pp. 458.

WILCOX, C. Public policies toward business. 3rd ed. Homewood, Ill.: Irwin, 1966. Pp. xiv, 882. $9.25. Reviewed in 1956 by G. W. Nutter.

WITHERS, W. Business in society—an introduction to business. New York: Appleton-Century-Crofts, 1966. Pp. xi, 408. $6.

Defense industry diversification—an analysis with 12 case studies. U.S. Arms Control and Disarmament Agency pub. no. 30. Washington: Supt. Docs., 1966. Pp. ix, 334. $2.25.

Japan—meeting the challenge of Asia's richest market. New York: Business International, 1966. Pp. 88. $40.

Land Economics; Agricultural Economics; Economic Geography; Housing

AHMED, K. Agriculture in East Pakistan. Decca: Ahmed Bros. Publications, 1965. Pp. 428.

BARKER, M. B. California retirement communities. Spec. rept. no. 2. Berkeley: Center for Real Estate and Urban Econ., Inst. of Urban and Regional Develop., Univ. of California, 1966. Pp. x, 103. Paper, $2.50.

BARTER, P. G. H. Problems of agricultural development. Lectures assembled on the occasion of his retirement from the Food and Agriculture Organization of the United Nations. Geneva: Lib. Droz, 1966. Pp. 95.

BOWLEY, M. The British building industry—four studies in response and resistance to change. New York and Cambridge: Cambridge Univ. Press, 1966. Pp. xiv, 488. $13.50.

BROOKS, D. B. Supply and competition in minor metals. Washington: Resources for the Future; Johns Hopkins Press, distrib., 1965. Pp. xv, 147. Paper, $3.

BUCK, J. L., DAWSON, O. L. AND WU, Y. L. Food and agriculture in Communist China. New York: Praeger; London: Pall Mall Press, for Hoover Inst. on War, Revolution, and Peace, Stanford Univ., 1966. p. xiii, 171. $6.

CLAWSON, M. WITH STEWART, C. L. Land use information—a critical survey of U.S. statistics including possibilities for greater uniformity. Washington: Resources for the Future; Johns Hopkins Press, distrib., 1965. Pp. xvii, 402. Paper, $6.

COLE, J. P. Latin America—an economic and social geography. Washington: Butterworths, 1965. Pp. xvii, 468. $10.95.

COURTENAY, P. Plantation agriculture. New York and London: Praeger, 1966. Pp. 215. $7.50.

COUTSOUMARIS, G., WESTEBBE, R. M., PSILOS, D., MICHALAKIS, A. AND XANTHAKIS, N. Analysis and assessment of the economic effects of the U.S. PL 480 program in Greece. Spec. stud. ser. no. 1. Athens: Center of Planning and Econ. Res., 1965. Pp. 293.

DEAN, E. The supply responses of African farmers—theory and measurement in Malawi. Contrib. to econ. anal. no. 41. Amsterdam: North-Holland Pub. Co., 1966. Pp. xiv, 174. $7.

FRANCO, A. Características do latifúndio e condições associadas com a sua existência no sul do Brasil. Projeto 206. Rio de Janiero: IICA-CIRA (Inst. Interamericano de ciências Agrícolas de OEA—Centro Interamericano de Reforma Agrária), 1965. Pp. 34.

GAUMNITZ, E. A., ed. Futures trading seminar—a commodity marketing forum for college teachers of economics. 1st ed., vol. 3. Madison: Mimir, 1966. Pp. xv, 264. $5.

GILES, A. Investigación económica sobre producción y mercadeo . . . un estudio en granjas avícolas de prestatarios del Crédito Supervisado del Valle del Cauca, Colombia. Bogotá, Columbia: IICA-CIRA (Inst. Interamericano de Ciências Agrícolas—Centro Interamericano de Reforma Agraria), 1966. Pp. 62.

HIRST, D. Oil and public opinion in the Middle East. New York: Praeger, 1966. Pp. 127. $6.

KANOVSKY, E. The economy of the Israeli Kibbutz. Harvard Middle Eastern monogr. no. 13. Cambridge: Center for Middle Eastern Stud., Harvard Univ.; Harvard Univ. Press, distrib., 1966. Pp. ix, 169.

MEYERSON, M. AND BANFIELD, E. C. Boston: the job ahead. Cambridge: Harvard Univ. Press, 1966. Pp. 121. $3.95.

MIKDASHI, Z. A financial analysis of Middle Eastern oil concessions: 1901-65. New York and London: Praeger, 1966. Pp. xv, 340. $17.50.

NIMITZ, N. Farm employment in the Soviet Union, 1928-1963. Santa Monica, Calif.: Rand, 1965. Pp. 155.

OURY, B. A production model for wheat and feedgrains in France (1946-1961). Contrib. to econ. analysis no. 40. Amsterdam: North-Holland Pub. Co., 1966. Pp. xv, 306.

PICKARD, J. P. AND TWERASER, G. C. Urban real estate research—1964. Res monogr no. 11 Washington: Urban Land Inst., 1965. Pp. 91. $4; single copies, $2 to ULI members and libraries.

PILLAI, V. R. AND PANIKAR, P. G. K. Land reclamation in Kerala. Kerala Univ. econ. ser no 1. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. x, 197. $10.50.

RAUNIKAR, R., PURCELL, J. C. AND ELROD, J. C. Consumption and expenditure analysis for meat, meat products, and eggs in Atlanta, Georgia. Tech. bull. n.s. 46. Athens: Georgia Agric. Experiment Stations Univ. of Georgia College of Agric., 1965. Pp. 61.

SEWELL, W. R. D., ed. Human dimensions of weather modification. Dept. Geog. res. paper no. 105. Chicago: Univ. Chicago Press, 1966. Pp. xii, 423.

SOVANI, N. V. Urbanization and urban India. New York: Asia Pub. House, Taplinger, distrib., 1966. Pp. 160. $9.50.

STAMER, H. Landwirtschaftliche Marktlehre. Pt. 1, Bestimmungsgründe und Entwicklungstendenzen des Marktes. Hamburg: Parey, 1966. Pp. 243.

VERNON, R. The myth and reality of our urban problems. Cambridge: Harvard Univ. Press, 1966. Pp. vii, 90. $2.95. First pub. in 1962.

WEBSTER, C. C. Agriculture in the tropics. London: Longmans, 1966. Pp. xiii, 488.

WILSON, J. Q., ed. Urban renewal: the record and the controversy. Cambridge, Mass. and London: M.I.T. Press, 1966. Pp. xix, 683. $12.50.

Fourth biennial report on water resources development. Econ. and Soc. Council, Official Records: 40th sess., suppl. no. 3. U.N. pub. no. E/4138. New York: United Nations, 1966. Pp. 85. $1.50.

Land tenure conditions and socio-economic development of the agricultural sector—Brazil, CIDA (Inter-American Com. Agric. Develop.). Washington, D.C.: Pan American Union, 1966. Pp. xxxiii, 609. $4.50.

Projective economic study of the Ohio River Basin [Vol. 3], Append. B.: Ohio River Basin comprehensive survey. Prepared for The Corps of Engineers, U.S. Army Engineer Div., Ohio River, by A. D. Little, Inc. Washington: Supt. Docs., 1964. Var. pp.

Labor Economics

ADAMS, G. Age of industrial violence—1910-15. The activities and findings of the U.S. Commission on Industrial Relations. New York and London: Columbia Univ. Press, 1966. Pp. xii, 316. $8.50.

BERTRAM, G. W. Consolidated bargaining in California construction: an appraisal of twenty-five years' experience. Monogr. ser. no. 12. Los Angeles: Inst. Industrial Relations, Univ. of California, 1966. Pp. xiv, 258. $3.

BRODERSEN, A. The Soviet worker. New York: Random House, 1966. Pp. ix, 278. $4.95; paper, $2.45.

BULLOCK, P. Equal opportunity in employment. Los Angeles: Inst. Industrial Relations, Univ. of California, 1966. Pp. vii, 114. Paper, 75¢.

COHEN, S. Labor in the United States. 2nd ed. Columbus, Ohio: Merrill Books, 1966. Pp. xv, 640. $8.95. First ed. reviewed in 1962 by R. J. Ward.

CORINA, J. Incomes policy—problems and prospects. Pt. 1, The labour market. Pt. 2, The development of incomes policy. London: Inst. of Personnel Management, 1966. Pp. 35; 82. 7s. 6d; 17s. 6d; together, £1 1s.

CORSON, J. J. AND PAUL, R. S. Men near the top—filling key posts in the federal service. Baltimore: Johns Hopkins Press, 1966. Pp. xvii, 189. $6; paper, $3.

DHOLAKIA, J. L. Industrial labour and economic development in India. Ahmedabad: Anada Book Depot, 1965. Pp. 176. Rs 6.50.

ENGLER, J. J. DE C., ZAGATTO, A. G. AND DE ARAÚJO, P. F. C. Produtividade de recursos e rendimento ótimo da lavoura canavieira, referentes a proprietários arrendatarios e parceiros em piracicaba. Brazil: IICA-CIRA (Inst. Interamecicano de Ciências Agricolas de OEA-Centro Interamericano de Reforma Agraria), 1965. Pp. 34.

FULMER, J. L. AND PITTS, J. E. Kentucky employment trends from 1951 to 1963 with projections to 1965-75. Lexington: Bur. Bus. Res., Univ. of Kentucky, 1965. Pp. v. 79. Single copies free.

GALENSON, W. A. A primer on employment and wages. New York: Vintage Books, Random House, 1966. Pp. 146. Paper, $1.95.

GORDON, R. A. AND GORDON, M. S., eds. Prosperity and unemployment. New York and London: Wiley, 1966. Pp. ix, 353. $3.95.

GOULD, J. M. The technical elite. New York: Augustus M. Kelley, 1966. Pp. 178. $5.

HUGH-JONES, E. M., ed. Wage-structure in theory and practice. Stud. indus. econ. no. 6. Amsterdam: North-Holland Pub. Co., 1966. Pp. x, 242. $7.

Contributions by M. W. Leiserson, J. Lecaillon, J. Marchal and J. R. Crossley.

KEYSERLING, L. H. The role of wages in a great society; stressing minimum-wage gains to help the working poor. Conf. on Econ. Progress stud. no. 18. Washington: Conf. on Econ. Progress, 1966. Pp. 114.

LECLAIR, M. J. Bibliography on labor in Africa, 1960-64. Washington: Supt. Docs., 1966. Pp. 121.

MABRY, B. D. Labor relations and collective bargaining. New York: Ronald, 1966. Pp. vii, 475. Text, $7.50; cases, $3.

MELTZ, N. M. Changes in the occupational composition of the Canadian Labour Force, 1931-1961. Ottawa: Dept. of Labour, Econ. and Res. Branch, 1965 Pp. 136.

Morgan, C. A. Labor economics. Homewood, ILL.: Dorsey Press, 1966. Pp. xii, 677. $8.5(Reviewed in 1962 by A. T. Helbing.

Roberts, B. C. and Smith, J. H., eds. Manpower policy and employment trends. London G. Bell, for London School of Econ. and Pol. Sci., 1966. Pp. 137. Paper, 25s.

Saunders, G. Wage determination in Canada. Occas. paper no. 3. Ottawa: R. Duhame Queen's Printer, 1965. Pp. 44.

Sheppard, H. L. and Striner, H. E. Civil rights, employment, and the social status of Amer ican Negroes. Report for U.S Commission on Civil Rights, contract no. CCR-66-5. Kala mazoo, Mich.: Upjohn Inst. Employment Research, 1966. Pp. vii, 85. (One copy free.) 50

Shimaoka, H. R. and Najita, J. M. Duration of collective bargaining agreements i Hawaii, 1966. Occas. paper 54. Honolulu: Indus. Rel. Center, Univ. of Hawaii, 196(Pp. 22. Single copies free.

Shultz, G. P. and Weber, A. R. Strategies for the displaced worker—confronting economi change. New York and London: Harper & Row, 1966. Pp. xvi, 221. $4.95.

Slavick, F. Compulsory and flexible retirement in the American economy. Ithaca: N.Y State School of Indus. and Labor Rels., Cornell Univ., 1966. Pp. vii, 172. Paper, $4.

Somers, G. G., ed. Proceedings of the eighteenth annual winter meeting—and index of IRR/ publications 1961-1965. Madison: Industrial Relations Res. Assoc., 1966. Pp. viii, 390. $4

Sturmthal, A., ed. White-collar trade unions—contemporary developments in industrialize societies. Urbana, Ill. and London: Univ. of Illinois, 1966. Pp. xv, 412. $7.95.

Turner, I. Industrial labour and politics—the dynamics of the labour movement in Easter Australia, 1900-1921. New York and Cambridge: Cambridge Univ. Press; Canberra Australian Nat. Univ., 1965. Pp. xx, 272. $10.

Ulman, L. The rise of the national trade union—the development and significance of it structure, governing institutions, and economic policies. 2nd ed. Cambridge: Harvard Univ Press, 1966. Pp. xix, 639. $12.50.

Employment and earnings statistics for the United States 1909-65. Based on the 195 Standard Industrial Classification, as amended by the 1963 Supplement. Dept. of Labor Bur. Labor Stat. bull. no. 1312-3. Washington: Supt. Docs., 1965. Pp. xxxv, 732. $4.25.

Manpower planning and utilization in the federal government. Washington: U.S. Civi Scve. Comm., 1963. Pp. 126.

The measurement and interpretation of job vacancies—a conference report of the Nationa Bureau of Economic Research. New York and London: Nat. Bur. Econ. Research Columbia Univ. Press, distrib., 1966. Pp. ix, 593. $12.50.

Ocupación y desocupación—Gran Santiago, diciembre de 1965. Inst. Econ. pub. no. 82 Santiago: Inst. de Econ., Univ. de Chile, 1965. Var. pp.

A tablulation of case studies on technological change—economic and social problems re- viewed in 160 case studies. Labour and Automation bull. no. 2. Washington and Geneva Internat. Labour Office, 1965. Pp. iii, 87. $1.

Population; Welfare Programs; Consumer Economics

Beeby, C. E. The quality of education in developing countries. Cambridge: Harvard Univ. Press, 1966. Pp. x, 139. $3.95.

Bourgeois-Pichat, J. Population growth and development. Internat. Conciliation no. 556 New York: Carnegie Endowment for Internat. Peace, Taplinger, distrib., 1966. Pp. 81. 50¢

Britt, S. H., ed. Consumer behavior and the behavioral sciences—theories and applications. New York and London: Wiley, 1966. Pp. xxviii, 592. $11.50.

Cartter, A. M. An assessment of quality in graduate education. Washington: American Council on Education, 1966. Pp. xvi, 131. $5; paper, $3.

Clark, H. F. and Sloan, H. S. Classrooms on Main Street—an account of specialty schools in the United States that train for work and leisure. Inst. for Instructional Improvement, Inc. New York: Teachers College Press, Columbia Univ., 1966. Pp. xv, 162. $3.95.

COALE, A. J. AND DEMENY, P. Regional model life table and stable populations. Princeton: Princeton Univ. Press, 1966. Pp. xiii, 871. $20.

CURLE, A. Planning for education in Pakistan. Cambridge: Harvard Univ. Press, 1966. Pp. xxii, 208. $5.25.

DONALDSON, E. F. AND PFAHL, J. K. Personal finance. New York: Ronald, 1966. Pp. xvi, 803. $8.75.

ELLIOTT, W. Y., ed. Education and training in the developing countries—the role of U.S. foreign aid. New York: Praeger, 1966. Pp. 399. $7.50.

FISHMAN, L., ed. Poverty amid affluence. New Haven and London: Yale Univ. Press, 1966. Pp. xii, 246. $6; paper, $1.75.

FLEISHER, B. M. The economics of delinquency. Chicago: Quadrangle Books, 1966. Pp. 127. $4.50.

GOODMAN, L. H., ed. Economic progress and social welfare. New York and London: Columbia Univ. Press, 1966. Pp. vii, 233. $5.

HEER, D. M. After nuclear attack—a demographic inquiry. New York and London: Praeger, 1966. Pp. 440. $15.

HIRSCH, W. Z. SEGELHORST, E. W. AND MARCUS, M. J. Spillover of public education costs and benefits. Los Angeles: Inst. of Govt. and Public Affairs, Univ. of California, 1964. Pp. xviii, 465.

HUSAIN, I., AFZAL, M. AND RIZVI, S.A.A.B. Social characteristics of the people of Karachi. Monogr. in econ. of develop., no. 14. Karachi, Pakistan: Pakistan Inst. Develop. Econ., 1965. Pp. 155. Rs 5.

KLINOV-MALUL, R. The profitability of investment in education in Israel. Jerusalem: The Maurice Falk Inst. for Econ. Research in Israel, 1966. Pp. xiv, 109.

MEISSNER, H. H., ed. Poverty in the affluent society. New York: Harper & Row, 1966. Pp. 251.

O'HARA, W. T., ed. John F. Kennedy on education. New York: Columbia Univ., Teachers College Press, 1966. Pp. xi, 305. $6.50; paper, $2.95.

ORNATI, O. WITH SWEET, J. S. Poverty amid affluence. New York: Twentieth Century Fund, 1966. Pp. xvi, 208. Paper, $3.25.

SEXTON, P. C. Spanish Harlem. New York: Harper & Row, 1966. Pp. xi, 208. Paper, $1.45.

STROUP, H. Bureaucracy in higher education. New York: Free Press; London: Collier-Macmillan, 1966. Pp. xi, 242. $5.95.

VON BRAUNSCHWEIG, C. Der Konsument und seine Vertretung—eine Studie über Verbraucherverbände. Univ. of Mainz, Forschungsinst. f. Wirtschaftspol, pub. no. 20. Heidelberg: Quelle & Meyer, 1965. Pp. 296. DM 35.

WILLIAMS, H. Planning for effective resource allocation in universities. Washington: American Council on Education, 1966. Pp. x, 78. $2.50.

WILLIAMS, P. Aid in Uganda—education. London: The Overseas Develop. Inst., 1966. Pp. 152. 20s.

WRIGHT, P. Earned degrees conferred 1963-1964—bachelor's and higher degrees (final report). U.S. Dept. Health, Educ. and Welfare, misc. no. 54. Washington: Supt. Docs., 1966. Pp. vii, 283. $1.75

WRIGHT, P. S. Enrollment for advanced degrees—fall 1963: final report. Dept. Health, Educ. and Welfare, circ. no. 786. Washington: Supt. Docs., 1965. Pp. x, 265. $1.50.

Annual price survey—family budget costs, October 1965. Supplement to *A Family Budget Standard* and *How to Measure Ability to Pay for Social and Health Services.* 9th ed. New York: Community Council of Greater N.Y., 1966. Pp. iv, 76. $2.

Vital and health statistics data from the national vital statistics system—natility statistics analysis—United States—1963. Pub. Health. Svce. pub. no. 1000, ser. 21, no. 8. Washington: Supt. Docs., 1966. Pp. 29. 30¢.

Vital and health statistics data from the national vital statistics system—seasonal variation

of births—United States, 1933-63. Pub. Health Svce. pub. no. 1000, ser. 21, no. 9. Washington: Supt. Docs., 1966. Pp. vi, 59. 45¢.

Related Disciplines

}AKER, R. M., JR. AND PHIFER, G. Salesmanship: communication, persuasion, perception. Boston: Allyn and Bacon, 1966. Pp. x, 422. $8.50.

}ARBER, R. J. The politics of research. Washington, D.C.: Pub. Affairs Press, 1966. Pp. vi, 167. $4.50.

}ENDIX, R. AND LIPSET, S. M., eds. Class, status, and power—social stratification in comparative perspective. 2nd ed. New York: Free Press; London: Collier-Macmillan, 1966. Pp. xviii, 677. $9.95. First ed. pub. in 1953.

;ERGER, A. S. A framework for independence. Port Jefferson Station, N.Y.: Pepper and Salt Press, 1964. Pp. vii, 64. Paper, 95¢.

:OOK, F. J. The corrupted land—the social morality of modern America. New York: Macmillan, 1966. Pp. 352. $5.95.

)UCHACEK, I. D. Nations and men—international politics today. New York: Holt, Rinehart and Winston, 1966. Pp. x, 390. $6.50.

;ASTON, D., ed. Varieties of political theory. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. vi, 154. $4.95.

Contributions by D. Easton, H. Simon, J. Buchanan, J. March, T. Parsons, M. G. Smith ad A. Rapoport.

ALK, R. A. AND MENDLOVITZ, S. H., eds. The strategy of world order: vol. 1, Toward a theory of war prevention; vol. 2, International law; vol. 3, The United Nations; vol. 4, Disarmament and economic development. New York: World Law Fund, 1966. Pp. xx, 394; xiv, 382; xv, 848; xv, 672. Available in 4-vol. set only, $14; paperbound set, $10.

EIS, H. The diplomacy of the dollar, 1919-1932. New York: Norton, 1966. Pp. vii, 81. First pub. in 1950 by Johns Hopkins Press.

OROWITZ, I. L. Three worlds of development—the theory and practice of international stratification. New York: Oxford Univ. Press, 1966. Pp. xiv, 475. $8.50.

ιUESTE, J. M. L. La política de establización británica en los años cincuenta. Madrid: Consejo Superior de Investigaciones Cientificas, Inst. "Sancho de Moncada" de Econ., 1965. Pp. 341. 250 ptas.

ιHUE, K. C. World of laughter—the motion picture comedy short, 1910-1930. Norman, Oklahoma: Univ. of Oklahoma Press, 1966. Pp. xviii, 240. $4.95.

ιJPHART, A. The trauma of decolonization—the Dutch and West New Guinea. Yale stud. in pol. sci. no. 17. New Haven and London: Yale Univ. Press, 1966. Pp. xi, 303. $7.50.

ASSE, B. L. The church and social progress—background readings for Pope John's *Mater et Magistra*. Milwaukee: Bruce, 1966. Pp. xi, 248. $5.50.

'cCONNELL, G. Private power & American democracy. New York: Knopf, 1966. Pp. 397. $6.95.

ERCHANT, L. T., ed. Neighbors taken for granted. New York and London: Praeger; Toronto: Burns and MacEachern, 1966. Pp. xv, 166. $4.95.

ICHELS, R. Political parties—a sociological study of the oligarchical tendencies of modern democracy. Trans. from the Italian ed. by E. Paul and C. Paul. New York: Free Press; London: Collier-Macmillan, 1966. Pp. 379. Paper, $2.45.

ITCHELL, B. R. AND BOEHM, K. British parliamentary election results, 1950-1964. New York: Cambridge Univ. Press, 1966. Pp. viii, 135. Paper, $4.50.

.EYER, K. Zentralplanwirtschaft und Zivilrecht—Juristische Untersuchungen zur Wirtschaftsordung der SBZ. Stuttgart: Gustav Fischer Verlag, 1965. Pp. ix, 195. DM 28.50.

)SEN, G. Democracy and economic change in India. Berkeley: Univ. of California Press, 1966. Pp. xviii, 326. $6.75.

RUDNER, R. S. Philosophy of social science. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. x, 118. $1.75.

RUTKOWSKI, E. H. The politics of military aviation procurement, 1926-1934: a study in the political assertion of consensual values. Columbus: Ohio State Univ. Press, 1966. Pp. 318. $6.50.

SCHELLING, T. C. Arms and influence. New Haven and London: Yale Univ. Press, 1966. Pp. viii, 293. $7.50.

SCOTT, A. M. WITH LUCAS, W. A. AND LUCAS, T. M. Simulation and national development. New York and London: Wiley, 1966. Pp. ix, 177. $5.95.

SMITH, B. L. R. The Rand Corporation—case study of a nonprofit advisory corporation. Cambridge: Harvard Univ. Press, 1966. Pp. 332.

VAN DER BEUGEL, E. H. From Marshall aid to Atlantic partnership—European integration as a concern of American foreign policy. New York, Amsterdam and London: Elsevier Pub. Co., 1966. Pp. xxi, 480. $9.75.

WARD, B. Spaceship earth. New York: Columbia Univ. Press, 1966. Pp. viii, 152. $4.50.

WHITAKER, A. P. AND JORDAN, D. C. Nationalism in contemporary Latin America. New York: Free Press, London: Collier-Macmillan, 1966. Pp. ix, 229. $6.95.

Bibliography of Asian studies 1964. *Jour. Asian Stud.*, vol. 29, no. 5, Sept. 1965. Ann Arbor, Mich.; Assoc. for Asian Studies, 1965. Pp. vi, 397. $5.

Contributions by E. G. Beal, Jr., H. L. Boorman, J. M. Echols, L. C. Goodrich, E. J. Keyes and J. E. Olson.

New program of the Communist Party U.S.A. (a draft). New York: Pol. Affairs Pub., 1966. Pp. 127. 95¢.

NOTES

SEVENTY-NINTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Hilton Hotel, San Francisco, California—December 27-29, 1966

Preliminary Announcement of the Program

Tuesday, December 27, 1966

8:30 A.M. ECONOMIC ANALYSIS OF WATER RESOURCE PROBLEMS (Joint Session with the American Farm Economic Association)

Chairman: MAURICE M. KELSO, University of Arizona
Papers: Nonmarket Values and Efficiency of Public Investments in Water Resources
A. ALLAN SCHMID, Michigan State University
Urban Water Supply Economics: A Second Look at New York City and Southern California
JACK HIRSHLEIFER, University of California, Los Angeles and J. W. MILLIMAN, Indiana University
Water Policy and Economic Optimizing: Some Conceptual Problems in Water Research
S. V. CIRIACY-WANTRUP, University of California, Berkeley
Discussion: ROBERT H. HAVEMAN, Grinnell College
M. MASON GAFFNEY, University of Wisconsin, Milwaukee
ARTHUR MAASS, Harvard University

8:30 A.M. THE MEASUREMENT OF PRICE CHANGE

Chairman: DANIEL BRILL, Board of Governors, Federal Reserve System
Papers: Some Significant Results from the Study of International Price Competitiveness
IRVING B. KRAVIS, University of Pennsylvania and ROBERT E. LIPSEY, National Bureau of Economic Research
The Construction of Industrial Price Indexes
JAMES K. KINDAHL, University of Chicago
Measurement of Price and Quality Changes in Consumer Capital Goods
PHOEBUS J. DHRYMES, University of Pennsylvania
Discussion: DOROTHY S. BRADY, University of Pennsylvania
GREGORY KIPNIS, National Industrial Conference Board

8:30 A.M. CONDITIONS FOR TECHNOLOGICAL PROGRESS AND ECONOMIC GROWTH IN COMMUNIST CHINA (Joint Session with the Association for the Study of Soviet-type Economies)

Chairman: ALEXANDER ECKSTEIN, University of Michigan
Papers: Role of Development Policies and Economic Organization in Innovation and Growth
EDWIN F. JONES, Department of State
Incentive Policies and Technological Progress in Agriculture
J. C. LIU, McGill University and the International Bank for Reconstruction and Development
Relationships between Foreign Trade, Innovation, and Outlook for Economic Growth
ROBERT DERNBERGER, University of Chicago

Discussion: J. M. MONTIAS, Yale University
HENRY ROSOVSKY, Harvard University

8:30 A.M. GRADUATE STUDENT PAPERS (Joint Session with *The American Economist*)
Chairman: GERHARD TINTNER, University of Southern California
Papers: A Two-Sector Growth Model with Diminishing Returns and Endogenous (Neo-Malthusian) Growth of Population
RASESH THAKKAR, University of Rochester
Stochastic Reserve Losses
ELEANOR M. BIRCH and JOHN M. HEINEKE, University of Iowa
Economic Theory and Policy Decisions: A Case Study of the Canadian Dollar since 1945
ALEX G. VICAS, Loyola College (Canada)
Discussion: MARK Z. FABRYCY, New York University
JOHN BOORMAN, University of Southern California
ERNEST J. BARTELL, Notre Dame University

10:45 A.M. POLICY ROUNDTABLE 1: SOME PROBLEMS OF FULL EMPLOYMENT (Joint Session with SSRC Committee on Economic Stability)
Moderator: R. A. GORDON, University of California, Berkeley
Panel: WALTER HOADLEY, Bank of America
WARREN L. SMITH, University of Michigan
HERBERT STEIN, Committee for Economic Development
POLICY ROUNDTABLE 2: RESEARCH AND DEVELOPMENT
Moderator: H. E. RILEY, National Science Foundation
Panel: BURTON KLEIN, RAND Corporation
ZVI GRILICHES, University of Chicago

2:30 P.M. MILITARY MANPOWER PROCUREMENT (Joint Session with Industrial Relations Research Association)
Chairman: HAROLD WOOL, U. S. Department of Defense
Papers: The Supply of Military Personnel in the Absence of a Draft
STUART H. ALTMAN, Brown University and ALAN E. FECHTER, Institute of Defense Analysis
Re-enlistments in the U. S. Navy: A Cost Effectiveness Study
FRANKLIN M. FISHER, Massachusetts Institute of Technology and ANTON S. MORTON, Arthur D. Little, Inc.
The Economic Cost of the Draft
WALTER Y. OI, University of Washington
Discussion: GEORGE H. HILDEBRAND, Cornell University
PAUL A. WEINSTEIN, University of Maryland

2:30 P.M. ECONOMIC DEVELOPMENT (Joint Session with Econometric Society)
Chairman: MOSES ABRAMOVITZ, Stanford University
Papers: Some Lessons of History for Developing Nations
RONDO CAMERON, University of Wisconsin
Sources of Postwar Growth in Nine Advanced Countries
EDWARD F. DENISON, Brookings Institution
Optimal Patterns of Growth in an Open Economy
HOLLIS B. CHENERY and ROBERT DORFMAN, Harvard University
Discussion: HENRY ROSOVSKY, Harvard University
CHARLES KINDLEBERGER, Massachusetts Institute of Technology

2:30 P.M. AGGREGATE MODELS
Chairman: SAMUEL H. NERLOVE, University of California, Los Angeles
Papers: Effects of Monetary and Fiscal Policies with Keynesian or Classical Demand Elasticities
CARL CHRIST, Johns Hopkins University

Policy Multipliers and the Structure of the Canadian Monetary Sector—An Econometric Analysis
HAROLD SHAPIRO, University of Michigan
Expectations and Adjustment in the Monetary Sector
EDGAR FEIGE, University of Wisconsin
Discussion: GARY FROMM, Brookings Institution
THOMAS SAVING, Michigan State University
GEORGE MORRISON, State University of New York at Buffalo

8:00 P.M. RICHARD T. ELY LECTURE
Invited Lecture: ABBA LERNER, Michigan State University and Tel-Aviv University

Wednesday, December 28, 1966

8:30 A.M. COST-BENEFIT ANALYSIS FOR GOVERNMENT DECISIONS
Chairman: WILLIAM H. MECKLING, University of Rochester
Papers: Government-Industry Development of a Commercial Supersonic Aircraft
STEPHEN ENKE, General Electric-Tempo, and JOHN WALGREEN, Office of the Secretary of Defense
An Application of Cost-Benefit Analysis to the Work-Experience Program
WORTH BATEMAN, Department of Health, Education, and Welfare
A Proposed Methodology for Comparing Federally Assisted Housing Programs
WILLIAM ROSS, U. S. Department of Housing and Urban Development
Discussion: GEORGE W. HILTON, University of California, Los Angeles
HENRY ROWEN, U. S. Bureau of the Budget
MARTIN ANDERSON, Columbia University

8:30 A.M. PROPERTY RIGHTS AND BEHAVIOR
Chairman: ARMEN A. ALCHIAN, University of California, Los Angeles
Papers: Stock vs. Mutual Savings and Loan Associations: Some Evidence of Differences in Behavior
ALFRED NICOLS, University of California, Los Angeles
Costs of Property Rights
HAROLD DEMSETZ, University of Chicago
Discussion: HOWARD KUNREUTHER, University of Chicago
ANDREW WHINSTON, Purdue University

8:30 A.M. ECONOMIC HISTORY
Chairman: ARTHUR H. COLE, Harvard University
Papers: John Law of Lauriston: Banker, Gamester, Merchant, Chief?
EARL J. HAMILTON, University of Chicago and State University of New York at Binghamton
Government Regulation and Growth in the French Paper Industry During the Eighteenth Century
WARREN C. SCOVILLE, University of California, Los Angeles
New Light on a Statistical Dark Age: U. S. Real Output, 1800-1840?
PAUL DAVID, Stanford University
Discussion: JOHN H. DALES, University of Toronto
STANLEY ENGERMAN, University of Rochester
ROSS M. ROBERTSON, Indiana University

10:45 A.M. POLICY ROUNDTABLE 3: GUIDEPOSTS AND INFLATION (Joint Session with Industrial Relations Research Association)
Moderator: ALBERT REES, Princeton University
Panel: WILLIAM G. BOWEN, Princeton University
JOHN T. DUNLOP, Harvard University
OTTO ECKSTEIN, Harvard University
GEORGE SHULTZ, University of Chicago

10:45 A.M. POLICY ROUNDTABLE 4: REGULATION OF INDUSTRY
Moderator: NEIL H. JACOBY, University of California, Los Angeles
Panel: WILLIAM J. BAUMOL, Princeton University
JOEL DEAN, Columbia University
JOHN R. MEYER, Harvard University
GEORGE J. STIGLER, University of Chicago

12:30 P.M. JOINT LUNCHEON: AMERICAN ECONOMIC ASSOCIATION AND AMERICAN FINANCE ASSOCIATION
Chairman: FRITZ MACHLUP, Princeton University
Talk: Coordination of Economic Policy
ALLAN SPROUL, California

2:30 P.M. ANTI-TRUST AND MONOPOLY
Chairman: AARON DIRECTOR, University of Chicago
Papers: The Goals of Anti-Trust Policy
ROBERT H. BORK, Yale University
Vertical Mergers, Market Power and the Anti-Trust Laws
WILLIAM COMANOR, Harvard University
Conscious Parallelism and the Kinked Oligopoly Demand Curve
WILLIAM HAMBURGER, University of North Carolina
Discussion: PHIL C. NEAL, University of Chicago
JOHN S. MCGEE, Duke University
B. PETER PASHIGIAN, University of Chicago

2:30 P.M. INVITED DOCTORAL DISSERTATIONS I
Chairman: E. CARY BROWN, Massachusetts Institute of Technology
Papers: Daily Newspapers, Monopolistic Competition, and Economies of Scale
JAMES N. ROSSE (Ph.D., University of Minnesota), Stanford University
Rational Choice and Patterns of Growth in a Monetary Economy
MIGUEL SIDRAUSKI (Ph.D., University of Chicago), Massachusetts Institute of Technology
The Canadian Experience with Flexible Exchange Rates
JOHN PIPPENGER (Ph.D., University of California, Los Angeles), University of California, Santa Barbara
Discussion: HARRY EASTMAN, University of Toronto
DALE JORGENSEN, University of California, Berkeley
PETER O. STEINER, University of Wisconsin

2:30 P.M. TOPICS IN MONEY (Joint session with American Finance Association)
Chairman: J. FRED WESTON, University of California, Los Angeles
Papers: Some Implications of Money Supply Analysis
DAVID FAND, State University of New York at Buffalo
Keynes and the Keynesians: A Suggested Interpretation
A. LEIJONHUFVUD, University of California, Los Angeles
Financial Regulation and the Effectiveness of Monetary Policy
WILLIAM BRAINARD, Yale University
Discussion: ALLAN H. MELTZER, Carnegie Institute of Technology

MYRON ROSS, Western Michigan University
E. J. KANE, Boston College

8:00 P.M. PRESIDENTIAL ADDRESS
Chairman: RICHARD LESTER, Princeton University
Presidential Address: FRITZ MACHLUP, Princeton University

9:15 P.M. BUSINESS MEETING

Thursday, December 29

8:30 A.M. ECONOMICS OF HEALTH
Chairman: JOSEPH J. SPENGLER, Duke University
Papers: The Allocation of Research Among Diseases
SIMON ROTTENBERG, Duke University
The Economics of Hospital Systems: Peak Loads and Regional Coordination
MILLARD F. LONG, University of Chicago and PAUL J. FELDSTEIN, University of Michigan
The Economic Effects of Malaria Eradication
ROBIN BARLOW, University of Michigan
Discussion: PETER NEWMAN, Johns Hopkins University
GEORGE H. BORTS, Brown University

8:30 A.M. THE "CONVERGENCE" HYPOTHESIS: THE PLANNING AND MARKET ELEMENTS IN THE DEVELOPMENT OF SOVIET, WESTERN AND DEVELOPING ECONOMIES
(Joint Session with Association for Comparative Economics)
Chairman: GERHARD COLM, National Planning Association
Papers: From the Aspect of:
Soviet-Type Economies, ABRAM BERGSON, Harvard University
Western-Type Economies, NEIL CHAMBERLAIN, Yale University
Developing Economies, SHANTI S. TANGRI, Wayne State University
Discussion: VICTOR PERLO, Economic Consultant
ALLAN GRUCHY, University of Maryland

8:30 A.M. INVITED DOCTORAL DISSERTATIONS II
Chairman: JOE S. BAIN, University of California, Berkeley
Papers: Natural Resources, Factor Mix, and Factor Reversal in International Trade
SEIJI NAYA (Ph.D., University of Wisconsin)
Adam Smith's Theory of Justice, Prudence and Beneficence
WILLIAM F. CAMPBELL (Ph.D. University of Virginia), Louisiana State University
The Effect of State Fair Employment Laws on the Economic Position of Nonwhites
WILLIAM LANDES (Ph.D., Columbia University), University of Chicago
Discussion: O. H. BROWNLEE, University of Minnesota
RICHARD NELSON, The RAND Corporation
ANTHONY M. TANG, Vanderbilt University and the Chinese University, Hong Kong

10:45 A.M. POLICY ROUNDTABLE 5: INTERNATIONAL PAYMENTS PROBLEMS
Moderator: GOTTFRIED HABERLER, Harvard University
Panel: EMILE DESPRES, Stanford University

H. G. JOHNSON, University of Chicago and London School of Economics
PETER KENEN, Columbia University
ROBERT ROOSA, Brown Brothers Harriman and Co.

10:45 A.M. POLICY ROUNDTABLE 6: ROLE OF GOVERNMENT IN THE ECONOMY
Moderator: ARTHUR F. BURNS, Columbia University

2:30 P.M. DAS KAPITAL: A CENTENARY APPRECIATION
Papers: The Marxian Model of Capital Accumulation
ALEXANDER ERLICH, Columbia University
Marxian Economics as Economics
PAUL SAMUELSON, Massachusetts Institute of Technology
Marxian Influences in "Bourgeois" Economics
MARTIN BRONFENBRENNER, Carnegie Institute of Technology and Center for Advanced Study in the Behavioral Sciences
Discussion: EVSEY DOMAR, Massachusetts Institute of Technology
DONALD GORDON, University of Rochester
SCOTT GORDON, Indiana University

2:30 P.M. THE EFFICIENCY OF EDUCATION IN ECONOMICS
Chairman: G. L. BACH, Stanford University
Papers: Experiments in the Teaching of Elementary Economics
BERNARD F. HALEY, University of California, Santa Cruz
The Effectiveness of Programmed Learning in Elementary Economics
KEITH LUMSDEN, Stanford University
A New "Test of Economic Understanding" for College Level Students
RENDIGS FELS, Vanderbilt University
Discussion: WILLIAM ALLEN, University of California, Los Angeles
DONALD PADEN, University of Illinois
PHILLIP SAUNDERS, Carnegie Institute of Technology

2:30 P.M. TRANSPORTATION AND PATTERNS OF URBAN DEVELOPMENT
Chairman: JOHN R. MEYER, Harvard University
Papers: An Aggregative Model of Resource Allocation in a Metropolitan Area
EDWIN MILLS, Johns Hopkins University
The Location of Economic Activity in Cities
LEON MOSES, Northwestern University and
HAROLD F. WILLIAMSON, JR., University of Illinois
Post-War Metropolitan Development: Housing Preferences and Auto Ownership
JOHN F. KAIN, Harvard University
Discussion: JULIUS MARGOLIS, Stanford University
BENJAMIN CHINITZ, U. S. Department of Commerce
RICHARD MUTH, Washington University

FELLOWSHIPS AND GRANTS

The Social Science Research Council's annual announcement describing fellowships and grants to be awarded in 1966-67 is now ready for distribution. It lists research training fellowships, foreign area fellowships, faculty research grants and grants for research on foreign areas. Inquiries about research training fellowships, offered to postdoctoral and advanced predoctoral candidates, and faculty research grants should be addressed to Social Science Research Council Fellowships and Grants, 230 Park Ave., New York, N.Y. 10017.

Inquiries about co-sponsored programs should be addressed to the other offices indicated. To avoid missing deadlines correspondence should be initiated no later than early October.

Foreign area fellowships for multidisciplinary predoctoral or postdoctoral training relating to Asia and the Near East, the Soviet Union and Eastern Europe, Africa, Latin America, or Western Europe are under the joint auspices of SSRC and the American Council of Learned Societies. Address inquiries to the Foreign Area Fellowship Program at 444 Madison Ave., New York, N.Y. 10022.

Grants for research on foreign areas will be offered by joint committees of SSRC and ACLS to social scientists holding the Ph.D. degree or its equivalent. Grants for research on Africa, Contemporary China, Latin America, and the Near and Middle East are administered by the SRC. Grants for Asia and for Slavic and East European Studies are administered by the American Council of Learned Societies, 345 East 46th Street, New York, N.Y. 10017.

The National Science Foundation has asked that the A.E.A. notify its members that economists are now eligible to apply for NSF Fellowships under its programs for the improvement of college science, mathematics, and engineering teaching. Fellowships available include predoctoral fellowships, postdoctoral fellowships, fellowships for college teachers, and "extramural fellowships" administered in cooperation with the Department of State in relation to the NATO nations. Fellowship applications for 1967-68 must be received by mid-October. More detailed information about all the programs may be obtained by writing to the Fellowship Section, Division of Graduate Education in Science, National Science Foundation, Washington, D.C., 20550.

The Agricultural Development Council, a nonprofit organization headed by John D. Rockefeller 3rd, is continuing in 1966-67 a program of grants to facilitate research in international agricultural development by professors of American universities. The program is supported by a grant of $1,500,000 from the Ford Foundation.

Applications for research grants will be considered only from full-time faculty members of American universities. While projects must be supervised by grant recipients, advanced graduate students may collaborate in studies. Proposed studies should relate to the economic and human problems of agricultural development in the developing nations. Applications are entertained from economists, sociologists, anthropologists and other social scientists dealing with rural problems. Preference is given to proposals that promise to increase understanding of the process of agricultural development. Most studies involve field work in Asia, Africa or Latin America.

Applications for grants of up to $2,500 may be submitted to the Council at any time. For grants of more than $2,500, applications are considered three times each academic year. In the next period, applications received by October 15, 1966 will be considered for award by November 15, 1966. Requests for research grant application forms or correspondence relating to the program may be addressed to Dr. Clifton R. Wharton, Jr., Director, American Universities Research Program, The Agricultural Development Council, Inc., 630 Fifth Avenue, New York, 10020.

The American Association of University Women announces its Fellowships for American Women and its International Fellowships, 1967-68. The former are open to U.S. women, the latter to women of other countries, who hold the doctorate or who have fulfilled all requirements for the doctorate, except the dissertation. In each case, the deadline for filing is December 1, 1966. Application forms and additional information may be obtained from: Fellowships Office, AAUW Educational Foundation, 2401 Virginia Avenue, N.W., Washington, D.C., 20037.

The Inter-University Committee on Travel Grants announces opportunities for advanced graduate students, faculty members, and postdoctoral reesarchers to engage in study and research in the Soviet Union, Bulgaria, Czechoslovakia and Hungary during the academic

year, 1967-68. The Inter-University Committee administers three separate exchanges with the USSR: an exchange of advanced graduate students and young faculty, an exchange of postdoctoral researchers, and an exchange of American teachers of Russian for a special ten-week program during the summer. The exchanges with Bulgaria, Czechoslovakia, and Hungary are open to advanced graduate students, faculty, and postdoctoral researchers. Participants are chosen in national competition through application and interview. Participants must have proficiency in the language of the receiving country commensurate with the needs of their programs. Applicants must be American citizens and with the exception of those applying for the summer exchange of language teachers which is open also to teachers of Russian in elementary and secondary schools, applicants must be affiliated with American universities or colleges. For additional information, write: Howard Mehlinger, Inter-University Committee on Travel Grants, 021 Lindley Hall, Indiana University, Bloomington, Indiana.

New Journals

The first issue of a new quarterly, *The Journal of Developing Areas,* will be published in September 1966. The *Journal* is intended to stimulate the descriptive and comparative study of regional development past and present with the object of promoting fuller understanding of man's relationship to the developmental process. The *Journal* is intended to be international both in its coverage and in the composition of its Board of Advisors. It will therefore welcome and consider articles from scholars in all countries. For further information, contact: The Editor, *The Journal of Developing Areas,* Western Illinois University, Macomb, Illinois 61455.

The Western Finance Association, in conjunction with the University of Washington's Graduate School of Business Administration, has announced the publication of the *Journal of Financial and Quantitative Analysis.* It is a quarterly journal and began publication with the March 1966 issue. Manuscripts should be submitted to the managing editor of the Journal: Stephen H. Archer, Graduate School of Business Administration, University of Washington, Seattle, Washington 98105.

A new journal, *The Appalachian Financial Review,* published its first issue in Spring 1966. It will be published annually, or more frequently, at the Graduate School of Business Administration, Duquesne University, Pittsburgh, Pennsylvania. The editor is Donald A. Fergusson, College of Business Administration, Syracuse University, Syracuse, New York.

Deaths

Mohiuddin Ahmed, air crash in Pakistan, February 1, 1966.

Wroe Alderson, Wharton School, University of Pennsylvania.

Arthur M. Borak, professor of economics, Khartoum, Sudan, January 24, 1966.

Melvin D. Brockie, associate professor of economics, California Institute of Technology, April 25, 1966.

Zenas C. Dickinson, professor emeritus of economics, University of Michigan, March 22, 1966.

Edward A. Duddy, professor emeritus of marketing, Graduate School of Business, University of Chicago, March 4, 1966.

Fred R. Fairchild, Knox professor emeritus of economics, Yale University, April 1966.

Joe S. Floyd, Jr., professor of finance, University of North Carolina.

Gilbert Goodman, assistant professor of economics, Wayne State University, May 9, 1966.

Kathleen C. Jackson, February 4, 1966.

Norris G. Kenny, Clearwater, Florida, March 14, 1966.

Harold King, Seton Hall, January 17, 1966.

Earl L. Knight, January 11, 1966.

James G. Lyne, January 16, 1966.

Leon C. Marshall, professor emeritus of economics, The American University.

Jean M. Massel, June 1965.

Robert G. Rice, assistant professor of business economics, University of Chicago, February 16, 1966.

William A. Salant, Chico, California, July 2, 1966.

Frank M. Surface, August 31, 1965.

John L. Tierney, Arlington, Virginia, May 17, 1966.

Retirements

Walter A. Baude, professor emeritus of statistics, University of Cincinnati, September 1, 1966.

Camille J. Botte, Ohio State University.

Elizabeth Brandeis, University of Wisconsin, June 1966.

Thomas W. Holland, professor emeritus of economics, George Washington University, September 1, 1966.

Hersel W. Hudson, professor of economics, College of Business Administration, Kent State University, June 1966.

Arthur D. Jacobsen, USAF.

William Jaffe, professor of economics, Northwestern University.

Ralph C. Jones, professor of economics, Yale University, July 1966.

Theodosi A. Mogilnitsky, professor and chairman of economics and finance, Loyola University, Chicago.

Lawrence H. Seltzer, professor of economics, Wayne State University, June 1966.

William R. Sherman, professor and head of department of economics and business administration, Hillsdale College, September 1965.

Rollin G. Thomas, professor of economics, Bowling Green State University.

Alva M. Tuttle, Ohio State University.

Jesse P. Watson, professor of economics, University of Pittsburgh, August 1966.

Wallace Wright, professor emeritus of economics, Iowa State University, May 30, 1965.

Visiting Foreign Scholars

Ramesh C. Bhardwaj, University of St. Andrews, Dundee, Scotland: visiting lecturer in economics, San Diego State College, 1966-67.

Micha Gisser, economist, Ministry of Agriculture, Israel: visiting associate professor of economics, Ohio State University.

William Gorman, Nuffield College, Oxford University: visiting professor of economics, Stanford University, spring and summer, 1966-67.

Donald M. Lamberton, University of New South Wales: visiting professor of economics, Stanford University, autumn 1966.

Staffan B. Linder, Stockholm School of Economics: visiting research associate in economics, Yale University, fall 1966.

Alan S. Milward, University of East Anglia, England: visiting associate professor of economics, Stanford University, 1966-67.

Ryoshin Minami, Hitotsubashi University: visiting research associate in economics, Yale University.

Yair Mundlak, Hebrew University: visiting associate professor of economics, University of Chicago, 1966-67.

Desmond H. O'Halloran, University of Dublin, Ireland: visiting lecturer in economics, Ohio State University.

Juan P. Perez Castillo, Universidad Central de Venezuela: visiting associate professor of economics, Tulane University, spring 1967.

C. Rangarajan, Indian Statistical Institute, New Delhi: visiting associate professor of finance, New York University, 1966-68.

T. N. Srinavasan, Indian Statistical Institute, New Delhi: visiting professor of economics, Stanford University, 1966-67.

Kenji Takeuchi, Kwansei Gakuin University, Japan: visiting assistant professor of economics, Lafayette College, 1966-67.

Brian R. Van Arkadie, Ministry of Overseas Development, British Government: visiting research staff economist, Yale University.

Promotions

Richard E. Attiyeh: assistant professor, department of economics, Yale University.

Eric N. Baklanoff: professor, department of economics, Louisiana State University.

Robin Barlow: associate professor, department of economics, University of Michigan.

Robert A. Barry: assistant professor, department of economics, College of William and Mary.

Yoram Barzel: associate professor, department of economics, University of Washington.

Joseph M. T. Becker: professor of economics, School of Commerce and Finance, St. Louis University.

Robert L. Bennett: associate professor, department of economics, University of Maryland.

Rudolph C. Blitz: professor, department of economics, Vanderbilt University.

Gordon R. Bonner: associate professor, College of Business and Economics, University of Delaware.

William C. Brainard: associate professor, department of economics, Yale University.

W. Robert Brazelton: associate professor, department of economics, University of Missouri at Kansas City.

Vernon M. Briggs: associate professor, department of economics, University of Texas.

Robert C. Brooks, Jr.: professor of business administration, Vanderbilt University.

Janet G. Chapman: associate professor, department of economics, University of Pittsburgh.

Richard V. Clemence: A. Barton Hepburn professor of economics, Wellesley College.

Richard N. Cooper: professor, department of economics, Yale University.

John Dorsey: associate professor of economics and director of the Bureau of Business and Economic Research, University of Maryland.

Barney Dowdle: associate professor of economics and forestry, University of Washington.

Frederick T. Downs: professor, department of economics, New Mexico State University.

Robert V. Eagly: associate professor, department of economics, Wayne State University.

Stanley Engerman: associate professor, department of economics, University of Rochester.

Zbigniew M. Fallenbuchl: professor, department of economics, University of Windsor.

Eugene F. Fama: associate professor of finance, Graduate School of Business, University of Chicago.

John R. Felton: associate professor, department of economics, University of Nebraska.

Belton M. Fleisher: associate professor, department of economics, Ohio State University.

John E. Floyd: associate professor, department of economics, University of Washington.

Roger N. Folsom: assistant professor, U. S. Naval Postgraduate School, Monterey, California.

Herman Freudenberger: professor, department of economics, Tulane University.

Seymour S. Goodman: associate professor, department of economics, Tulane University.

Walter Gross: professor, department of marketing, University of Georgia.

Niles M. Hansen: associate professor, department of economics, University of Texas.

W. Lee Hansen: professor, department of economics, University of Wisconsin.

Van Court Hare: associate professor, Graduate School of Business, Columbia University.

Robert Hawkins: assistant professor, Graduate School of Business Administration, New York University.

George W. Hilton: professor, department of economics, University of California, Los Angeles.

Samuel P. S. Ho: assistant professor, department of economics, Yale University.

Leon Hollerman: professor, department of economics, Claremont Men's College.

Alphonse G. Holtmann: associate professor, department of economics, Wayne State University.

Ching-Yao Hsieh: associate professor, department of economics, George Washington University.

Stephen H. Hymer: associate professor, department of economics, Yale University.

Michael D. Intriligator: associate professor, department of economics, University of California, Los Angeles.

M. Bruce Johnson: associate professor, department of economics, University of Washington.

Ronald Jones: professor, department of economics, University of Rochester.

Anne O. Krueger: professor, department of economics, University of Minnesota.

James W. Kuhn: professor, Graduate School of Business, Columbia University.

James W. Land: assistant professor, department of economics, Yale University.

Robert J. Lawrence: associate professor, department of economics, University of Texas.

Wilford L. L'Esperance: associate professor, department of economics, Ohio State University.

Harold F. McClelland: professor, department of economics, Claremont Men's College.

Rodney Morrison: assistant professor, department of economics, Wellesley College.

Walter Y. Oi: professor, department of economics, University of Washington.

Van Doorn Ooms: assistant professor, department of economics, Yale University.

Rudolph Penner: associate professor, department of economics, University of Rochester.

George E. Rejda: associate professor, department of economics, University of Nebraska.

Fred Renwick: assistant professor of finance, Graduate School of Business Administration, New York University.

Clark W. Reynolds: associate professor, department of economics, Yale University.

Karl W. Roskamp: professor, department of economics, Wayne State University.

Gian S. Sahota: associate professor, department of economics, Vanderbilt University; at University of São Paulo, Brazil, 1966-67.

Thomas R. Saving: professor of economics, Michigan State University.

Jeffrey M. Schaefer: assistant professor, department of economics, Rutgers—The State University.

Gerhard Schmidt: associate professor, Fairleigh Dickinson University.

Wiliam G. Shepherd: associate professor, department of economics, University of Michigan.

Gordon Shillinglaw: professor, Graduate School of Business, Columbia University.

Abraham Shuchman: professor, Graduate School of Business, Columbia University.

Frederick T. Sparrow: associate professor, department of political economy, Johns Hopkins University.

Robert M. Stern: professor, department of economics, University of Michigan.

William J. Stober: associate professor, department of economics, Louisiana State University.

Boris Swerling: professor of economics, Georgetown University,

Ronald L. Teigen: associate professor, department of economics, University of Michigan.

Judith G. Thornton: associate professor, department of economics, University of Washington.

George W. Ulseth: professor, department of economics, Wisconsin State University.

Ranbir Varma: professor, department of economics, Long Island University.

Harold Watts: professor, department of economics, University of Wisconsin.

James H. Weaver: associate professor, department of economics, The American University.

Burton Weisbrod: professor, department of economics, University of Wisconsin.

Jacob Weissman: professor, department of economics, Hofstra University.

Joseph T. White: assistant professor, department of economics, Georgetown University.

Marina Von N. Whitman: associate professor, department of economics, University of Pittsburgh.

John H. Wicks: associate professor, department of economics, University of Montana.

Dean A. Worcester: professor, department of economics, University of Washington.

Administrative Appointments

Wiliam R. Allen: vice chairman, department of economics, University of California, Los Angeles.

Robert F. Barlow, dean, Whittemore School of Business and Economics: academic vice-president, University of New Hampshire.

Thomas R. Beard: head, department of economics, Louisiana State University.

Michael J. Brennan: dean, Graduate School, Brown University.

Richard E. Caves: chairman, department of economics, Graduate School of Arts and Sciences, Harvard University.

Sanford Cohen: faculty director, International Manpower Institute, U.S. Department of Labor, summer 1966.

Charles Crapo: director, Worker Education Program and assistant professor, department of economics, University of Maine.

Herman Freudenberger: acting chairman, department of economics, Tulane University, 1966-67.

C. Edward Galbreath: chairman, department of economics, George Washington University.

Abraham L. Gitlow: acting dean and professor of economics, School of Commerce, New York University.

Bela Gold: Timken professor of industrial economics and director, research program in industrial economics, Case Institute of Technology.

Bernard Hall: dean and professor of economics, College of Business Administration, Kent State University.

Carl Kaysen, professor, Harvard University: director, Institute for Advanced Studies, Princeton University.

G. E. Kiser: acting head, department of marketing, University of Arkansas, 1966-67.

Harvey Kiser: director of research, National Grain and Feed Dealers' Association.

Ben T. Lanham, Jr., assistant dean, College of Agriculture: vice president for research, Auburn University, July 1, 1966.

Leland C. Lehman: chairman and professor, department of economics, Macalester College, September 1965.

Cyril C. Ling: director, Management Center and associate professor of business administration, University of Richmond.

Leo M. Loll, Jr.: dean, College of Business, Economics and Government, University of Alaska, April 1, 1966.

Bevars D. Mabry: chairman and professor, department of economics, Bowling Green State University, September 1966.

Edward Marcus: chairman, department of economics, Brooklyn College, July 1, 1966 and president, Metropolitan Economic Association.

John M. Montias: director of graduate studies, department of economics, Yale University.

B. B. Myers: head, department of commerce, College of Arts, Science and Technology, Kingston, Jamaica.

Frederick G. Reuss: chairman and professor, department of economics, Goucher College.

Richard Rosett: chairman, department of economics, University of Rochester.

Wilson E. Schmidt: head and professor, department of economics, Virginia Polytechnic Institute.

David Schwartzman: chairman, graduate faculty, New School for Social Research.

Warren C. Scoville: chairman, department of economics, University of California, Los Angeles.

Sherman Shapiro: coordinator of economics University of Illinois, Chicago.

Michael Sherman: assistant to dean and director of admissions, Graduate School of Business, Columbia University.

John R. Tabb: vice-president for development and planning, Old Dominion College.

Hans B. Thorelli: chairman, department of marketing, Graduate School of Business, Indiana University.

Ralph N. Traxler, Jr., associate professor, Emory University: dean, School of Business Administration, University of South Alabama.

Gerald E. Warren: head, department of economics, DePauw University.

Harold W. Watts: director, National Institute for Research on Poverty, University of Wisconsin.

Appointments

Henry Aaron, Harvard University: Council of Economic Advisers, July 1966.

Charles Abercrombie, University of Illinois: assistant professor of marketing, University of Arkansas.

Dale Adams, University of Wisconsin: staff member, department of agricultural economics and rural sociology, Ohio State University.

Earl Adams: assistant professor, department of economics, University of Pittsburgh.

Irma Adelman: professor, department of economics, Northwestern University.

Ralph S. Alexander: visiting professor of economics, Rutgers—The State University.

Clopper Almon, Jr., Harvard University: associate professor of economics, University of Maryland.

Stuart H. Altman: assistant professor of economics, Brown University.

William H. Anderson: professor of economics, Claremont Men's College, September 1966.

James W. Angell, Columbia University: visiting professor, department of economics, City University of New York, fall 1966 and visiting professor, New School for Social Research, 1966.

Dennis R. Appleyard: lecturer, department of economics, University of North Carolina.

Johan Arndt: assistant professor of marketing, Graduate School of Business, Columbia University.

Wallace N. Atherton, University of California, Davis: associate professor, department of economics, California State College, Long Beach.

Henry G. Aubrey: visiting professor of economics, Columbia University, fall 1966.

Michael B. Ayre: senior economist, F. W. Dodge Company, a division of McGraw-Hill, Inc.

Jarvis Babcock, University of Michigan: assistant professor of economics, Wayne State University.

J. Allison Barnhill, University of Washington: assistant professor, department of economics and commerce, Simon Fraser University.

Lowell Bassett, Purdue University: assistant professor of economics, University of Washington.

Donald V. T. Bear, University of Chicago: associate professor, department of economics, University of California, San Diego, July 1965.

J. R. Becklin, Indiana University: assistant professor of economics, New Mexico State University.

Bernard R. Bell: assistant director, economics of the projects department, International Bank for Reconstruction and Development.

Yoram Ben-Porath: assistant professor, department of economics, University of Chicago.

George J. Benston, University of Chicago: associate professor, University of Rochester.

Elliot Berg, Harvard University: visiting associate professor, department of economics, and research associate, Center for Research on Economics Development, University of Michigan, spring 1966.

Robert E. Berney: assistant professor of economics, College of Economics and Business, Washington State University.

Cicely W. Blanco: assistant professor of economics, Georgetown University.

Aryeh Blumberg: visiting lecturer in economics, Vanderbilt University.

Douglas R. Bohi: instructor in economics, College of Economics and Business, Washington State University.

Ben W. Bolch: assistant professor of economics, Vanderbilt University.

August C. Bolino: associate professor, The Catholic University of America.

Thomas Borcherding, Duke University: assistant professor of economics, University of Washington.

John D. Bowman: visiting assistant professor of economics, University of Wisconsin.

J. Hayden Boyd, University of Minnesota: assistant professor of economics, Ohio State University.

G. Knight Boyer: instructor in economics, U.S. Air Force Academy.

Stanley E. Boyle: professor, department of economics, College of Business, Virginia Polytechnic Institute.

Michael E. Bradley: assistant professor, department of economics, Pennsylvania State University.

Lawrence Braslow, University of Maryland: assistant professor, department of economics, California State College, Long Beach.

Frank P. R. Brechling: professor of economics, Northwestern University.

Andrew F. Brimmer, U.S. Department of Commerce: member, Board of Governors, Federal Reserve System, March 1966.

Joe E. Brown: professor of economics, University of Missouri at Kansas City.

Philip R. Brown: instructor in accounting, Graduate School of Business, University of Chicago, October 1, 1966.

Earl Brubaker: assistant professor of economics, University of Wisconsin.

Karl Brunner, University of California, Berkeley: Reese professor of economics and banking, Ohio State University.

William Bryan: project analyst and research economist, Ohio Office of Appalachia.

Joseph M. Burns: acting assistant professor, department of economics, University of California, Los Angeles.

John F. Burton, Jr., Yale University: associate professor of industrial relations and public policy, Graduate School of Business, University of Chicago.

J. Douglas Campbell: assistant professor, College of Business and Economics, University of Delaware.

Richard E. Capel, North Carolina State University: assistant professor, department of agricultural economics, University of Manitoba.

Phillip G. Carlson, professor, School of Business Administration, Emory University.

Deane C. Carson: professor of banking and finance, Graduate School of Business Columbia University.

Thomas J. Casey, Jr., Tulane University: visiting assistant professor, department of economics, Louisiana State University, 1966-67.

John Chang, University of Michigan: assistant professor, department of economics and business, Lafayette College.

Barry R. Chiswick: acting assistant profesor, department of economics, University of California, Los Angeles.

Renato Cirillo, The Catholic University of America: associate professor of economics University of Alberta.

Allan R. Cohen, Harvard Graduate School of Business Administration: visiting professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

Jon S. Cohen: lecturer, department of economics, Yale University.

Sanford Cohen, University of Michigan: professor, department of economics, University of New Mexico.

Francis X. Colaço: assistant professor of economics, Brown University.

Lawrence P. Cole, Purdue University: assistant professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

Apostolos Condos, Iowa State University: assistant professor, department of economics, Duke University.

Alfred H. Conrad, Harvard University: professor, department of economics, City University of New York.

Sydney Coontz, University of Utah: associate professor, department of economics and commerce, Simon Fraser University.

Andre B. Corbeau, Tulane University: visiting assistant professor, department of economics, Louisiana State University, 1966-67.

Judith Cox: acting assistant professor of economics, University of Washington.

Curtis A. Cramer, University of Maryland: assistant professor of economics, University of Wyoming.

Robert L. Crouch: visiting associate professor, department of economics, Northwestern University, 1966-67.

William E. Cullison: assistant professor, department of economics, Virginia Polytechnic Institute.

John Cunnyngham, Columbia University: associate professor of economics, Ohio State University.

Herman E. Daly, Louisiana State University: Ford Foundation visiting professor of economics, University of Ceara, Brazil, 1966-67.

Mark R. Daniels: assistant professor of economics, Brown University.

John Darling, University of Illinois: lecturer, department of marketing, School of Commerce and Business Administration, University of Alabama.

Paul Davidson: professor of economics and associate director of the Bureau of Economic Research, Rutgers—The State University.

Wayne J. Davis: instructor in economics, Rutgers—The State University.

Robert D. Dean, University of Pittsburgh: assistant professor of economics, College of Business Administration, and research associate, Center for Urban Regionalism, Kent State University.

Paul G. Demeny, Princeton University: associate professor, department of economics and research associate, Population Studies Center, University of Michigan.

Joel S. Demski: assistant professor of accounting, Graduate School of Business, Columbia University.

Calvin L. DePass: assistant professor, department of economics, University of New Mexico.

Antal Deutsch, Queen's University: associate professor, McGill University.

Peter A. Diamond: associate professor, department of economics, Massachusetts Institute of Technology.

Stanley Diller: assistant professor of economics, Graduate School of Business, Columbia University.

Michael R. Dohan, Massachusetts Institute of Technology: assistant professor of economics, Division of Humanities and Social Sciences, California Institute of Technology.

Albert E. Drake, University of West Virginia: professor, department of quantitative methods, School of Commerce and Business Administration, University of Alabama.

Emmanuel Drandakis, Yale University: associate professor, department of economics, University of Rochester.

Michael R. Edgmand: assistant professor, department of economics, Oklahoma State University.

Franklin R. Edwards: assistant professor of banking and finance, Graduate School of Business, Columbia University.

Robert Ekelund, Jr.: instructor, department of economics, Louisiana State University.

Richard A. Elnicki: lecturer, department of economics, Yale University.

M. Jarvin Emerson: chief economist and director, Kansas Office of Economic Analysis.

John L. Enos: fellow and university lecturer, Magdalen College, Oxford.

Gene L. Erion: professor, department of economics, University of Alaska.

Mary L. Eysenbach: assistant professor of economics, University of Washington.

Noel J. J. Farley: assistant professor, department of economics, Goucher College.

George R. Feiwel, University of Alberta: professor of economics, University of Tennessee.

Thomas P. Ference: assistant professor of behavioral science, Graduate School of Business, Columbia University.

Alfred J. Field, Jr.: lecturer, department of economics, University of North Carolina.

Mary Fish, West Texas State University: assistant professor, department of economics, School of Commerce and Business Administration, University of Alabama.

Anthony C. Fisher: assistant professor of economics, Brown University.

Walter Fisher: visiting professor of economics, Northwestern University, winter and spring, 1966-67.

Thomas G. Fox: assistant professor, department of economics, Pennsylvania State University.

Seymour Friedland, Claremont Graduate School, visiting professor of finance, Graduate School of Business Administration, New York University, 1966-67.

Mark R. Garff, University of Washington: assistant professor of economics, University of Wyoming.

David Geithman: visiting assistant professor of economics, Tulane University.

Peter J. Ginman: instructor, department of economics, Boston University, 1966-67.

Errol Glustoff, Stanford University: acting assistant professor of economics, University of Washington.

Mal Golden, Santa Clara University: assistant professor, School of Business Administration, York University, Toronto, September 1966.

Bernard Gordon: consultant, Charles River Associates, Cambridge, Massachusetts.

Amor Gosfield, University of California, Santa Barbara: professor of economics, California State College, Dominguez Hills.

Henry G. Grabowski: lecturer, department of economics, Yale University.

Douglas H. Graham: assistant professor of economics, Vanderbilt University.

Malcolm Gray, University of Aberdeen: visiting professor, department of economics and commerce, Simon Fraser University.

Carl C. Greer: assistant professor of banking and finance, Graduate School of Business, Columbia University.

David M. Grether: research staff economist and lecturer, department of economics, Yale University.

Sam Gubins: lecturer in economics, Goucher College.

Warren C. Hall, Jr., Ohio University: assistant professor of economics, Illinois Institute of Technology.

Robert S. Hamada: instructor in finance, Graduate School of Business, University of Chicago.

Paul B. Han: visiting associate professor of industrial management, Graduate School of Business, University of Chicago, autumn quarter 1966.

William G. Harris, University of Oregon: instructor in economics, Georgetown University.

Robert H. Haveman: associate professor of economics, Grinnell College.

Edward Herman, State University of New York, Plattsburgh: associate professor of economics, College of Business Administration, University of Cincinnati, September 1, 1966.

Yhi-Min Ho: visiting assistant professor of economics, Tulane University.

James O. Horrigan, University of Notre Dame: assistant professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

David B. Houston: associate professor, department of economics, University of Pittsburgh.

Teh-Wei Hu: assistant professor, department of economics, Pennsylvania State University.

David S. Huang: professor of economics, Southern Methodist University.

Kai-Loo Huang: professor of economics and business administration, Moravian College.

Norman S. Hubbard: assistant professor, department of economics, Brooklyn College.

Donald L. Huddle, Rice University: visiting research staff economist, department of economics, Yale University.

Jonathan R. T. Hughes: professor, department of economics, Northwestern University.

Archer Huneycutt, Southwest Missouri State College: associate professor of marketing, University of Arkansas.

Manley R. Irwin: associate professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

John Iton: research assistant professor, department of economics, University of Pittsburgh.

Paul E. Ivory, University of California, Berkeley: assistant professor, department of economics and commerce, Simon Fraser University.

Lamar B. Jones: assistant professor, department of economics, Virginia Polytechnic Institute.

Joe Jones: associate professor of general business, and associate director, Bureau of Business and Economic Research, University of Arkansas.

Pamela H. Kacser, American University: visiting lecturer, department of economics, Columbia University, 1966-67.

C. Harry Kahn: associate director, Bureau of Economic Research and professor of economics, Rutgers—The State University.

Hiromitsu Kaneda, University of California, Davis: visiting research staff economist, department of economics, Yale University.

Robert M. Kaplan: assistant professor, department of economics and commerce, Simon Fraser University.

Richard D. Karfunkle: economist, Textile Fibers Department, E. I. du Pont de Nemours and Company, Inc.

Mohamad W. Khouja: assistant professor, department of economics, Oklahoma State University.

Wade R. Kilbride: assistant professor of economics, U.S. Air Force Academy.

Larry J. Kimbell: acting assistant professor, department of economics, University of California, Los Angeles.

David Klahr: instructor in industrial administration, Graduate School of Business, University of Chicago.

Panos Konstas: visiting assistant professor of economics, Oklahoma State University.

John J. Korbel, University of Wisconsin: associate professor of economics and business administration, Whittemore School of Business and Economics, University of New Hampshire.

Helen C. Kramer, Brooklyn College: assistant professor, Stony Brook College.

James R. Krum: assistant professor, College of Business and Economics, University of Delaware.

William E. Kuhn, Roosevelt University: professor, department of economics, University of Nebraska, 1966-67.

Howard Kunreuther: assistant professor of applied economics, Graduate School of Business, University of Chicago.

Paul W. Kuznets, University of Indiana: visiting research staff economist, department of economics, Yale University.

Gerald M. Lage: assistant professor, department of economics, Oklahoma State University.

Charles Lamphear, Kansas State University: assistant professor, department of economics, University of Nebraska, 1966-67.

William Landes, Stanford University: assistant professor, department of economics, University of Chicago.

Harold Q. Langenderfer, University of North Carolina: visiting professor of accounting, Duke University, first semester 1966-67.

Gurcharan S. Laumas, Wayne State University: assistant professor of economics, College of Business Administration, Kent State University.

James W. Leasure: associate professor, department of economics, University of North Carolina.

Maw Lin Lee: associate professor, department of economics, Pennsylvania State University.

Susan J. Lepper: research associate and lecturer, department of economics, Yale University.

Louis Levine: professor, department of economics, Pennsylvania State University.

Darrell R. Lewis, Luther College: visiting assistant professor, department of economics, Louisiana State University, 1966-67.

John Lindauer: associate professor of economics, Claremont Men's College.

Peter Lindert: assistant professor of economics, University of Wisconsin.

Paul W. Lindloff, Jr., University of Houston: visiting associate professor of accounting, Duke University.

Wesley H. Long: assistant professor, department of economics, Pennsylvania State University.

Victor M. Longstreet: senior member, International Management and Marketing Group, a division of Newsome & Company, Inc., Boston.

Gene Lynch, Texas Christian University: associate professor of banking and finance, University of Arkansas.

Donald Market, Texas Christian University: assistant professor of finance, University of Arkansas.

Patrick A. Martinelli: instructor in economics, U.S. Air Force Academy.

William F. Matlack: assistant professor of economics, Claremont Men's College.

John J. McCall: associate professor of management science, Graduate School of Business, University of Chicago, April 1, 1966.

Joan M. McCrea, Hollins College: assistant professor of economics, Arlington State College, University of Texas.

William McElroy: instructor in economics, Georgetown University.

John McGee: visiting professor, department of economics, University of Washington.

Joseph P. McKenna: professor of economics, College of Business, Virginia Polytechnic Institute.

Charles A. Mecimore, University of Alabama: assistant professor, department of accounting, University of Alabama.

Paul Medow: associate professor, New School for Social Research.

Dileep R. Mehta: assistant professor of banking and finance, Graduate School of Business, Columbia University.

David R. Miller: assistant professor of economics, Syracuse University.

Merton H. Miller: Edward Eagle Brown professor of banking and finance, Graduate School of Business, University of Chicago.

Bridger M. Mitchell: assistant professor of economics, Stanford University.

Glen H. Mitchell, Southern Illinois University: professor of business administration and economics, New Mexico State University.

Morris E. Morkre, Northwestern University: instructor, department of business and economics, Illinois Institute of Technology.

Donald G. Morrison: assistant professor of marketing, Graduate School of Business, Columbia University.

Randall I. Mount, Purdue University: assistant professor of economics, College of Business Administration, Kent State University.

Robert A. Mundell: professor, department of economics, University of Chicago.

Bernard Munk: assistant professor, department of economics, University of Michigan.

John M. Munro, Indiana University: assistant professor, department of economics and commerce, Simon Fraser University.

Tracy W. Murray: assistant professor, department of economics, University of New Mexico.

Seiji Naya: assistant professor of economics, University of Wisconsin.

Jürg Niehans: professor of political economy, Johns Hopkins University.

Fred Nordhauser: instructor in economics, U.S. Air Force Academy.

Vahid F. Nowshirvani: research staff economist and lecturer, department of economics, Yale University.

Ben Okner, President's Council of Economic Advisers: assistant professor of economics, Ohio State University.

Tsvi Ophir: visiting associate professor of accounting and business economics, Graduate School of Business, University of Chicago, 1966-67.

Daniel Orr, University of Chicago: associate professor, department of economics, University of California, San Diego.

Thomas J. Orsagh: assistant professor, department of economics, University of North Carolina.

Joseph A. Pechman, Brookings Institution: Irving Fisher visiting research professor, department of economics, Yale University.

Sam Peltzman, University of California, Los Angeles: research fellow, National Bureau of Economics Research, 1966-67.

Jerry Petr, Indiana University: assistant professor, department of economics, University of Nebraska, 1966-67.

S. James Press: associate professor of statistics, Graduate School of Business, University of Chicago, April 1, 1966.

Aline O. Quester: instructor, department of economics, Boston University, 1966-67.

David L. Rados: assistant professor of marketing, Graduate School of Business, Columbia University.

Muhammad A. Rahman, University of Hawaii: visiting research associate, department of economics, Yale University.

Denis T. Raihall: assistant professor, College of Business and Economics, University of Delaware.

Kadiyala K. Rao: assistant professor, department of economics, Wayne State University.

J. F. Reichert, Royal Military College: instructor, department of economics and commerce, Simon Fraser University.

L. Winston Ring: assistant professor of marketing, Graduate School of Business, Columbia University.

R. Richard Ritti: associate professor of behavioral science, Graduate School of Business, Columbia University.

Alice Rivlin, Brookings Institution: deputy assistant secretary for program coordination, Department of Health, Education and Welfare.

Kong Kyun Ro: visiting assistant professor, department of economics, University of Pittsburgh.

Sidney E. Rolfe: visiting professor, New School for Social Research.

Paul Roman: assistant professor, department of economics, St. Louis University.

Murray N. Rothbard: associate professor, department of economics, Polytechnic Institute of Brooklyn.

Jerome Rothenberg: professor, department of economics, Massachusetts Institute of Technology.

Thomas J. Rothenberg, Northwestern University: acting associate professor, department of economics, University of California, Berkeley.

Dick Rottman, Arizona State College: associate professor of insurance, University of Arkansas.

Nancy Ruggles, Yale University: research staff member, National Bureau of Economic Research.

Richard Ruggles, Yale University: research staff member, National Bureau of Economic Research.

Emiko Sakurai, University of Iowa: assistant professor of management, University of Arkansas.

Joan Sato: assistant professor, department of economics, University of New Mexico.

Richard Schramm: assistant professor of banking and finance, Graduate School of Business, Columbia University.

Carl C. Schwan, Jr., Vanderbilt University: research department, Industrial Nucleonics Corporation, Columbus, Ohio.

Stuart O. Schweitzer: assistant professor, department of economics, Wayne State University.

Thomas T. Sekine, University of Birmingham: assistant professor, department of economics and commerce, Simon Fraser University.

Donald E. Sexton, Jr.: assistant professor of operations research, Graduate School of Business, Columbia University.

David L. Shapiro: acting assistant professor, department of economics, University of California, Los Angeles.

Edward Shapiro, University of Detroit: associate professor, department of economics, Wayne State University.

Judith C. Shapiro, London School of Economics: assistant professor of economics, University of Washington.

Ansel M. Sharp, Oklahoma State University: economic advisor, Ministry of Planning and Development, Kenya, Africa.

Lawrence H. Shaw: assistant professor of economics, Vanderbilt University.

Arthur S. Shenfield: visiting professor of economics, Graduate School of Business, University of Chicago, autumn quarter 1966.

James Sherbaniuk, University of Manitoba: assistant professor, department of economics and commerce, Simon Fraser University.

Jagdish N. Sheth: assistant professor of marketing, Graduate School of Business, Columbia University.

Joseph Shister, State University of New York at Buffalo: member of the New York State Board of Mediation.

Robert D. Shriner, University of Oklahoma: research associate, Division of Business and Economic Research, University of Wyoming.

Ammar Siamwalla: research staff economist, department of economics, Yale University.

Alvin J. Silk: assistant professor of marketing, Graduate School of Business, University of Chicago.

Larry D. Singell, Bowling Green State University: assistant professor, department of economics, Wayne State University.

Neil M. Singer, assistant professor of economics, University of Maryland.

Arthur A. Sloane: associate professor, College of Business and Economics, University of Delaware.

Alan Sorkin: lecturer, department of economics, Goucher College.

Jean-Robert Spichiger: assistant professor in economics, University of Puerto Rico.

Robert N. Stearns, Yale University: instructor in economics, Ohio State University.

Frank G. Steindl, Oklahoma State University: visiting research economist, Federal Reserve Bank of Cleveland, 1966-67.

Matthew A. Stephenson: assistant professor, department of economics, College of Business, Virginia Polytechnic Institute.

Daniel E. Strayer: instructor in economics, U.S. Air Force Academy.

W. Craig Stubblebine: associate professor of economics, Claremont Men's College.

J. Dirck Stryker: research staff economist and lecturer, department of economics, Yale University.

Sam Sydneysmith, North Carolina State University: assistant professor, department of economics and commerce, Simon Fraser University.

Russel A. Taussig: associate professor of accounting, Graduate School of Business, Columbia University.

Phillip Taylor, Lamar State College of Technology: assistant professor of general business, University of Arkansas.

Henry Thomassen: associate professor of economics, Emory University.

Edgar H. Thornber: research associate, Graduate School of Business, University of Chicago, July 1, 1966.

George S. Tolley, North Carolina State College: professor, department of economics, University of Chicago.

John F. Trainor: instructor in economics, Washington State University.

A. Dale Tussing: assistant professor of economics, Syracuse University.

Dan Usher: assistant professor of economics, Graduate School of Business, Columbia University.

J. F. Vallery, Jr., University of Tennessee: lecturer, department of economics, University of Alabama.

John M. Vernon: assistant professor, department of economics, Duke University.

Hugh D. Walker, Vanderbilt University: assistant professor of economics, Indiana University.

Gary Walton, University of Washington: assistant professor of economics, Ohio State University.

Leon Wegge: department of economics, University of California, Davis.

William D. Wells: professor of psychology and marketing, Graduate School of Business, University of Chicago.

Harold R. Williams, University of Nebraska: assistant professor of economics, College of Business Administration, Kent State University.

T. David Williams, Northwestern University: lecturer in economics, University of Malawi.

Charles R. Wolf: assistant professor of banking and finance, Graduate School of Business, Columbia University.

Robert J. Wolfson: professor of economics, Syracuse University.

Henry W. Woudenberg, Jr., Michigan State University: assistant professor of economics, College of Business Administration, Kent State University.

Kunio Yoshihara: assistant professor, department of economics: University of Michigan.

John P. Young, Division of Finance, Department of State: visiting professor of economics, Claremont Men's College, 1966-67.

David B. Zenoff: assistant professor of international business, Graduate School of Business, Columbia University.

John A. Zerby: lecturer, department of economics, University of Sydney, Australia.

Leaves for Special Appointments

Kenneth J. Arrow, Stanford University: visiting professor of economics, Massachusetts Institute of Technology, autumn 1966-67.

Werner Baer, Vanderbilt University: professor, Faculty of Economics, University of São Paulo, Brazil, 1966-67.

Donald C. Bartholomew, University of Missouri at Kansas City: program economist, Seoul, Korea, 1966-67.

Barbara Bergmann, University of Maryland: Administration for International Development, 1966-67.

Edward C. Budd, Pennsylvania State University: Office of Business Economics, U.S. Department of Commerce, 1966-67.

Carl F. Christ, Johns Hopkins University: visiting professor, University of Essex, England, 1966-67.

Joseph S. Chung, Illinois Institute of Technology: Fulbright lecturer, Seoul National University, Korea, 1966-67.

Roger Dehem, Laval University: visiting professor, department of economics, Queen's University, autumn 1966-67.

William G. Dewald, Ohio State University: consultant, Reserve Bank of Australia, Sydney.

James E. Dugan, University of Colorado: Fulbright lecturer, Liberia, 1966-67.

Robert A. Flammang, Louisiana State University: visiting professor of economics, National University of the South, Bahia Blanca, Argentina, spring 1967.

Walter Galenson, University of California, Berkeley: visiting professor of economics and industrial and labor relations, Cornell University, 1966-67.

Ronald B. Gold, Ohio State University: Harvard University project, AID, Chilean Ministry of Finance.

Seymour S. Goodman, Tulane University: visiting associate professor of economic development, University of Istanbul, Turkey, spring 1967.

Douglas H. Graham, Vanderbilt University: professor of economics, Vargas Foundation, Rio de Janeiro, 1966-67.

Gerald K. Helleiner, University of Tonorto: senior research fellow and director, Economic Research Bureau, University College, Dar es Salaam, Tanzania, 1966-67.

William F. Hellmuth, Oberlin College: research professor and director, Economic Research Bureau, University College, Dar es Salaam, Tanzania, February-August 1966.

Bert Hoselitz, University of Chicago: visiting professor, University of California, Santa Cruz, winter quarter 1966-67.

Paul Luey, University of Hong Kong: visiting fellow, department of economics, Research School of Pacific Studies, Australian National University, June to November 1966.

Roland N. McKean, University of California; Los Angeles: visiting professor, Harvard University, second semester.

George G. S. Murphy, University of California, Los Angeles: visiting senior lecturer, University of Colchester, Essex, 1966-67.

J. Carter Murphy, Southern Methodist University: special field staff, Rockefeller Foundation, Bangkok, Thailand, 1966-67.

Sam Peltzman, University of California, Los Angeles: National Bureau of Economic Research, New York, 1966-67.

Joseph D. Phillips, University of Illinois: Harvard Advisory Group, Buenos Aires, 1966-68.

David Robinson, University of Arkansas: Fulbright lecturer, Finland, 1966-67.

Marvin E. Rozen, Pennsylvania State University: senior research adviser, Pakistan Institute of Development Economics, 1966-68.

Gian S. Sahota, Vanderbilt University: professor of economics University of São Paulo, Brazil, 1966-67.

Frederic C. Shorter, Princeton University: social science consultant, Ford Foundation in Turkey.

Robert F. Smith, Louisiana State University: curriculum director, American Institute for Free Labor Development program for South American labor specialists, Loyola University, 1966-67.

Anthony M. Tang, Vanderbilt University: visiting professor of economics, Chinese University of Hong Kong, Vanderbilt Overseas Professorship Program.

Milton C. Taylor, Michigan State University: fiscal adviser to the government of Nigeria, research and graduate instruction, University of Ibadan, 1966-67.

Joseph M. Thorson, Southern Illinois University: to organize a business school at the University of Liberia, under the auspices of Cornell University's AID program, 1966-68.

Oliver E. Williamson, University of Pennsylvania: special economic adviser to the Assistant Attorney General.

Resignations

Richard Ablin, State University of New York at Buffalo.

David S. Ball, Vanderbilt University.

John M. Brazzel, U.S. Air Force Academy, December 1965.

Willard Carleton, New York University.

Robert S. Carlson, Graduate School of Business, Columbia University.

Eaton H. Conant, Graduate School of Business, University of Chicago, August 31, 1966.

Gerald L. Duskin, Rutgers—The State University.

Daniel O. Fletcher, Ohio State University.

Harold W. Fox, Rutgers—The State University.

Peter G. Franck, Robert College, Istanbul.

Lewis P. Freitas, Graduate School of Business, Columbia University.

Donald F. Gordon, University of Washington.

David G. Greene, Rutgers—The State University.

Randall B. Haydon, Kent State University, August 1966.

Claude Hillinger, State University of New York at Buffalo.

Charles T. Horngren, Graduate School of Business, University of Chicago, June 30, 1966.

Eugene Lerner, Graduate School of Business Administration, New York University.

Meno Lovenstein, Ohio State University.

W. David Maxwell, Tulane University.

Jora R. Minasian, State University of New York at Buffalo.

Wayne L. Mock, Graduate School of Business, Columbia University.

A. Monroe Moore, Rutgers—The State University.

Richard F. Muth, Graduate School of Business, University of Chicago.

Dominic F. O'Keefe, School of Commerce, New York University, September 1, 1966.

Edward F. Renshaw, University of North Carolina.

Rubin Saposnik, State University of New York at Buffalo.

Ira O. Scott, Jr., Graduate School of Business, Columbia University.

George B. Simmons, Graduate School of Business, Columbia University.

William Swift, Temple University.

J. Van Fenstermaker, Kent State University, July 1966.

Theodore Voyadjis, State University of New York at Buffalo.

Miscellaneous

Jacob Marschak, emeritus professor of economics and business administration, University of California, Los Angeles, recalled to service, 1966-67.

Dudley F. Pegrum, emeritus professor of economics, University of California, Los Angeles, recalled to service, 1966-67.

SIXTY-THIRD LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1966. Abstracts of many of the dissertations are supplied. This list excludes theses undertaken in the same period.

General Economics; Methodology

Lawrence A. Boland, Ph.D. Illinois 1966. On the methodology of capital theory.

Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

AHMAD M. AL-SAMARRIE, Ph.D. George Washington 1966. Inequality of the size distribution of family money income: interstate comparison.

The dual aims of this study are to explain interstate variations in income inequality in 1949 and 1959, and to analyze the resulting changes in income concentration during this period. Income inequality is measured by Gini's concentration ratio, as based on the Lorenz Curve. Income refers to pretax money income received by families in each of the 48 states of the continental United States and the District of Columbia, published in the *United States Census of Population*, 1950 and 1960. Among the ten explanatory variables tested in a cross-section analysis, using a linear multiple-regression correlation model, five are found to be significantly related to the regional variations in income concentration: property income as per cent of total personal income; the proportion of total income originating in agriculture; the level of education measured by the median school years completed by adult persons; the proportion of nonwhite persons in state population; and the extent of labor participation in the state economy measured by per capita civilian employment. There was a tendency for the states to become more similar in the size distribution of their family incomes during the 'fifties, and this convergence phenomenon is associated with comparable movements in several other key state economic and demographic variables.

HIROSHI ATSUMI, Ph.D. Rochester 1966. The efficient capital accumulation in open models.

It is proved that a balanced growth path with maximal sustainable consumption has the normative significance that, if society maximizes the sum of per capita utility over time, then every optimal path converges to this balanced path. Open-growth models with labor force explicit, such as a neoclassical model, Mrs. Robinson's simple model, and a dynamic Leontief model, are used in the analysis.

RICHARD E. ATTIYEH, Ph.D. Yale 1966. Estimation of a fixed coefficients vintage model of production.

ALBERT BOOKBINDER, Ph.D. Fordham 1966. Money and ex ante investment as determinants of aggregate demand.

RONALD BRITTO, Ph.D. Brown 1966. Linear programming and the theory of investment.

ANTONIO CAMACHO, Ph.D. Minnesota 1965. Alternative concepts of informational efficiency in resource allocation mechanisms.

ANTHONY S. CAMPAGNA, Ph.D. Rutgers—The State University 1966. Expectations and the investment function.

An empirical study attempts to integrate expectations into an investment function via a model of investment appropriations as opposed to the more usual investment expenditures. Cross-section firm data of capital appropriations provided by the National Industrial Conference Board for the years 1956 and 1961 for seven industries are used to examine statistically the importance of selected financial variables in explaining capital appropriations.

M. V. CHARI, Ph.D. New York (Buffalo) 1966. Technical progress, production functions and economic growth.

The existing literature on one-sector growth models appears to suggest that unless technical progress is Harrod-neutral, it is impossible for the economy to exhibit steady proportionate growth in the long run. The present research attempts to investigate the shape and form of aggregate production function which would enable the system to exhibit steady proportionate growth even when the conditions of Harrod neutrality are not satisfied. Its equilibrium characteristics and the economic implications are critically evaluated. The analysis is extended to include the possibilities of nonexponential growth in technology and labor force.

GILBERT A. CHURCHILL, Ph.D. Indiana 1966. Location and its relation to output: a sensitivity analysis.

BRUNO CONTINI, Ph.D. Carnegie Inst. Technology 1966. Models of restricted bargaining for organizations with multiple objectives.

This concerns the design of a resource allocation decision-making scheme for organizations that have partially conflicting objectives. The particular method examined is a restricted bargaining approach which yields an unambiguous solution with the following properties: (1) it is reached by spontaneous agreement among the group members; (2) it is Pareto-optimal; (3) it guarantees a lower bound to each individual's utility. The basic idea of the method is to provide incentive for all group components to engage in a bargaining process that consists of making concessions from an initial set of demands until agreement is reached. A mathematical model is formulated and examined in terms of its implications for organizational behavior and problems of market equilibrium in an exchange economy.

JOHN A. COUGHLAN, Ph.D. Catholic 1965. The contributions of Frank Albert Fetter (1863-1949) to the development of economic theory.

MICHAEL DAVENPORT, Ph.D. Pennsylvania 1965. Corporate savings behavior: a cross-section analysis.

MICHAEL EISNER, Ph.D. Wisconsin 1965. William Morris Leiserson: economics in action.

MOHAMED EL-HODIRI, Ph.D. Minnesota 1966. Optimality criteria of economic growth as examples of optimal resource allocation over time.

A micro-economic model of growth is formulated. Optimality here means maximizing a functional whose arguments are the integral of an instantaneous utility function over time and (variable) terminal time and stocks, subject to feasibility. Under concavity and independence optimality implies the existence of a price-rental path such that producers always maximize instantaneous profits. This is shown by using ancient results from calculus of variations. Existence of an optimal program is also established.

JAMES P. GANDER, Ph.D. California (Berkeley) 1966. Economics of the growth of the firm.

MALCOLM S. GOLDEN, Ph.D. Claremont 1966. A review of major themes in the structural unemployment hypothesis: an empirical investigation of the disaggregated manufacturing sector, 1948-1960.

The postwar behavior pattern of production-worker employment in U.S. manufacturing reveals a diverse pattern. One group of industries has been relatively stagnant or has experienced an irregularly declining employment trend; another group has achieved an upsurge in employment. The object of this study is to seek an explanation for that employment diversity by analyzing the three major arguments by advocates of structural unemployment: (1) technological change; (2) demand transformation; and (3) market power.

RICHARD GONCE, Ph.D. Wisconsin 1966. The development of John R. Commons system of thought.

MICHAEL J. GOOTZEIT, Ph.D. Purdue 1966. The Corn Laws and Ricardian economics: a modern treatment.

NILS H. HAKANSSON, Ph.D. California (Los Angeles) 1966. Optimal investment and consumption strategies for a class of utility functions.

ROBERT J. HALL, Ph.D. Iowa 1966. A multi-stage model of the firm.

ROBERT S. HOLBROOK, Ph.D. California (Berkeley) 1965. Alternative models of consumer behavior: the permanent income hypothesis and the life cycle hypothesis.

YOUNG P. JOUN, Ph.D. Washington 1966. The demand for air travel.

A theory of demand incorporating time and money budget constraints is formulated to estimate airline passenger demand. An estimating model is constructed and least-squares estimates are made utilizing both time series and cross-section data. Price elasticity is found to be greater than one, and income elasticity to be close to unity.

JOHN G. KEANE, Ph.D. Pittsburgh 1965. Analysis of United States beer demand 1935-1964 with emphasis on 1945-1961.

CHULSOON KHANG, Ph.D. Minnesota 1965. A neoclassical growth model of a resource-poor open economy.

BILLY F. KIKER, Ph.D. Tulane 1965. The concept of human capital in the history of economic thought.

To measure the wealth-producing factor "human capital" requires a unit of measurement and since most economic values are measured in terms of money, a number of economists are assigning monetary values to human beings. They are, moreover, attempting to determine precisely what investment activity comprises human-capital formation, and to set forth an analytical framework within which the concept may be investigated. A survey of the literature reveals that this concept is not new. Beginning with William Petty, the literature has been investigated to determine primarily who considered human beings as capital and the motives and procedures for valuing them in monetary terms. An attempt has been made to sketch the history of the philosophy underlying such evaluation.

FENG Y. LEE, Ph.D. Michigan State 1965. Econometric analysis of the demand for basic living materials in Japan.

RICHARD E. LEVITAN, Ph.D. Harvard 1966. Demand in an oligopolistic market and theory of rationing.

ROLF R. MANTEL, Ph.D. Yale 1966. Toward a constructive proof of the existence of equilibrium in a competitive economy.

THOMAS H. MAYOR, Ph.D. Maryland 1965. The sources of decline in the United States capital-output ratio, 1869-1958.

The real aggregate capital-output ratio from 1869-1958 was reasonably stable except for a relatively brief interval after the 1920's when the ratio fell significantly. Neither capital-saving technological progress nor changes in the supply of saving—two popular hypotheses—appears able to explain the decline. More important are changes which affected the user cost of capital—increases in the relative price of capital goods, in the rate of depreciation, and especially in the tax rate.

JOHN L. MCENTAFFER, Ph.D. Boston College 1965. A theoretical and statistical analysis of the effects of changes in income distribution in the U.S. on the consumption function.

The thesis attempts to measure the impact of changes in family income distribution since 1950 on several recently fitted quarterly consumption functions. Eight measures of income distribution were added as final independent variables to three aggregate consumption functions closely related to the absolute and relative income hypotheses. Some income distribution variables significantly reduce the unexplained variation in consumption. The thesis also contains a review of aggregate consumption theory and early attempts to measure and illustrate income distribution changes.

ALI I. MEDANI, Ph.D. California (Berkeley) 1965. Constant vs. nonconstant production elasticities among firms.

YASUO MURATA, Ph.D. Stanford 1965. Embodied technical progress in a multisectoral economic growth model.

This dissertation extends the substitution theorem to a dynamic system where capital goods are classified not only in terms of kind but also in terms of vintage, assuming that each industry has a Cobb-Douglas type of production function. Then the existence of a unique balanced growth of production is proved both in abstraction from technical change and in case of technical progress.

KEITH PHILLIPS, Ph.D. Washington 1965. Some tests of simple aggregate expenditures models.

This study utilizes modern econometric methods to test the predictive power of a naive quantity theory as compared to a simple expenditures model of aggregate economic activity. On the basis of the evidence the former is clearly preferable.

J. EUGÈNE POIRIER, Ph.D. Yale 1966. Planning economic growth capital-labor substitution and foreign-aid.

JOEL POPKIN, Ph.D. Pennsylvania 1965. Interfirm difference in direct-investment behavior of U. S. manufacturers.

JOHN E. ROYLE, Ph.D. Western Reserve 1966. Short-run cost and supply curves in automation and implications for the theory of the firm.

A presentation of short-run cost behavior in automation, and a consideration of the industrial conditions of automated production. The consequences of automation on pricing and competition are also discussed.

EYTAN SHESHINSKI, Ph.D. Mass. Inst. Technology 1966. Essays on the theory of production and growth.

CALVIN D. SIEBERT, Ph.D. California (Berkeley) 1966. A micro-econometric study of fixed capital investment behavior.

MARVIN R. SNOWBARGER, Ph.D. Michigan 1966. An interaction analysis of consumer durable expenditures.

PHILIP E. SORENSEN, Ph.D. California (Berkeley) 1966. Edgeworth on monopoly, taxation, and international trade.

MATTHEW A. STEPHENSON, Ph.D. Tulane 1965. The role of technological change in the Enlish Classical school of economics.

This study examines the view of the English Classical economists from Adam Smith to John Stuart Mill on technological change and the movement of the economy toward a stationary state where capital accumulation and population growth cease. Ricardo's view that technology did not prevent historically diminishing returns to labor and capital in agriculture is compared with the emphasis on technology of the other Classicists.

DAVID W. STEVENS, Ph.D. Colorado 1965. Capital as a determinant of economic growth: allocation in a tri-ethnic community

IAN A. STEWART, Ph.D. Cornell 1966. A quarterly econometric model of the Canadian economy 1951-1962.

After a review of the actual events of the period and of the literature pertaining to the workings of fluctuating exchange rate systems a model containing 51 endogenous variables is estimated by two-stage least-squares. Impact multipliers are derived from the model and a test run of the model's ability to generate the course of endogenous variables over a thirteen quarter intra-sample period. Simulations are run of various alternative policies over the 1958 to 1961 period.

VINCENT TARASCIO, Ph.D. Rice 1966. Pareto's methodological approach to economics: a study in the history of some scientific aspects of economic thought.

The purpose of this study is to establish the place of Vilfredo Pareto in the contemporary methodological issues of his time, and to evaluate his contribution toward making economics more scientific. The major conclusion of this study is that the economics profession has, to a remarkable extent, adopted Pareto's methodology. This contribution has been implicitly recognized in evaluations of Pareto's work but has not heretofore been accorded the explicit attention it deserves.

MAU S. TSAI, Ph.D. Southern Illinois 1966. Models of optimal bond investment decision under uncertainty.

For each bond in the portfolio, the author determines the quantities to buy, to sell and to hold in each of a sequence of time periods. The models are dynamic quadratic programming models. The possible objectives of the investor are specified, the initial prices and coupon rates on the bonds are assumed known and so are the future prices and their probabili-

ties. After the optimal quantities to buy, to sell and to hold are found, the author discusses the properties of the solutions.

ANDREW G. VERZILLI, Ph.D. Boston College 1965. Explanation of net new appropriations for expenditures on new plant and equipment for total manufacturing.

This thesis tests the hypothesis that variables which have been used to explain actual expenditudes on new plant and equipment can be used to explain the variation in net new capital appropriations. The period tested is 1953 through 1962.

THAYER H. WATKINS, Ph.D. Colorado 1965. A generalized naive model for the variables of the national income accounts.

Economic History; Economic Development; National Economies

MANUEL AVILLA, Ph.D. Pennsylvania 1965. Interregional inequalities and economic underdevelopment.

JOHN D. BOYD, Ph.D. Washington 1966. Technological change, population growth and the distribution of income between developed and underdeveloped countries.

This study attempts to provide empirical content for a formal theoretical analysis of the effect of growth on the terms of trade. It finds, under reasonable assumptions about real world parameters, that a world-wide increase in real output of one per cent will result in an increase of real income of no less than .9 of one per cent in the underdeveloped nations.

MARIO S. BRODERSOHN, Ph.D. Harvard 1966. A multiregional input-output analysis of the Argentine economy.

MUWAFFAK C. CHALLAH, Ph.D. Oklahoma 1965. Economic development and planning in Syria, 1950-1962.

JOHN H. CHAPMAN, Ph.D. Colorado 1965. The effects of trade pattern changes on regional input-output analysis.

CHRISTOPHER K. CLAGUE, Ph.D. Harvard 1966. Economic efficiency in Peru and the United States.

FRANCISCO DE NADAL, Ph.D. Boston College 1966. The industrial development corporation, a case study: the National Institute of Industry in Spain.

JORGE FREYRE, Ph.D. Yale 1966. External and domestic financing in the economic development of Puerto Rico; analysis and projections.

ALAN G. GREEN, Ph.D. Harvard 1965. Regional aspects of Canada's economic growth.

JOSÉ S. GUTIERREZ, Ph.D. Iowa (Ames) 1966. Regression analysis of cross-section survey data for planning an evaluation of economic development programs.

RICHARD HYSE, Ph.D. New York 1966. Economic factors in the unification of sovereign states.

JACK L. KNUSEL, Ph.D. Colorado 1965. An analysis of German development aid.

YOON T. KUARK, Ph.D. Minnesota 1966. A comparative study of economic development between South and North Korea during the post-Korean war period.

The economic development contrasts between free South and communist North Korea are examined by comparing the growth rates of national income, industrial outputs, capital formation and finance, and by comparative performances in individual industries covering agriculture, mining, manufacturing, electric power, transportation, education and foreign trade. Also included are analyses of production functions of individual industries (South Korea alone), sources and uses of foreign aid and the comparison of standards of living.

PONG S. LEE, Ph.D. Yale 1966. The doctrine of balanced growth: with special reference to Japan, 1878-1918.

MEIR MERHAV, Ph.D. New School 1966. Technological dependence, monopoly and the limits to capitalist growth.

DAVID J. MISHKIN, Ph.D. Illinois 1966. The American colonial wine industry: an economic interpretation.

EDWARD H. MOSCOVITCH, Ph.D. Mass Inst. Technology 1966. Urban-rural investment allocation in Venezuela.

FRANCIS MWIHIA, Ph.D. Pittsburgh 1965. Decision rules for optimal tax policy with illustrations from Kenya's experience as to their practical application.

PHILIP A. NEHER, Ph.D. Brown 1966. Multisector models of economic growth with applications to the Latin American experience.

The single sector, neoclassical model of economic growth is extended to the small open economy which can trade securities at a fixed rate of interest. Interest payments and receipts are accounted for, so domestic product and national income are separately distinguished. Their ratio converges in the limit if certain stability conditions are met. Self-excitation of a borrowing-repaying-lending sequence is a consequence of certain imbalanced growth states.

SE-HARK PARK, Ph.D. Washington (St. Louis) 1965. An analysis of regression estimators for urban employment multipliers and their application to the employment impact of the aerospace industry in the St. Louis SMSA.

The primary purpose of the study is to develop a framework for urban employment multiplier analysis, and to estimate, through multiple regression and correlation differential employment impacts on the St. Louis SMSA economy resulting from monthly variations in the activity levels of major export industries and the area investment sector during the period of January 1958 to June 1964.

RICHARD W. PARKS, Ph.D. California (Berkeley) 1965. An econometric model of Swedish economic growth, 1861-1955.

RAMANBHAI C. PATEL, Ph.D. Southern California 1966. Applications for stochastic processes to economic development.

BARRY W. POULSON, Ph.D. Ohio State 1965. Value added in manufacturing, mining, and agriculture in the American economy from 1809 to 1839.

This study may be viewed as a first step toward a comprehensive measure of the growth of the American economy from 1809 to 1839. Because of the nature of the data the study is confined to the three principal commodity-producing sectors—manufacturing, agriculture, and mining. The measure of performance used is value added which is defined as the value of output to producers' prices less the value of materials and fuels directly consumed in production at delivered prices.

RICHARD T. PRATT, D.B.A. Indiana 1966. Theory of income generation in a region.

ROBERT W. RAYNSFORD, JR., Ph.D. Harvard 1966. The German experience with development aid, 1956-1965.

THEODORE P. SCHULTZ, Ph.D. Mass. Inst. Technology 1966. The distribution of personal income: case study The Netherlands.

BHEKH, B. THAPA, Ph.D. Claremont 1966. Planning for development in Nepal: a perspective for 1965-80.

Economic development is a recent phenomenon in Nepal, introduced after the successful revolution in 1951. Isolation from the rest of the world prior to this event left most Nepalese unaware of the changes that had taken place outside their borders over the past century. This dissertation represents an effort to fill the need for a long-run perspective plan for Nepal, within which short-run plans may be formulated.

ARLON R. TUSSING, Ph.D. Washington 1966. Employment and wages in Japanese industrialization: a quantitative study of Yamanashi Prefecture in the Meiji era.

This study provides the first comprehensive figures on employment and wages for an important part of Japan, 1868-1908. It develops a superior method for estimating total labor force in a preindustrial society and relates intensive use of labor force to growth.

HUGH E. URBANTKE, Ph.D. Houston 1966. The United East India Company in the seventeenth century: a twentieth century prototype.

The writer describes the history of the United East India Company in relation to the

Dutch economic environment of the seventeenth century. In examining the early developme of the Dutch East India Company, he reviews the international trade potentials of the si teenth century; the economic analytical techniques; economic analysis and economic institu tions; and existing managerial, financial and productive market structures.

DONALD R. WELLS, Ph.D. Southern California 1965. The United States unemployment prob lem: structural or lack of aggregate demand.

CHIOU-SHUANG YAN, Ph.D. Purdue 1966. Technical change and investment.

YI-CHANG YIN, Ph.D. Columbia 1966. Agricultural reorganization of mainland China, 1949 1957: its process and effects on agricultural performance.

MAHMOOD A. ZAIDI, Ph.D. California (Berkeley) 1966. A study of wages, unemployment prices, profits and productivity in Canada, 1923-39 and 1947-62.

Statistical Methods; Econometrics; Social Accounting

WILLIAM R. BAILEY, Ph.D. George Washington 1966. An appraisal of input-output analysi based on a documentation of the interindustry relations study for 1947.

The 1947 input-output program of the federal government is thoroughly documented, an the estimates derived from the various models developed during the program are publishe for the first time. The quality of the estimates is analyzed in terms of the structure and th empirical content of the models, and this analysis is integrated with other important analyse of the input-output method. Finally, it is shown that the effectiveness of a general model o national income formation can be enhanced by incorporation with an input-output model.

JERALD R. BARNARD, Ph.D. Iowa (Ames) 1965. Design and use of social accounting system in state developing planning.

This study is concerned with the development of a comprehensive information system a the state and regional levels for private and public planning. A social accounting system wa developed for the state of Iowa in the form of a 70-sector matrix to measure the real an financial transactions in connection with the production, consumption, accumulation an regional trading processes. The model includes detailed current and capital account transac tions for industry, households, and government, and a system of classification converters tha depict the functional relationships among the data to provide an integrated and comprehen sive system of real and monetary flows for evaluation, analysis, and information productio for planning purposes.

LOWELL R. BASSETT, Ph.D. Purdue 1966. The scope of the "correspondence principle" in a qualitative environment.

A. WAYNE CORCORAN, Ph.D. New York (Buffalo) 1966. An integrated system of data analysis for allocation problems.

YOEL HAITOVSKY, Ph.D. Harvard 1965. Estimation of multivariate statistics from grouped and missing data.

ASATOSHI MAESHIRO, Ph.D. Michigan 1965. Comparison of the forecasting properties of a large econometric model fitted by alternative statistical techniques.

EUSTACE G. PANAS, Ph.D. Harvard 1966. Recent developments in social accounting.

ROBERT J. PAUL, Ph.D. Arkansas 1966. Some quantitative techniques for determining performance evaluation standards for nonrepetitive activities.

THOMAS J. ROTHENBERG, Ph.D. Mass. Inst. Technology 1966. Econometric estimation with a priori information.

JUGAL K. SHARMA, Ph.D. California (Berkeley) 1965. The application of Monte Carlo methods to the evaluation of small sample properties of 3-stage least-squares procedure.

The Monte Carlo method is used to evaluate small sample properties of OLS, 2SLS, and 3SLS estimators for eleven variations of a three equation simultaneous equation model. The evaluation criteria include bias, standard deviation and root mean squared error of estimation. In some cases the ranking of various methods is inconsistent with those of previous

studies. This may be due to differences in models, error specifications, sample sizes or number of replications.

LAWRENCE A. SHERR, Ph.D. Michigan 1966. The value of the delayed call provision: a decision-making model.

MANUEL SIGUENZA, Ph.D. Colorado 1965. Mathematical economics. A methodological evaluation.

JAMES, D. SMITH, Ph.D. 1966. The income and wealth of top wealth holders in the United States, 1958.

CHARLES G. STALON, Ph.D. Purdue 1966. Double counting bias in the wholesale price index.

TSAUH AN SU, Ph.D. Rutgers 1966. The applicability of a dynamic input-output model to an expanding economy.

HRISHIKESH D. VINOD, Ph.D. Harvard 1966. Some techniques for studying economic interdependence.

HELEN W. H. YIN, Ph.D. Columbia 1966. The industrial statistics reporting system of Communist China: 1949-1958.

RICHARD ZIND, Ph.D. Southern California 1965. An estimate of some basic parameters of the Moroccan economy.

Economic Systems; Planning and Reform; Cooperation

MRINAL K. DATTA-CHAUDHURI, Ph.D. Mass. Inst. Technology 1966. Planning in a multiregional economy: a case study for India.

SALWA A. S. FARGHALI, Ph.D. Southern California 1965. Planning under socialism and risk.

AHMED S. ISPAHANI, Ph.D. Southern California 1966. The optimization of economic resources for economic growth in Iran.

ABDUL F. M. KANDEEL, Ph.D. Southern California 1966. The "surplus" approach for project appraisal (an application to the Aswan High Dam).

DAVID A. KENDRICK, Ph.D. Mass. Inst. Technology 1966. Programming investment in the steel industry.

FASSIL G. KIROS, Ph.D. Southern California 1966. A multidimensional interpretation of comprehensive developmental planning.

MICHAEL J. LAVELLE, Ph.D. Boston College 1966. The Soviet image of the United States economy: 1953-1963.

This study presents the Soviet image of American business cycle history, the U.S. growth record, and the American experience in the utilization of labor and capital. A secondary purpose has been to show changes that have taken place in the Soviet image in the past ten years. Beginning in 1958, the need felt by Soviet economists to have a measure of their own economy, led to fuller use of U.S. statistics and concern for factual comparisons between the U.S. and Soviet economies.

FRED D. LEVY, Ph.D. Yale 1966. Economic planning in Venezuela.

DEBORAH D. MILENKOVITCH, Ph.D. Columbia 1966. Pricing and the market in Yugoslav economic thought.

Business Fluctuations and Forecasting

CHARLES McV. BECKER, Ph.D. Arizona 1966. A comparision of specific cycles in United States copper output with reference cycles.

Specific cycles in U.S. copper output since World War II were measured essentially by the method developed by the National Bureau of Economic Research. These specific cycles were then compared with the NBER reference cycles as to timing, duration, amplitude, secular influence and conformity. A fairly high degree of similarity was found.

MURIEL J. CONVERSE, Ph.D. Michigan 1965. An econometric study of residential construction expenditures in the U.S.

MALCOLM DOWLING, Ph.D. Pittsburgh 1965. An industry model of inflation.

CHARLES G. DRAKE, Ph.D. Missouri 1965. Unemployment insurance as an automatic stal izer.

In an examination of contributions, benefits, total and taxable wage data for cyclical c(formity the benefit series show consistently better cyclical conformity than the contribut series. Unemployment insurance is a reasonably satisfactory automatic stabilizer in terms timing and direction of changes of values but has significant limitations in relative magnitu

MICHAEL KERAN, Ph.D. Minnesota 1966. Monetary policy and the business cycle in Jap.

This study tests statistically, with the use of difference equations, the idea that if inter: and external instability are consistent (e.g., internal inflation associated with external d(cits), one policy tool can move the system simultaneously toward reducing external and i ternal fluctuations. This study shows if external imbalance is due to internal factors, a response mechanisms operate with lags, monetary policy directed at reducing external insi bility will actually increase fluctuations, as in Japan's case. If policy is directed internal fluctuations would be reduced.

ANA MARTIRENA-MANTEL, Ph.D. Yale 1966. A model of economic fluctuations.

GORDON R. SPARKS, Ph.D. Michigan 1965. An econometric analysis of the residential buildi cycle.

ATSUSHI SUZUKI, Ph.D. Purdue 1965. Business-cycle measurement.

KENNETH F. WALLIS, Ph.D. Stanford 1966. Some econometric problems in the analysis inventory cycle.

Money, Credit and Banking; Monetary Policy; Consumer Finance and Mortgage Credit

LESLIE M. ALPERSTEIN, Ph.D. Pittsburgh 1965. A re-evaluation of commercial banking bo rowing from the Federal Reserve.

ROBERT T. AUBEY, Ph.D. California (Los Angeles) 1965. Mexico: a study of the financi relationship between the government and the private sector of the economy.

JOHN W. BAY, Ph.D. Boston College 1966. Time lapse and cyclical changes in commerci bank investment portfolios, 1949-1961.

In recent years there has been considerable controversy in the literature, both theoretic and empirical, over how commercial banks adjust their portfolios cyclically. The purpo of this dissertation is to examine the manner in which banks adjusted their portfoli(cyclically from 1949 to 1961. Special emphasis is placed on the passage of time as a methc of adjusting investment portfolios. The findings of the study show that the passage of tin is an important part in bank adjustments during expansions and recessions. Changes i maturities of government securities due to the passage of time are an alternative to, or supplement to purchases and sales of securities.

BEN W. BOLCH, Ph.D. North Carolina 1966. A spectral analysis of interrelationships betwee the money stock and certain asset markets.

Spectral and correlation analysis are applied to several economic time series representin percentage rates of change in a measure of the money stock (currency plus demand deposit adjusted), a measure of commodity prices, a measure of production, a measure of equit prices, a measure of income velocity of money, and several distinct measures of yields o various types of interest-bearing assets. The period of the analysis extends from April 195 to March 1965, and all series are sampled at semimonthly intervals. The aim of the stud is to estimate leads, lags, and degrees of association between these series on both the tim and frequency domains.

RICHARD L. BOLSTER, Ph.D. American 1966. The relationship of monetary policy to th stock market: the experience with margin requirements.

Margin requirements control entry into the stock market on a credit basis. In nearly al instances margin requirement increases have been followed by a halt in the build-up o security credit, a decrease in the rate of build-up, or a decline of security credit. Margin re quirement reductions have been followed in nearly all cases by an increase in security credit

a decrease in the rate of decline, or a halt in the decline. A similar trend in stock prices has usually followed somewhat later.

DAVID BOND, Ph.D. Yale 1966. The effects of a change in the ceiling rates on deposits at commercial banks.

JAMES A. CACY, Ph.D. California (Berkeley) 1966. The commercial bank's demand for marketable assets.

JAMES W. CHRISTIAN, Ph.D. Texas 1966. An approach to the analysis of the differential effects of monetary policy.

BRUCE C. COHEN, Ph.D. Purdue 1966. Bank deposit growth by ownership type, 1951-1961.

ROBERT L. COMEAU, Ph.D. Brown 1966. Financial intermediaries and Canadian monetary policy.

ALBERT H. COX, Ph.D. Michigan 1965. Interest on deposits of commercial banks in the United States: a study in financial regulation.

JEROME C. DARNELL, D.B.A. Indiana 1965. Chain banking as a form of banking organization.

E. E. DARROW, Ph.D. Ohio State 1965. The structure of the agricultural long-term credit market in Ohio.

The purpose of this study is to gain insight into the structure of the agricultural long-term credit market in Ohio. Three characteristics—seller concentration, product differentiation, and barriers to entry—are used to analyze structure. It is concluded that this market tends to be oligopolistic. The Markov chain process is used to project future industry volume for each market subgroup. The projection indicates this market will become even more concentrated in the future.

ADOLFO DIZ, Ph.D. Chicago 1966. Money and prices in Argentina, 1935-1962.

AARON J. DOUGLAS, Ph.D. Stanford 1966. Studies in monetary dynamics.

JOHN A. DOUKAS, Ph.D. Purdue 1966. A model of commercial banking system behavior: an econometric predictive test.

DOUGLAS FISHER, Ph.D. Chicago 1965. A British test of recent developments in term structure theory.

KATHARINE H. HSIAO, Ph.D. Columbia 1966. Money in Communist China: an analysis of structure and policy, 1953-1957.

DAVID T. HULETT, Ph.D. Stanford 1966. A short-run model of commercial bank portfolio behavior.

The portfolio behavior of nine large New York City banks in the markets for securities and borrowed reserves was investigated within the framework of a three-week first difference model. Deposits and business loans are assumed to be exogenously determined; the banks predict portfolio stability from the composition of deposits. Stable deposits were allocated to long-term securities with a lag, large interest rate elasticities were found, and the banks adjusted rapidly to changes in their desired portfolios.

EDWARD A. K. KALIAN, Ph.D. Columbia 1966. Money and banking in Iraq, 1914-1918: a study in economic history.

RONALD A. KRIEGER, Ph.D. Wisconsin 1965. Inflation propagation in Argentina: a short-run dynamic analysis.

Propagating factors that perpetuate the process of inflation in Argentina are investigated through the formulation and solution of a dynamic open-economy model that features a free-market "rural" sector and an administered-price "urban" sector. Quantitative and institutional evidence demonstrate the relevance of the assumptions of the theory to Argentine reality for the period 1958-64. Tests performed on an econometric version of the model indicate a strong probability that the postulated linear relationships indeed exist.

CHARLES M. LINKE, D.B.A. Indiana 1966. Interest rate regulation on commercial bank deposits: its evolution and impact in the State of Indiana.

PATRICK J. LYNCH, Ph.D. Purdue 1966. Commercial bank portfolio behavior.

Helen F. McHugh, Ph.D. Iowa (Ames) 1965. Differentials in uses of consumer credit by young urban families.

A random sample of 92 couples, married from one to three years, was interviewed in Des Moines, Iowa, for its uses of consumer credit in establishing households. Differentials in uses of consumer credit by these couples are investigated for possible relationships to social and economic characteristics. The couples' credit orientations are studied as they relate to time, space and material aspects of living. Each such element is correlated with the composite variable, socioeconomic status.

Donald McLeod, Ph.D. Columbia 1966. Some aspects of the development of the market for Canadian treasury bills, 1953-1962.

Paul A. Meyer, Ph.D. Stanford 1966. Regional interest rate differentials and the structure of the banking industry.

John R. Morris, Jr., Ph.D. Purdue 1966. The relationship between monetary growth and real growth in India 1951-1964.

Edward S. Pearsall, Ph.D. Princeton 1966. An econometric study of financial markets.

This study investigates the applicability of functional models of supply and demand to the explanation of behavior in the vast network of financial markets in the real world. The flow-of-funds data of the Federal Reserve Board, covering 13 sectors of the economy and 17 financial market instruments, are used for a period from 1962 to 1964. Three models are distinguished; perfect competition, limited availability of credit (lender predomination), and forced loans (borrower predomination). The author specifies and estimates some 72 supply and demand equations.

Richard H. Puckett, Ph.D. Maryland 1966. The permanent income wealth approach to the demand of money; analytical and empirical aspects.

Vladimir Salyzyn, Ph.D. Illinois 1966. The competition for personal savings deposits in Canada.

Robert J. Saunders, Ph.D. Kentucky 1965. Commercial bank profitability: a statistical study.

Stuart G. Schmid, Ph.D. California (Los Angeles) 1965. A theory of trade credit.

Gloria Shatto, Ph.D. Rice 1966. Corporate demand for liquid financial assets, 1947-62.

The study focuses on postwar changes in the pattern of corporate holdings of liquid financial assets, factors which affect such holdings, and implications for policy which the findings suggest. The main hypothesis is that the relative decline in the corporate demand for money was caused by the growth in quantity and quality of money-substitutes, higher costs of money resulting from tighter monetary policy, expectations of increasing economic stability, the growth of trade credit, and managerial and technological developments within firms which resulted in more efficient utilization of money.

Samuel L. Skogstad, Ph.D. Washington (St. Louis) 1965. Effects of money substitutes on the flow of money income: a theoretical and empirical evaluation.

Lawrence B. Smith, Ph.D. Harvard 1966. The postwar Canadian residential mortgage market and the role of government.

Donald P. Stegall, D.B.A. Indiana 1965. Commercial bank savings deposit management for maximum profit.

Edward J. Stevens, Ph.D. Yale 1966. Deposits at savings and loan associations.

Fred M. Struble, Ph.D. Colorado 1965. An analysis of the term structure of interest rates controversy.

Francis X. Thiel, Ph.D. Fordham 1966. The demand for savings and loan shares.

Helmut F. Wendel, Ph.D. Columbia 1966. Short-run interest rate expectations in the government securities markets.

Colin Wright, Ph.D. Chicago 1966. Saving and the rate of interest: an empirical investigation.

The author derives a consumption function which includes wealth, expected income, and

the rate of interest by assuming that individuals act as though they maximized an n period utility function, containing current and future consumption as well as wealth, subject to a budget constraint containing the present value of present and future income. He argues that the coefficient of the interest variable is an estimate of the substitution effect and finds that it is significantly negative, which implies that the compensated saving elasticity is significantly positive.

JAMES A. ZWERNEMAN, Ph.D. Notre Dame 1966. Credit unions in Massachusetts: a financial analysis.

Public Finance; Fiscal Policy

JOHN R. ALLAN, Ph.D. Princeton 1965. The income tax burden on Canadian stockholders.

The author analyzes the difference between the burden on the corporate shareholder in Canada that is imposed by the present combination of corporate income tax and individual income tax, and the burden that would be imposed if partnership treatment were substituted. Computations are made for both distributed and retained earnings and for various assumptions regarding the shifting of the business tax. The results are examined for their effects upon progressivity.

ROBERT J. ALLISON, Ph.D. Colorado 1966. The effect of taxes and transfer payments on the distribution of income: 1962.

HENRY F. ANDERSSON, Ph.D. Southern California 1966. Effects of Swedish fiscal policy and planning on economic growth and stability, 1430-1964.

ROY W. BAHL, Ph.D. Kentucky 1965. Factors associated with variations in city expenditures.

FREDERICK E. BAMFORD, Ph.D. Boston 1966. An appraisal of federal corporate income tax proposals from 1954 to 1964.

EARL S. BEECHER, Ph.D. California (Los Angeles) 1965. A study of the public indebtedness of California.

CHARLES F. BONSER, D.B.A. Indiana 1966. The Indiana corporate tax structure: impact and alternatives.

J. MARTIN CAROVANO, Ph.D. California (Berkeley) 1965. Financing the publicly controlled institutions of higher education, 1949-50.

RONALD DURANT, Ph.D. Wisconsin 1966. An analysis of the earnings dynamics of Wisconsin income taxpayers for the period 1948-1960.

It is the purpose of this thesis to investigate the dynamic aspect of the personal earnings distribution. The results include empirical estimates of the determinants of such measures of earnings change as (1) the absolute and (2) the percentage change in earnings during any one time-period. The vehicle for the analysis undertaken in this thesis was the hypothesizing and testing of a multivariate regression model which analyzed the general determinants of the earnings dynamics of married male head-of-household taxpayers in Wisconsin and in the active work force during the period 1948-1960.

DONALD L. ELLICKSON, Ph.D. Wisconsin 1966. The history of land taxation theory.

The literature concerning economic rent and special land taxation is reviewed. The Physiocrats and the classical economists regarded rent of land as having qualities different from other forms of income. Their critics and successors emphasized similarities rather than differences, tending to blur the valid core in the older distinction that incomes from site values of land invited special taxation as the best example of a pure surplus, "unearned" by a specific contribution to society.

DUNCAN K. FOLEY, Ph.D. Yale 1966. Resource allocation and the public sector.

HARVEY GALPER, Ph.D. Yale 1966. The federal government expenditure process: a case study.

DHARAM P. GHAI, Ph.D. Yale 1966. Income elasticity of the Uganda tax structure.

CHRISTOPHER GREEN, Ph.D. Wisconsin 1966. Transfer-by-taxation: one approach to improved income maintenance.

The dissertation is about a method of making monetary transfer payments to low-income

families through the income tax system. The first five chapters are designed to explore the history of and provide a basis for judging the relevance of transfer-by-taxation plans such as the negative income tax. The last six chapters take up some critical questions which must be considered if transfer-by-taxation is to become a practical reality.

ROBERT L. HARLOW, Ph.D. Yale 1966. Factors affecting American state expenditures.

JAMES S. H. HUNTER, Ph.D. Princeton 1966. Counter-cyclical debt policy in theory and practice.

This study deals with the use of debt management by the United States in 1950-62 for the promotion of economic stability. It reviews theories on counter-cyclical policies of debt management and concludes that economic stabilization is only one of sometimes conflicting objectives but that debt management does have a potential for influencing spending decision.

GURCHARAN S. LAUMAS, Ph.D. Wayne State 1966. The shifting of the corporation income tax: a theoretical and empirical study with special reference to India.

DANIEL W. LYNCH, Ph.D. Kentucky 1965. The development of state and local local debt in Kentucky: 1890-1962.

CHARLES E. MCLURE, JR., Ph.D. Princeton 1966. An analysis of regional tax incidences, with estimation of interstate incidence of state and local taxes.

This examines, on an analytical as well as empirical level, the incidence of taxes on an open economy, specifically the extent to which the burden of taxes imposed by one jurisdiction can be shifted elsewhere. It concludes that 20 to 25 per cent of the burden of taxes is exported, depending on whether one takes a short- or long-run view of the shifting process.

M. EUGENE MOYER, Ph.D. Wisconsin 1966. The validity of income distributions from a multi-year sample of Wisconsin income tax returns.

The thesis is a critique of a new source of data on the economic behavior of households, the Wisconsin Assets and Income Studies' sample of individuals who filed Wisconsin income tax returns during the years 1947-1959. The data are unique in that they allow an analyst to follow the economic behavior of an individual through time.

WALLACE E. OATES, Ph.D. Stanford 1965. The stabilization and tax harmonization problems.

The first part is a formal study of the stabilization problem in a federalist system with both central and subcentral government units. Using a model complete with financial-asset variables and portfolio-balance relationships, the analysis suggests that regional government bodies are likely to be severely constrained in their capacity to use conventional fiscal policy for stabilization purposes. The second part investigates tax harmonization problems between regions where there is interregional mobility of resources and consumer units.

BENJAMIN A. OKNER, Ph.D. Michigan 1965. The progressivity of the personal income tax.

JOHN M. C. OLSON, Ph.D. Southern California 1966. An analysis of fiscal policy during the Truman Administration 1945-53.

JACK W. OSMAN, Ph.D. Rutgers 1966. The determinants of interstate variations in state and local government expenditures in the United States.

DUANE W. PETTYJOHN, Ph.D. Colorado 1965. An empirical analysis of the state and local tax burden on homeowners in the community of Boulder, State of Colorado.

GEORGE B. PIDOT, JR., Ph.D. Harvard 1966. The public finances of metropolitan government in the metropolitan United States.

AYYANNA RAMINENI, Ph.D. Minnesota 1966. Comparative analysis of the Kaldor Indian tax reforms and the Shoup Japanese tax reforms.

Both reports recommended (1) the inclusion of capital gains in income with a proviso that the top bracket rate should not exceed 45 per cent, and (2) an enlargement of the personal tax base through the introduction of a net-wealth tax, an integrated general gift tax, and a personal expenditures tax recommended by Kaldor only. However, the tax measures as enacted differed considerably when each country tried to modify the proposals to fit its national goals. The Indian measures, which were aimed at achieving socialistic goals, resulted in operational inefficiency and adverse economic effects. In contrast, the Japanese measures, which were aimed toward achieving capitalistic goals, were operationally successful and had favorable economic effects.

MICHAEL K. TAUSSIG, Ph.D. Mass. Inst. Technology 1966. The charitable contributions deduction in the federal personal income tax.

ETER A. TINSLEY, Ph.D. Princeton 1966. Potential GNP and discretionary fiscal policy.

The author specifies and estimates a relatively disaggregated model of the U. S. economy fter the war on the basis of quarterly data. Special emphasis is given to the influence of lternative tax policies and to an examination of existing notions of "capital output." For is own estimates, nonlinear production functions are used. Endogenous explanations of everal types of expenditures for consumption and investment, as well as of employment, wages, and prices, are provided.

ACKIE W. WILSON, Ph.D. 1966. An empirical analysis of the determinants of the United States Treasury Bill rating, 1953-64: an institutional approach.

USTIN B. ZULU, Ph.D. Colorado 1965. An economic study of the postwar African experience with taxation, with special reference to the Republic of Uganda.

International Economics

OHN Q. ADAMS III, Ph.D. Texas 1966. Economic change, exports, and imports: the case of India, 1870-1960.

AUL S. ARMINGTON, Ph.D. California (Berkeley) 1966. Tariffs, competitiveness, and price elasticities of demand for imports—a study in the pure theory of international trade.

IORGIO BASEVI, Ph.D. Chicago 1965. International trade restrictions and resource allocation in the United States.

MILOSLAV BERNASEK, Ph.D. Wayne State 1966. Australian wage policy balance of payments and economic growth: a study of some contemporary problems.

ELEUTHERIOS N. BOTSAS, Ph.D. Wayne 1966. Economic aspects of international migration: the case of Greek labor mobility.

RALPH C. BRYANT, Ph.D. Yale 1966. Dollar balanced and the international monetary system.

ADOLFO M. CANITROT, Ph.D. Stanford 1965. Patterns of growing trade and investment in a growing economy.

E. RAY CANTERBERY, Ph.D. Washington (St. Louis) 1966. A dynamic theory of international short-term capital flows.

This study develops a dynamic theory of foreign exchange, with emphasis on short-term apital flows. Its hypothesis is that traditional foreign exchange concepts are not necessarily pplicable in determining appropriate international monetary policy. The concept of interest-ate parity is re-examined in the light of recent foreign-exchange market developments. The ew model is presented in the form of difference equations. Novel aspects in theory are developed, and contrasted with those of the conventional covered interest-rate arbitrage function. *Spot* and *forward* price-determination equations are solved simultaneously for exchange rices and for a new "interest-rate parity." Perhaps the most interesting implication of the nodel is that a policy of pegging domestic short-term interest rates will, under certain circumstances, tend to affect the forward rate in a direction opposite to that desired.

ROBERT P. CLARK, JR., Ph.D. Johns Hopkins 1966. The LAFTA debate in Venezuela: a test case in building consensus.

The dissertation deals with the policy of the Venezuelan government towards the Latin American Free Trade Association and other forms of Latin American economic integration from 1958 to 1965. Of special interest is the treatment of business-interest group behavior regarding the issue, as well as efforts by government bureaucrats to build a consensus in favor of joining the Association.

THOMAS E. DAVIS, Ph.D. Michigan 1965. An econometric model of the Canadian balance of payments.

GUY R. DECARLO, Ph.D. Maryland 1966. The effects of interest rate differentials of short-term capital movements.

STANLEY L. DOLINS, Ph.D. Colorado 1965. Economic allocation of share-costs in joint international ventures: an examination of the NATO and OECD-DAC experience.

THOMAS P. ENGER, Ph.D. Ohio State 1965. Protectionism and development.

The objective of this study is to determine whether unrestricted international trade contributes to economic development or serves as a barrier to retard development. First, a systematic examination is made of the theories and policy statements. Second, recent empirical studies on the relationship of international trade to economic development are analyzed to determine whether they support or refute points raised by dissenting economists and the United Nations. Lastly, three case studies of underdeveloped nations are analyzed. Peru, Burma, and Jamaica were selected on the basis of differences in economic structure. The analysis of each nation provides an opportunity to apply trade theory and to determine whether commercial policy has made any identifiable contribution to economic development.

MICHAEL GARDONE, Ph.D. Kentucky 1965. The international role of the dollar.

STEVEN M. GOLDMAN, Ph.D. Stanford 1966. Economic growth and international trade: a study in the theory of economic development.

The role of primary good exports in economic growth is examined within the context of a three-sector model of capital accumulation and a dynamic world market. The pattern of production and growth is then characterized with reference to the rates of growth in domestic population and world demand and under varying assumptions regarding capital intensities of the production sectors.

LOUIS G. GUADAGNOLI, Ph.D. American 1965. Role of medium-term export credit guarantees and insurance in financing economic development: Argentina as a case study.

The dissertation consists of two parts: the first considers the principles of operation of medium-term export credit guarantee and insurance programs. In the second part guaranteed medium-term export credits are evaluated as a source of financing for the developing countries and there is included a case study of Argentina's experience with such credits during the years 1960 to 1962.

HELMUT A. HAGEMANN, Ph.D. Harvard 1966. The supply and demand for international reserves.

KANJI HAITANI, Ph.D. Ohio State 1965. Japan's export trade: its structure and problems.

A comprehensive analysis is made of the problems arising from the structural changes in Japan's export trade primarily from 1953-1963. Japan's best chance to succeed in expanding its exports more rapidly, and thereby make possible a continued rapid growth of its economy, lies in its making constant and intensive efforts to: (1) improve the national competitiveness of its exports, especially of industrial machinery and passenger cars; (2) extend increasingly larger amounts of export credits at competitively low cost to underdeveloped countries; and (3) eliminate, through vigorous economic diplomacy, various restrictions imposed on Japanese exports.

WILARD E. HOHNSTEIN, Ph.D. Oklahoma 1966. The German role in the Common Market.

WON TACK HONG, Ph.D. Columbia 1966. A study of the changes in the structure of manufacturing industry and in the trade pattern of manufactured products in Korea, Taiwan, and Japan.

KYUNG MO HUH, Ph.D. Michigan 1965. The development and prospects of Japan's trade in Asia.

ASHOK KAPOOR, Ph.D. North Carolina 1966. Foreign collaborations in India: problems and prospects.

The role of foreign collaboration is to a large measure determined by the factors preventing effective utilization of transferred rights and services (and capital). Changes in business and government policies could lead to a greater inflow and better utilization of foreign rights and services. This study attempts to determine some of the specific problems of interbusiness and government-business relationships and the changes in business and government policies that are needed in order to secure greater contribution for foreign rights and services.

JOHN R. KARLIK, Ph.D. Columbia 1966. The costs and benefits of being a reserve-currency country: a theoretical approach applied to the United States.

ABDULHAY KAYOUMY, Ph.D. Washington 1965. Monopoly pricing of Afghan Karakul in international markets.

Karakul once was a virtual monopoly of Afghanistan. Its principal market is foreign. This study develops an estimating model based on standard trade theory and applies it to discover that Afghanistan reaped a small gain but lost a substantial share of its monopoly in world markets when it attempted to exploit its monopoly position. A good deal of the apparent gain was borne by domestic producers.

JAMES E. LANDES, Ph.D. Colorado 1965. Japan and the contracting parties to the General Agreement on Tariffs and Trade: a case study in postwar commercial policy.

ROGER LAWRENCE, Ph.D. Columbia 1966. Tariff preference and welfare: a study of the EEC and its French overseas associates.

JAMES L. LEIBFRIED, Ph.D. Kansas State 1965. A study of the International Wheat Agreement: its development, negotiation, and provisions.

The author presents the view that the outcome of the debate in the United States concerning the role of the International Wheat Agreement may have far reaching effects not only because of the international economic and political prominence of the United States but also because of her major role in the international wheat trade and generally strong support in the past of international commodity agreements. The purpose of this case study was to make an objective contribution to the discussion and to offer some constructive suggestions. Following a review of relevant economic and political concepts, an attempt is made to answer nine relevant questions.

CARL E. LIEDHOLM, Ph.D. Michigan 1965. An analysis of comparative advantage in the Indian iron and steel industry.

YOUNGIL LIM, Ph.D. California (Los Angeles) 1965. Export industries and pattern of economic growth in Ceylon.

LAURENCE LYNN, Ph.D. Yale 1966. An empirical analysis of United States foreign economic aid and the United States balance of payments 1954-1963.

GAIL E. MAKINEN, Ph.D. Wayne State 1966. The "pay-off" period of direct foreign investment by the United States automotive industry.

STANLEY V. MALCUTT, Ph.D. Pittsburgh 1965. The significance of governmental import quotas with special emphasis on United States trade policy.

PADMA MALLAMPALLY, Ph.D. Chicago 1965. The influence of relative prices on the exports of Germany and Japan, 1950-62.

TORLEIF MELOE, Ph.D. Columbia 1966. The United States control of petroleum imports—a study of the federal government's role in the management of domestic oil supplies.

CONSTANTINE MICHALOPOULOS, Ph.D. Columbia 1966. Trade and development in light of the Greek experience.

NORMAN C. MILLER, Ph.D. Pittsburgh 1965. Capital flows and balance of payments theory.

TURLEY R. MINGS, Ph.D. California (Berkeley) 1966. An inter-country comparison of the factors determining the cyclical behavior in Sweden, Canada, Austria, and Belgium, 1929-1937.

PETER V. MINI, Ph.D. Tulane 1965. Private long-term foreign investment in Italy, 1956-61: its causes and its contributions to Italy's economic growth.

During 1956-61 there was a ninefold increase in the annual flow of foreign private investment into Italy. The Organization for European Cooperation and Development liberalizations of capital movements and commodity trade, Italy's restrictive monetary policies, the rapid growth of its stock prices in 1959-61, and the Swiss situation of high liquidity are the most important causes of the capital inflow to Italy. Sectorally, this capital is attracted by Italy's export-oriented industries. Geographically, it is concentrated in the industrialized North. This unbalanced regional distribution runs against the postulates of the Vanoni Plan, but indirectly contributes to Southern development by encouraging labor "migration" to the North.

ADRIANUS MOOY, Ph.D. Wisconsin 1966. Primary commodity fluctuation and stabilization; an econometric analysis of the international tin stabilization scheme.

SAMUEL A. MORLEY, Ph.D. California (Berkeley) 1965. American corporate investment abroad since 1919.

JACK W. MURRAY, Ph.D. Texas 1966. The function of reserve requirements: an international survey.

VEDULA N. MURTI, Ph.D. Pennsylvania 1966. An econometric model of the world tea market.

VAN DOORN OOMS, Ph.D. Yale 1966. Regionalization and export performance: a study of primary commodities.

HSUN OU-YANG, Ph.D. New School 1966. The changes of the United States exports of manufacturers.

TERUTOMO OZAWA, Ph.D. Columbia 1966. Imitation, innovation and trade: a study of foreign licensing operations in Japan.

ERNEST J. PAVLOCK, Ph.D. Michigan 1965. A comparison of periodic income reporting among the United States, West Germany, the Netherlands and Sweden.

WILLIAM POOLE, Ph.D. Chicago 1966. The Canadian experiment with flexible exchange rates 1950-62.

ROBERT RIPPEY, Ph.D. Syracuse 1966. Terms of trade of underdeveloped countries.

JOHN T. ROWNTREE, Jr., Ph.D. California (Berkeley) 1966. The efficiency of intergovernmental grants.

DANIEL M. SCHYDLOWSKY, Ph.D. Harvard 1966. Tariffs and national income: a policy model.

EDWARD H. SHAFFER, Ph.D. Columbia 1966. The United States oil import program: an evaluation.

AHARON H. SHAPIRO, Ph.D. New School 1966. British monetary policy and the balance of payments problem 1958-1964.

GRAHAM K. SHAW, Ph.D. Columbia 1966. European economic integration and stabilization policy.

JOHN T. SPROTT, Ph.D. Colorado 1965. The external accounts and debt servicing of Brazil.

JOHN W. TANNER, Ph.D. Oregon 1966. An analysis of the effect of direct foreign investment on foreign exchange earnings and the balance of payments of the United States.

The purpose of this thesis is to investigate the impact of direct capital outflows on commodity and service exports from the United States, and to offer an improved method of estimating this impact. A central issue in the study concerns the total effect of direct investment on the foreign exchange receipts accruing to an investing country for any given period as a result of its past and present direct investment activity.

JOHN E. TILTON, Ph.D. Yale 1966. International trade patterns is nonferrous metals.

YASUO UEKAWA, Ph.D. Rochester 1966. A study of some topics in mathematical economics.

Part I proves a theorem on the existence of a solution to the linear programming problem. Part II proves theorems on existence and stability of balanced growth equilibrium with fixed land supplies and endogenous land-augmenting technical progress. Part III generalizes the factor price equalization and the Samuelson Stolper theorem to the many goods case.

ROGER B. UPSON, Ph.D. Michigan 1965. The usage of foreign capital markets by United States international companies.

JACQUES VAN YPERSELE, Ph.D. Yale 1966. Sharing the defense burden among western allies.

VITHAL S. VARTIKAR, Ph.D. Wayne State 1966. Commercial policy of India.

JAMES W. VIGEN, Ph.D. Ohio State 1965. Analysis of demand for wheat in the European Economic Community with projections to 1970 and 1975.

The wheat economy of the EEC is examined through construction of statistical demand relationships for total domestic demand, demand for wheat consumed as food, and demand for wheat utilized as livestock feed. Equations are developed for individual countries and fit to data for the period 1951-1962 by means of multiple regression technique. Projections made for the years 1970 and 1975 indicate declining per capita consumption of wheat as food and increased utilization of wheat as livestock feed.

HELEN Y. WAEHRER, Ph.D. Columbia 1966. Inter-industry skill differences, labor earnings and United States foreign trade, 1960.

DENIS W. WARE, Ph.D. Ohio State 1965. The economic implications of free trade in agricultural products between Canada and the United States.

The effect of tariff removal by Canada and the United States on 21 agricultural products is studied using least-squares multiple regression analysis. These products represent consumer items such as frozen vegetables and inputs such as corn. Estimates made for 1960 indicate that for the majority of the commodities there would have been less than a 5 per cent change in trade volume, with little impact on third countries.

HERBERT WHEELER, Ph.D. Fordham 1966. The interest arbitrage theory of forward exchange and the United States dollar 1959-63.

JOSEPH T. WHITE, Ph.D. Boston College 1966. United States direct foreign investment and host nation income levels.

The hypothesis of this thesis is that direct investment by firms intending to sell output within the host nation is a function of the Gross National Product and that host nation. Correlation and regression results indicate that such a functional relation does exist for U.S. direct investment in the manufacturing, trade and petroleum refining sectors. There is no evidence of such a relationship in most extractive (export) industries or in public utilities. The results of this thesis also show that significant institutional changes can change the parameters of this function.

H. DAVID WILLEY, Ph.D. Columbia 1966. Changes in British and German manufacturing productivity, 1951-1962.

HAROLD R. WILLIAMS, Ph.D. Nebraska 1966. Domestic economic activity and the United States balance of payments.

SALIM YASIN, Ph.D. Colorado 1966. Postwar developments in the cotton economy and the economics of an international cotton agreement.

YEONG-HER YEH, Ph.D. Minnesota 1965. Economies of scale, market size and degree of concentration in a hypothetical common market for the ECAFE region.

ABDULLAH ZARINNIA, Ph.D. Nebraska 1966. The adequacy of world monetary reserves within the period of 1964-1974.

ARVID M. ZARLEY, Ph.D. Purdue 1965. The impact of economic integration on third countries: a case study of Canadian export shares in the British market under alternative courses of European integration. Business Finance; Investment and Security Markets; Insurance.

Business Finance; Investment and Security Markets; Insurance

MUZAFFER AHMAD, Ph.D. Chicago 1965. Demand for life insurance.

The purpose of this study is to examine the influence of income and certain other variables on the purchase of life insurance. The data for 1956 and for 1962 collected by Michigan Survey Center were used. Analytic methods are simple, two-stage, and three-stage least-squares analysis. Income elasticity is suggested to be at least unity. It contradicts certain earlier findings. Tax subsidy initially shows a significant influence, though no firm conclusions are drawn. Number of income receivers and dummy for the group with unstable incomes are found to have discernible influence.

FRED D. ARDITTI, Ph.D. Mass. Inst. Technology 1966. Risk and the required return on equity.

GUILFORD C. BABCOCK, Ph.D. California (Los Angeles) 1965. A general method for measuring investment worth.

JAMES C. BAKER, D.B.A. Indiana 1966. The German securities exchange: a definitive study with evaluation.

WILLIAM H. BEAVER, Ph.D. Chicago 1965. Financial ratios as predictors of business failures.

HERMAN R. BOBBITT, JR., Ph.D. Pennsylvania 1966. Risk management in the corporation.

JEROME BRAVERMAN, Ph.D. California (Los Angeles) 1966. An analysis of credibility theory: its basis and application to casualty insurance ratemaking.

OSEPH B. BUCHWALD, Ph.D. California (Los Angeles) 1965. Life-care contracts in California.

DARWIN B. CLOSE, Ph.D. Pennsylvania 1965. Underwriting problems and opportunities for American property liability insurance companies transacting business in the major countries of the European Economic Community and Great Britain.

ROBERT H. DAINES, D.B.A. Indiana 1966. A study of investment constraints and practices of multiple life insurance companies.

TANLEY DILLER, Ph.D. Columbia 1966. Anticipations, extrapolations, and the terms structure of interest rates.

OHN R. FOSTER, D.B.A. Indiana 1966. The effect of the financing of shopping centers on occupancy by independent retailers.

ENTON E. GUP, Ph.D. Cincinnati 1966. The economics of the security option markets.

The study makes a comprehensive economic analysis of the Put and Call option markets. irst it traces the historical development of options from prebiblical times up to the present. ext it utilizes both quantitative and qualitative methods to analyze the factors determining e market price paid for Put and Call options and factors affecting the aggregate option arket from 1961-1964. Finally the profitability of security options is tested by use of a comex model and numerous portfolio simulations.

ILLIAM F. HARDIN, JR., Ph.D. North Carolina 1966. Cash management in a small manufacturing firm.

This thesis is concerned with the assessment and application of probabilities to the probns of cash management in an actual case.

ALCOLM R. HENDERSON, Ph.H. Alabama 1965. Changes in plant and equipment investment, internal funds, equity capital, and long-term debt, as related to changes in various interest rates—1949 through 1962—an empirical study.

OBERT S. HOEKE, Ph.D. Wisconsin 1966. The determinants of stock market investment purposes.

LLY HUMPHREY, Ph.D. Arkansas 1966. An evaluation of selected decision techniques in relation to the post-audit of capital expenditures.

OME C. LAYA, Ph.D. Stanford 1965. A cash flow model and the rate of return: the effect of price-level changes and others factors on book yield.

HER A. LEVY, Ph.D. Stanford 1965. Measuring capital depreciation under conditions of technological change: United States manufacturing industries 1946-1961.

RIS J. LUNESKI, Ph.D. Minnesota 1965. The theory of the investment decision: a pragnatic view.

The rate-of-return and present-value methods of evaluating alternative investment deons under various decision environments are reviewed. It is found that the rate-of-return thod possesses certain deficiencies when used in the general model, though these deficiencies not too serious. The present-value method fits the general model when all information reints are lifted. Use of a managerially determined cost-of-capital rate may be useful in h methods. Requirements for optimality in decision making can not be approached in ctice.

LLIAM F. MATLACK, Ph.D. Pennsylvania 1966. An application of a decision model for he buying of insurance by consumers.

LE D. MCFARLANE, D.B.A. Indiana 1966. Application of the Markowitz portfolio election model as a managerial tool.

HAM MCLAREN, Ph.D. Yale 1966. Retained earnings as a source of funds for corporate investment.

ICE MORGAN, Ph.D. Yale 1966. Corporate debt and the evaluation of corporate earnings.

NIEL OUNJIAN, Ph.D. Harvard 1966. Long-term public financing of small corporations -the Reg A market.

LBUR A. RAPP, Ph.D. Northwestern 1966. The role of reacquired common stock in nancial management.

VRUDHULAS K. SASTRY, Ph.D. Pennsylvania 1966. Dividends, investments and external financing behavior of the corporate sector in India: an econometric study.

Business Organization; Managerial Economics; Marketing; Accounting

ROBERT E. BOGGS, D.B.A. Indiana 1966. The effect of accounting policy decisions on reported earnings per share of common stock.

JOHN H. BRAGG, D.B.A. Indiana 1966. Managerial controls for smaller-sized dairies in Massachusetts.

ROBERT L. BRYSON, JR., D.B.A. Indiana 1965. The evolution of private-brand marketers in the petroleum industry.

HARRISON S. CAMPBELL, Ph.D. Columbia 1966. The utility of purchase options in the theory of inventory control.

GASTON CHINGARI, Ph.D. California (Los Angeles) 1966. A methodology for the simulation of numerically controlled production system.

GEOFFREY CHURCHILL, Ph.D. North Carolina 1966. Simulation tests of an intermittent scheduling heuristic model for large lot size.

The thesis examines heuristic models which will yield low cost production schedules for the large-lot intermittent production case. Inventories are also considered. System optimality is not a goal due to the combinatorial problem implied. A phenomenon given the name "interference queueing" appears, in which delays are interlocking and cumulative if operation times vary. This causes local decision rules to yield results markedly superior to the results of any schedule attempted.

DAVID W. CONRATH, Ph.D. California (Berkeley) 1965. A model of suborganizational decision making—with an application to budgeting in NASA.

VICTOR J. COOK, Ph.D. Michigan 1965. Branding behavior of the firm in appliance and tire marketing.

PETER D. COUCH, Ph.D. Wisconsin 1965. Some effects of training and experience on concepts of supervision.

WILLIAM F. DUHAMEL, Ph.D. Stanford 1966. The use of variable Markov processes as a partial basis for the determination and analysis of market segments.

LAURENCE P. FELDMAN, Ph.D. Minnesota 1965. The location of franchised retail automobile dealerships in the Twin Cities metropolitan area.

The extent to which dealership location adapts to a dynamic metropolitan market environment is examined with emphasis upon discernible patterns in the number and special distribution in a given metropolitan area and whether objective methods of appraising dealership location can be developed.

JAMES F. FILGAS, D.B.A. Indiana 1966. Management by logic: a history of Yellow Transit Freight Lines, Incorporated.

ROBERT J. FREEMAN, Ph.D. Arkansas 1966. Municipal accounting in Arkansas.

ROBERT M. FULMER, Ph.D. California (Los Angeles) 1965. Theoretical and operational implications of the product manager system in the consumer goods industry.

JOSEPH N. FRY, Ph.D. Stanford 1966. An investigation of the influence of family branding on consumer brand choice in selected frequently purchased packaged goods.

JAMES A. GENTRY, JR., Ph.D. North Carolina 1966. The economic impact and accounting treatment of investment incentives.

Investment incentives are defined as those devices in the existing tax laws of this country whose primary purpose is to stimulate capital investment. The availability of these provisions to taxpayers has economic effects on the individual taxpayer and on the economy as a whole. A system of graduated investment incentives is recommended in place of the present system. Methods of accounting for the incentives are also discussed.

EDMUND R. GRAY, Ph.D. California (Los Angeles) 1966. Organization problems in industrial research.

EDWARD L. GUFFEY, Ph.D. Arkansas 1966. An inquiry into management practices and the comparative effectiveness of family operated businesses.

DAMODAR N. GUJARATHI, Ph.D. Chicago 1965. The economics of Davis-Bacon Act.

BERNARD J. HAMILTON, Ph.D. California (Los Angeles) 1966. An investigation of the effects of job design on output and physiological costs for a strenuous task.

JACK C. HAYYA, Ph.D. California (Los Angeles) 1966. A study of the appropriate use of PERT (Program Evaluation Review Technique) in procurement contracts.

HAROLD W. HENRY, Ph.D. Michigan 1965. Long-range planning in industrial corporations: an analysis of formalized practices.

STANLEY J. HILLE, Ph.D. Minnesota 1966. Marketing research in United States Class I railroads with special emphasis on western transcontinental railroads.

Support is given to the following hypotheses: marketing research can make a significant contribution to the efficiency and profitability of railroads; formalized marketing research is more effective than marketing research diffused through the organization; railroads have some special problems which make marketing research particularly difficult to undertake; some formalized marketing research organization patterns are particularly suitable for railroads.

BERNARD L. HINTON, Ph.D. Stanford 1966. A model of creative problem-solving performance and the effects of frustration.

STEPHEN NAI-KAI HU, Ph.D. North Carolina 1966. On some aspects of the theory of the multi-product firm.

An attempt is made to offer a systematic exposition of the theory of the multi-product firm. There are a number of decision variables, such as the adaptability of the fixed factor, changing prices, and changing demands, which affect the firm's behavior. How each of these variables independently affects the firm's operation has been analyzed and a synthetic analysis of how the impacts on the firm's operation of some important variables is integrated has also been provided.

ABU BAKR A. HUSSEIN, Ph.D. New York 1965. Theory of optimum locations of inspection stations for statistical quality control.

ROBERT E. JENSEN, Ph.D. Stanford 1966. A study of effects of alternative accounting systems on security analysis and portfolio selection decisions.

ELBERT B. JOHNSON, Ph.D. Iowa 1965. Implication of changes in the sizes of establishment for the location of industrial activity.

ORACE E. JOHNSON, Ph.D. Chicago 1966. Business corporations and philanthropy: a study of why corporations give.

LLOYD KIRBAN, Ph.D. New York 1965. The role of pre-employment job orientation in voluntary termination.

PETER H. KNUTSON, Ph.D. Michigan 1965. The effect and treatment of price-level changes in the investment decisions of industrial firms.

ARTHUR LAUFER, Ph.D. California (Los Angeles) 1966. An investigation of the use of selected neuromuscular response tests as predictors of sensory motors performance in aging individuals.

ROBERT A. LENBERG, Ph.D. Minnesota 1965. Marketing in the powered pleasure boat industry: a commodity study.

This thesis is concerned with the marketing problems of family-type boats. The characteristics of the markets for such boats are considered in detail. The channels of distribution, pricing policies and practices, brand policies, and promotional methods used in the industry are analyzed.

JOSEPH LEVY, Ph.D. Chicago 1966. Optimal location on a network.

HUNTLEY G. MANHERTZ, Ph.D. Rutgers 1966. The econometrics of space and time potentials in market demand analysis—a dynamic interregional model applied to the processed vegetable industry in the United States.

The dissertation demonstrates the application of variance-covariance analysis to the

systematic isolation of the effects of specific spatially manifested variables. These variables are subsequently incorporated to generate a dynamic system of causal structural dmand, supply, and transfer functions, and serve to provide a basis for the objective evaluation of the role of space and time potentials in market demand analysis. The operational capacity of the system was investigated by tests and predictions with respect to the estimated demand and supply functions for processed vegetables.

BURTON H. MARCUS, Ph.D. Northwestern 1965. Market and product differentiation of selected magazines.

ANTHONY MASTORS, Ph.D. California (Los Angeles) 1966. An experimental investigation and comparative evaluation of production-line balancing techniques.

KENNETH R. MACCRIMMON, Ph.D. California (Los Angeles) 1965. An experimental study of the decision-making behavior of business executives.

KENNETH MCLENNAN, Ph.D. Wisconsin 1965. A study of managers in manufacturing industry: the skill and knowledge acquisition patterns and requirements of their jobs.

DAVID B. MONTGOMERY, Ph.D. Stanford 1966. A probability diffusion model of dynamic market behavior.

JOHN D. MUNDIE, Ph.D. Stanford 1966. The perfecting of marketing information for industries in a developing industrial economy with reference to the province of Manitoba.

JOHN G. MYERS, Ph.D. Northwestern 1966. Social-psychological correlates of consumer brand choice.

HENRY W. NASH, Ph.D. Alabama 1965. An analysis and evaluation of productivity and efficiency of the retail sales persons in Mississippi department stores—1963.

DOMINIC G. PARISI, Ph.D. Northwestern 1966. The impact of change in information technology on management organization, decision making, and interpersonal relations in a large insurance company.

MASON P. ROSENTHAL, Ph.D. Chicago 1965. Gasoline sales volume estimation for individual service stations.

MARTIN M. ROSNER, Ph.D. Chicago 1965. An analysis of organization influences on hospital adoption of new drugs.

KENDRITH M. ROWLAND, D.B.A. Indiana 1966. Selected determinants of effective leadership.

JOHN K. RYANS, JR., D.B.A. Indiana 1965. An analysis of appliance retailer perceptions of retail strategy and decision processes.

DALE H. SCHARINGER, D.B.A. Indiana 1965. A study of specified issues in management development.

WILLIAM F. SCHMELTZ, Ph.D. Western Reserve 1966. Accounting and management control practices in petroleum refining.

A study of accounting and managerial information in the petroleum industry, with special reference to the problem of joint processing costs.

ROGER K. SUMMIT, Ph.D. Stanford 1966. Simulation of a management-decision process utilizing a computer model of the aerospace industry.

RYLAND A. TAYLOR, Ph.D. Notre Dame 1966. The use of a sponsor's computer in a credit union operation, with a tested program for such use.

ROMAN V. TUASON, JR., Ph.D. Northwestern 1965. Experimental simulation on a predetermined marketing mix strategy.

JOSEPH C. ULLMAN, Ph.D. Chicago 1965. Inter-firm differences in the cost of search for white collar workers.

GLEN L. URBAN, Ph.D. Northwestern 1966. A quantitative model of product planning with special emphasis on product interdependency.

WILLIAM VAN MUSE, Ph.D. Arkansas 1966. An inquiry into the possible relationships between the success of a social fraternity and adherence to basic business management principles.

REDERICK A. WEBSTER III, Ph.D. California (Berkeley) 1965. A theory of product diversification strategy with selected case histories.

ARY G. WILLIAMS, Ph.D. Stanford 1966. *Image perception heterogeneity and attribute relevance as indicators of consumer affective response in a service industry.*

ATRICK M. WILLIAMS, Ph.D. California (Los Angeles) 1966. An evaluation of barriers to the delegation of authority in formal organizations.

WIGHT J. ZULAUF, Ph.D. Minnesota 1965. Accounting analysis and the ability to pay wages.

The term "(in)ability to pay" has not been effectively or uniformly defined in the erature but involves an estimation of economic consequences of a wage demand. The ajor specifications of ability to pay are (1) the entity, (2) the wage demand, (3) the .ture of the standard of economic consequences, (4) the time period, and (5) the antitative level of the standard of economic consequences. Accounting's contribution is ated to determination of firm rates of return, one of the key economic consequences. ols of accounting analysis available to supplement the rate of return include produc-ity, marginal, cash flow, and breakeven analyses.

Industrial Organization; Government and Business; Industry Studies

AN J. ABOUCHAR, Ph.D. California (Berkeley) 1966. The spatial efficiency of the So-viet cement industry: 1913-1940.

UCE T. ALLEN, Ph.D. Cornell 1965. The impact of product-market concentration on nterindustry wage behavior: United States manufacturing, 1947-60.

This study tests the hypothesis that workers in concentrated industries receive larger ge increases than those employed in structurally competitive industries. A study of ual and cyclical per cent changes in BLS average hourly earnings and Census con-tration ratios discloses a relationship usually significant statistically. However, no lanation of this phenomenon could be found in the literature, and the author's at-pt at explanation is not satisfactory.

HAMMED S. AL-MAHDI, Ph.D. Pennsylvania 1966. Middle East crude oil industry: a ase study of weakening oligopoly.

WLAND G. AMOA, Ph.D. Mass. Inst. Technology 1965. A study in demand: an analysis f the cocoa bean and cocoa products markets of the United States.

LE C. BARLETT, Ph.D. Michigan 1965. The demand for passenger air transportation 947-1962.

ID A. BELSLEY, Ph.D. Mass. Inst. Technology 1965. Industry production behavior: n econometric analysis.

TH S. BENJAMIN, Ph.D. New School 1966. The viability of little business.

IN BERNHARDT, Ph.D. California (Berkeley) 1966. The effect of product segmenta-on, geographic market segmentation and product differentiation on intra-industry erging in manufacturing industries.

ER G. BIESIOT, JR., D.B.A. Southern California 1966. Concentration and trends in e California petroleum industry.

vestigation of California's petroleum industry's structure and behavior, 1924-1964, ws immense industrial growth as number of companies decreased. The majors' share narket remained fairly constant. Behavior of this rigid oligopoly varies from com-tive to monopolistic, with crude production more competitive than refining. Gasoline es in constant dollars have continuously declined. The industry's future is dependent smog eradication, development of competitive sources, import quotas, Connally Act acement, lowering of depletion allowance, and shale oil production.

ER D. BLACKWELL, Ph.D. Northwestern 1966. Price levels of funerals: an analysis the effects of entry regulation in a differentiated oligopoly.

RLES P. BLITCH, Ph.D. North Carolina 1966. Product innovation and price dis-

The purpose of this empirical investigation of the price policy of the innovator of cellophane, E. I. du Pont de Nemours and Company, Incorporated, in the period 1924-1951, is to test the hypothesis that the innovator used a discriminatory price policy. Analysis of the data supports the hypothesis. Findings also suggest that price discrimination in association with high entry barriers severely limited competition. Policy conclusion suggests nondiscriminatory pricing in new product markets with substantial obstacles to entry.

WILLIAM H. BRICKNER, Ph.D. Stanford 1966. Pricing strategies for new industrial products in oligopolistic industries.

WILLIAM D. CLARK, Ph.D. Arkansas 1966. An investigation into the causes of motor-carrier loss and damage claims.

PETER M. COSTELLO, Ph.D. Michigan State 1966. Patents in the antibiotic industry.

The thesis explores questions of price and product competition in the antibiotic segment of the ethical drug industry under the influence of product patents. The industry is examined for the early 1940's through 1960 in an attempt to delineate the effects of product patents on technical progress, pricing, and on the competitive behavior of firms.

CURTIS CRAMER, Ph.D. Maryland 1966. The economics of natural gas pipeline pricing.

Natural gas pipeline companies render various types of services to many different consumers. This variation of services raises the question whether or not cost-pricing is reflected in gas pipeline rates. Rate comparisons indicate significant industry uncertainty in an appropriate demand-commodity rate structure. The study of natural gas pipeline rates reveals that the rates are responsive to demand factors and do not appear to follow closely the rules governing marginal cost pricing.

WAGIH G. DAFASHY, Ph.D. Arkansas 1966. An analysis of the entity theory of business enterprise.

PAUL D. DOAK, Ph.D. Iowa State 1965. Resource utilization and productivity estimates in the food and kindred products industry.

The elasticities of substitution of capital for labor were estimated for the three- and four-digit industries in the food and kindred products industry. These estimates indicate that the elasticities are greater than zero but equal to or less than one. These elasticities are not different from one in many instances which indicates that the Cobb-Douglas function adequately describes the relationship between factor usage and output in terms of value added. In addition, estimates of technological advance indicate that this factor has been responsible for more than 50 per cent of the increased output per worker as opposed to increased use of capital per worker.

ANTHONY N. DOHERTY, Ph.D. Tulane 1965. An economic analysis of section 2F of the Robinson-Patman Act.

This study consists of a theoretical and empirical evaluation of the provisions establishing legal liability for the buyer under the Robinson-Patman Act. The primary objectives of the study are twofold: first, to bridge the gap in theoretical economics by adapting, extending and elaborating upon the theory of price discrimination so as to provide a more comprehensive theory pertaining to the buying side of the market; second, in the light of the theory presented, to ascertain the influence of the buyer-liability provisions of the Robinson-Patman Act on competition, vertical-price relationships and economic integration.

ALFRED S. EICHNER, Ph.D. Columbia 1966. The emergence of oligopoly: sugar refining as a case study.

BRUCE ERICKSON, Ph.D. Michigan State 1965. Price fixing under the Sherman Act: case studies in conspiracy.

Of all antitrust law violations price fixing is the most common. Yet economists' knowledge about the causes and consequences of price fixing is limited. This study summarizes the literature on conspiracy and examines two price-fixing agreements in detail. On the basis of the foregoing it attempts to answer such questions as: What are the prerequisites for conspiracy? What conditions allow conspiracy to continue? What are the characteristic patterns of conspiracy in terms of industrial structure, conduct, and performance?

Bert M. Evans, Ph.D. Harvard 1966. Internal-external efficiencies and structural change in the perishable bakery products industry.

Malcolm Galatin, Ph.D. Mass. Inst. Technology 1965. The measurement of economics of scale and technological change in the multi-unit plant; with special reference to steam electric power generation.

Lee E. Glick, Ph.D. Pittsburgh 1965. An analysis of the effect of regulatory policies on major chains in retail food distribution.

George R. Green, Ph.D. Pennsylvania 1965. A micro-econometric analysis of costs in United States petroleum refining establishments.

Paul T. Green, Ph.D. Columbia 1966. The patent enigma.

Mohammed K. Hamid, Ph.D. Iowa 1966. Price and output decisions in the computer industry.

Robert E. Harrison, Ph.D. Arkansas 1966. Better Business Bureaus: their activities in improving business ethics and practices in selected cities.

Glen W. Herrin, Ph.D. Alabama 1965. The economic, accounting and financing problems of the commercial airlines, 1945-1962.

Archer W. Huneycutt, Ph.D. Arkansas 1966. A theoretical analysis of the past, present, and future of retail discounting as a mode of operation.

Norman N. Kangun, D.B.A. Indiana 1965. A model of the action process leading to the enactment and enforcement of Sunday closing laws: a selected community study.

William H. Kaven, Ph.D. Cornell 1966. The impact of private label ice cream on industry structure and conduct.

The rise of supermarket chains has led to the marketing of a manufacturer's output under both manufacturer's label and a private label at widely different prices only partially explicable by differences in quality. This gap is to be explained by conditions of monopolistic competition for the chains' annual contracts in the case of the latter. There are important economies of scale in production. Hence the large manufacturers are squeezed severely by the chains and are unable to press their own label products effectively in the market, while small local producers unable to handle large chain store contracts are being forced out.

Helen Kramer, Ph.D. Wisconsin 1965. The economic impact of the aerospace and electronics industries in the United States, 1950-1961.

A linear regression model is used to measure by States the relationship between personal income and employment in the principal defense industries. Two alternative hypotheses, that large-scale military spending has occurred at the expense of the standard of living, and that this spending has been an essential prop to the economy, are not supported by the data. A third hypothesis, that the impact has been significant in selected regions, is confirmed for eight States.

Wei Kang Liang, Ph.D. Pennsylvania 1965. The wage share in the one hundred largest corporations.

John L. Livingstone, Ph.D. Stanford 1966. The effects of alternative accounting methods on regulatory rate of return decisions on the electric utility industry.

Wesley Long, Ph.D. Michigan 1966. Determinants of the demand for inter-urban air travel in the United States.

John H. Lorant, Ph.D. Columbia 1966. The role of capital-improving innovations in American manufacturing during the 1920's.

Dougald A. MacFarlane, Ph.D. Washington 1965. Marginal cost pricing of water with special reference to the city of Everett, Washington.

Marginal cost pricing constrained by legal requirements is studied in connection with the supply of water to Everett, Washington. The legal requirements require some form of price discrimination in this case. Marginal cost price, as usually defined, does not yield an efficient solution.

Paul F. McGouldrick, Jr., Ph.D. Harvard 1965. Profits, capital, and capital spending in the 19th Century cotton textile industry of New England.

JEROLD J. MORGAN, Ph.D. Alabama 1965. Post-auditing in state and local government with emphasis on governmental post-auditing in Alabama.

DENNIS C. MUELLER, Ph.D. Princeton 1966. The determinants of industrial research and development.

On the basis of an investigation of survey data of individual firms, the determinants of the composition and size of expenditure on research and development it is found that sales volume and diversification positively affect the R&D expenditures of a firm. However, taking dividends, advertising and capital expenditures as the other endogenous variables, and profits and depreciation as the major explanatory variables, the author finds a negative correlation between sales and research. He offers alternative explanations and policy suggestions on basis of divergent findings.

ALBERT C. NEISSER, Ph.D. New School 1966. Joint costs, economies of scale and competitive pricing in international communications.

LOUIS R. OLIKER, D.B.A. Indiana 1966. The pharmaceutical industry: institutional characteristics and statutory review.

JAMES R. OLSON, Ph.D. Minnesota 1965. An economic analysis of the Northwestern flour milling industry with special reference to taxation and location.

The changes in milling and baking technology, in relative wheat prices, in the location of wheat production of needed quality, in transportation cost relationships, and the differential rates of growth in demand in the several product markets are of greater importance to the continuing profitability of milling capacity locations in the Northwestern flour milling industry than differences in operating costs or state and local taxes.

GERALD P. OWENS, Ph.D. Ohio State 1965. Factors affecting demand for outdoor recreation.

Data were collected by interview in a tri-state area regarding participation in fourteen outdoor activities, distances travelled, characteristics of participators, etc. Relationships between participation and various characteristics are identified and measured. Special attention is given to derivation of demand schedules and income-consumption curves and attendant elasticities. Study findings are discussed in terms of practical application in locating, planning, and operating public and private recreational enterprises.

KAREN R. POLENSKE, Ph.D. Harvard 1966. A case study of transportation models used in multiregional analysis.

RUSSELL A. PORTER, JR., Ph.D. Arkansas 1966. An investigation of the effects of discount department store competition on selected retail establishments in the Dallas metropolitan area.

PHILIP P. ROBBINS, Ph.D. Pittsburgh 1966. The Tarbell Papers and the history of the Standard Oil Company: an aspect of a new economic era.

CHARLES M. SACKREY, Ph.D. Texas 1965. Overcapacity in the United States international air transport industry.

PHILIP B. SCHARY, Ph.D. California (Los Angeles) 1966. Competition in domestic air cargo.

PAUL L. SCHMIDBAUER, Ph.D. California (Berkeley) 1965. Information and communications requirements of the wheat market: an example of a competitive system.

NORMAN SCHNEIDER, Ph.D. California (Berkeley) 1966. Mixed oligopoly: a study in the control of industry in a developing economy.

EDWARD M. SMITH, Ph.D. Alabama 1965. An analysis of the methods of intra- and inter-market area delineation for service drug wholesalers in Alabama.

JACK M. STARLING, Ph.D. Arkansas 1966. The utilization of part-time employees by retail establishments.

MAHLON R. STRASZHEIM, Ph.D. Harvard 1966. Efficiency in the international airline industry.

FRANCIS X. TANNIAN, Ph.D. Virginia 1965. Water and sewer supply decisions: a case study of the Washington Suburban Sanitary Commission.

This study analyzes particular local public-spending decisions, using the suburban Mary-

land region near Washington, D.C., as a case study area, with emphasis on the post-World War II period. An analysis of the role played by the Washington Suburban Sanitary Commission leads to the conclusion that the Commission's performance "has been undesirable because its supply decisions have been wasteful" and that the Commission's "institutional structure has induced misuse of public power by private initiative." The study calls for "more economically aware and responsive institutions . . ." to assure narrowly distributed political largesse is replaced with economic efficiency.

CHARLES L. TROZZO, Ph.D. Harvard 1966. Technical efficiency in the location of integrated blast furnace capacity.

ALEXANDER G. VICAS, Ph.D. Princeton 1966. The lag of unrestricted imitation following product innovation.

Presents case studies of eleven product innovations, not protected by patents, in three industries—cigarettes, fountain pens, and phonograph records. Various hypotheses on the lag of imitation behind innovation are tested with reference to data obtained chiefly from the daily press and trade journals.

MELVIN M. WAGNER, Ph.D. California (Berkeley) 1966. Interregional competition in the frozen vegetable industry.

Long-run transportation models incorporating estimates of regional production costs and consumption compared with past trends indicate continued cost (and price) reductions and resultant large increases in consumption in this competitive industry. Transport firm output is measured in units of weight at the point of consumption (distance a summary of bundles of inputs) instead of ton-miles. The correspondence between assumptions of transportation models and of theory relevant to the long run is emphasized.

JOHN A. WALGREEN, Ph.D. Boston College 1965. The economics of ocean liner freight conferences.

This dissertation is a study of the pricing of ocean liner freight carriage in United States foreign trade. Freight rates are determined by cartel-like associations of steamship lines known as shipping conferences. The economic factors which create and which limit the monopoly power of the conferences are analyzed. The author concludes that the operations of shipping conferences in U.S. foreign trade produces some economic misallocation of resources but that the exploitation of conference monopoly power is restricted by competition from nonconference shipping services, secret price cutting by conference members, and shippers' monopsony power.

JAMES E. WILLIS, Ph.D. Tulane 1965. A study of growth in the telephone industry of Louisiana from 1950 to 1962.

This dissertation is an investigation of a factor presumed to have influenced the diverse growth patterns of sub-state residence main telephone stations in Louisiana from 1950 to 1962. Previous studies indicated that station growth should have been related to changes in employment levels. Statistical analysis of the data, however, indicates that there is no consistent influence on the growth patterns attributable to employment level changes. Possible causes for these observations and suggestions for further study are offered.

Land Economics; Agricultural Economics; Economic Geography; Housing

WILLIS E. ANTHONY, Ph.D. Minnesota 1965. Size structure and growth of livestock slaughter firms.

HAROLD L. BAKER, Ph.D. California (Berkeley) 1966. Role of timber growing in land use in Hawaii.

The role of timber growing in land use in Hawaii is determined through the application of equilibrium economics. Projected long-run demand for Hawaiian-grown timber is derived in the goal year 2000. Next, the expected long-run timber supply function is developed, giving full cognizance to the timber demand-supply situation throughout the Pacific Basin. Finally, these functions are equated, and the timber volume thus established translated into the acreage needed for timber growing.

RICHARD C. BARTH, Ph.D. Wisconsin 1966. A study of the demand for housing.

MERRILL J. BATEMAN, Ph.D. Mass. Inst. Technology 1965. Cocoa in the Ghanian economy.

KONRAD BIEDERMAN, Ph.D. Ohio State 1965. Regional loan rate differentials for wheat.

The study explores the feasibility of using a transportation model to determine optimum regional loan rate differentials for wheat. This approach assures independence from historical price relationships which may be distorted. It also permits use of lowest-cost mode of transportation and treatment of each class of wheat on the basis of its own supply and demand conditions. Findings indicate that this approach is a workable alternative to present methods of setting loan rate differentials.

FAHMI K. BISHAY, Ph.D. Iowa State 1965. Marginal rates of substitution between land, labor and fertilizer in relation to the optimum planning of resource combinations.

The object of this study is to investigate the economic and technical relationships between fertilizer (as a form of capital), land and labor. For this investigation to be carried out, the derivation of production functions which relate fertilizer, labor, and land have been conducted. These functions represent the production of nine crops, namely: corn, cotton, wheat, barley, cereals, rice, paddy, maize and peanuts raised in five countries: the United States, Canada, Sweden, U.A.R. (Egypt), and India. The determination of the optimum policies with respect to resource allocations in fertilizer investments versus other productive projects have been investigated. However, the approach is to modify the problem into agricultural policy formulation and to use programming techniques to determine the optimum solution. Moreover, some measures of reliabilities reflected in establishing the confidence intervals for marginal physical products, insoquants, and isoclines have been specified too.

BARRY W. BOBST, Ph.D. Washington State 1966. Optimal spatial and structural changes in Washington's fluid milk industry.

The number, size, and location of fluid milk plants in Washington in 1975 have been estimated. One estimate was made in an uncontrolled setting, in which fluid milk plants were allowed to become spatial monopsonists and monopolists in the raw and processed milk markets, respectively. Another estimate was made under a set of assumed restrictions which limited the share of the market that could be supplied by any one plant in each geographic consuming area.

LARRY M. BOONE, Ph.D. Washington State 1966. A study of dry edible pea prices in the Pacific Northwest.

A description of the market structure was developed from information obtained by interviews with people in the industry supplemented by published supply and disposition data. Price appears to be influenced primarily by domestic supplies and by foreign demand. Domestic disappearance and price do not appear significantly related.

PAUL G. BRADLEY, Ph.D. Mass. Inst. Technology 1966. The economics of crude petroleum production.

KEITH C. BROWN, Ph.D. Southern Methodist 1966. A theoretical and statistical study of decision-making under uncertainty: competitive bidding for offshore petroleum lands.

Bids for leases appear to be lognormally distributed with constant variance within scales. To make an optimal bid for a tract, it is necessary to estimate the probability that any proposed bid has of winning. The variance of the distribution of competitors' bids may be estimated from similar past sales and an iterative procedure is suggested for estimating the mean. The resulting formulations are subjected to sensitivity analysis and their implications discussed.

GEORGE L. CASLER, Ph.D. Purdue 1966. The effect of cardiovascular impairment on farm organization.

An analytical study of systems selection by obtaining the least-cost solution to a network and then comparing the effect of the systems selected on the potential profit of the farm using linear programming. The effect of restrictions appropriate for farmers with heart disease, reduced rates of energy expenditure and hours worked per day was estimated with different levels of mechanization.

LON C. CESAL, Ph.D. Iowa State 1966. Normative resource demand functions and elasticities programmed for representative farms.

CHAO-CHEN CHEN, Ph.D. Cornell 1965. An analysis of the supply-demand-price structure of onions in the United States.

R. Gene Conatser, Ph.D. Southern Methodist 1966. Land reform and economic development: Mexico 1930-1960.

There exists a growing literature on the potential contribution of a program of land reform to the economic development of the less developed countries of the world. In the period from 1930 to 1960, Mexico pursued an active program of land expropriation and distribution which transferred almost 35 per cent of the total crop land of the country to groups of formerly landless peasants. An examination of the available data indicates that this land reform program did not result in increased agricultural investment, greater factor mobility in agriculture, or more efficient sized agricultural units. The major visible contribution of the program is reduced rural unrest.

Ralph W. Cummings, Jr., Ph.D. Michigan 1965. The structure and functioning of the Indian wheat market, with special reference to Khana, Punjab, 1956/57-1963/64.

Rachael Dardis, Ph.D. Minnesota 1965. The welfare cost of agricultural protection.

A partial equilibrium model for both final and intermediate goods is developed to measure the welfare cost of agricultural protection. In the second part of the study absolute and relative costs of protection are obtained for the grain sector in the United Kingdom and the feed-livestock sector in West Germany for selected years. It is concluded that cost rather than degree of protection is important since it reflects the economic consequence of protection.

Dinkerrai R. Desai, Ph.D. Kansas State 1966. The effect of selected weather factors on yields of wheat grown under various rotational practices at the Fort Hays Branch Agricultural Experiment Station.

In order to improve the information available to farmers for decisions with respect to abandonment of growing wheat prior to harvest, the effect of selected weather variables and cultural practices on wheat yields, for various portions of the growing season, was studied using regression analysis. Yield data for more than 30 years from experimental fields were used and compared with results obtained from geographically aggregated data by other researchers.

Mohamed G. D. A. El-Dahab, Ph.D. Kansas State 1965. An aggregation procedure for deriving representative farms in estimating supply functions.

This study was designed to evaluate aggregation procedure for utilizing sample data for constructing representative farms to be used in estimating aggregate supply functions by linear programming procedures. Results obtained are compared with those of a previous study using unaggregated data for 49 sample farms. Producers' response to milk price changes is determined by programming nine constructed representative farms under three basic milk price situations programmed in the original study of 49 farms. The constructed representative farm method results in a reduction of 82 per cent in computational time and results obtained indicate that estimates by this method are not substantially different from those obtained by conventional programming procedures of the original study.

Abdel El-Yamani, Ph.D. Minnesota 1965. The process of technological advance in Egyptian agriculture: lessons from the U.S. experience.

A. Myrick Freeman, II, Ph.D. Washington 1965. The federal reclamation program and the distribution of income.

The Bureau of Reclamation, in developing irrigation projects, has significant effects on income distribution. This dissertation examines a sample of single purpose Bureau of Reclamation projects, using a theoretical model embodying both efficiency and redistribution elements, to determine the welfare effects of such projects. It is found that most of the Bureau projects examined, corrected for proper measurement concepts, were inefficient. While the redistribution effect was in the desired direction, any reasonable weights still yielded approximately the same ranking of projects, and did little to improve their overall efficiency rating.

Philip H. Friedly, Ph.D. Southern California 1965. Requirements and determinants for the economic allocation of space in community planning.

Harald B. Giaever, Ph.D. California (Berkeley) 1966. Optimal dairy cow replacement policies.

A Markovian dynamic programming framework is used for the formulation of a dairy

cow replacement model, in which different cow characteristics exhibit stochastic variation. A multi-variate normal model is used as a basis for estimation of cow population parameters from empirical data. The normal stochastic process is then transformed to a Markov process by definition of new state variables. Optimal replacement policies for dairy herds are derived under different price assumptions.

BERNAL L. GREEN, Ph.D. Purdue 1966. Factors affecting participation in selected outdoor recreation activities.

This study is one of several related endeavors designed to explore the possibilities for alternative uses of resources in low-income areas. Multiple regression techniques are used to analyze the extent that participation in four outdoor recreation activities (camping, boating, fishing, and hunting) is affected by selected independent variables. Data obtained by mail questionnaire indicated occupation, age, vacation time, education, type of equipment owned, income, marital status, and distance to recreation area as the significant variables.

JAMES W. GRUEBELE, Ph.D. Minnesota 1965. Changing market structure of the Minnesota dairy manufacturing industry.

WILLIAM O. HANCOCK, Ph.D. Purdue 1966. Farm plans and supply response estimates for Indiana: hog and beef cattle farms.

Objectives are to (a) derive farm plans and (b) estimate supply response of hogs and beef cattle production to prices in Indiana as part of a North-central regional study. Thirty-eight representative farm types based on size, location, and products are classified from a 971-farm survey. Representative farms are linear programmed for various price combinations for hogs, beef cattle, and corn. Normative supply responses for Indiana are estimated based on the numbers of farms in each category.

IAN W. HARDIE, Ph.D. California (Berkeley) 1966. Deriving implicit grower prices for a walnut processing and marketing cooperative.

How should the total pool of profits earned by a marketing cooperative be divided among its members? Finding an answer to this question for Diamond Walnut Growers, Inc. (a California cooperative) constitutes the objective of this dissertation. A linear programming model is formulated to maximize the cooperative's profit by optimally allocating approximately 270 types of unprocessed walnuts to the finished products. The dual solution of this linear program attaches shadow prices to the unprocessed walnuts.

ALBERT C. HEINLEIN, Ph.D. Western Reserve 1966. A probabilistic approach to evaluating community expenditures on industrial development.

The study explores the feasibility of using statistical decision theory to evaluate alternative community industrial development efforts. A 5-variable model is found to account for 90 per cent of the variance in number of new plants per community. Industrial parks make the greatest improvement in probability of attracting a new plant with an expected payoff of about 2:1.

GLENN A. HELMERS, Ph.D. Iowa (Ames) 1965. Factors affecting the demand for farm labor.

Demand functions for several types of farm labor are estimated nationally and regionally for the ten farm production regions. Least-squares time-series regression procedures are employed with independent variables representing a number of aspects of the farm and nonfarm economy. Migration functions for farm population, hired, family and total farm labor are estimated using a least-squares cross-sectional analysis. State data are employed through the use of differences in dependent and independent variables between selected years. The results indicate substantial structural differences in the estimated demand and migration functions between hired and family farm labor.

WILLIAM W. HICKS, Ph.D. Stanford 1965. The agricultural development of Mexico, 1940-1960, with special emphasis on cotton and wheat production in Northern Mexico.

I. Introduction: Structural change in Mexican economy. Changes in share of income and exports from agriculture and share of agricultural products in total imports. Changes in level and pattern of demand for agricultural production. II. Mexican agricultural output—increase in total agricultural output by crop, geographic distribution, "commercial" farming. III. Agricultural inputs in Mexican agriculture—harvested area, labor, irrigation,

fertilizer, machinery, improved seeds, pesticides and credit. IV. The productivity of inputs in Mexican agriculture, for the country as a whole and individual states. V. Evaluation of Mexican agricultural development—qualified definition of optimal strategy.

YU-CHU HSU, Ph.D. Cornell 1965. Income variability and resource productivity of rice farms in Changhwa and Nantou Counties, Taiwan, China.

GEORGE JOYCE, Ph.D. Alabama 1965. An analysis of the intra- and inter-market shopping travel behavior of motor vehicle users in a small community area—Gardendale, Alabama.

GERALD L. KARR, Ph.D. Southern Illinois 1966. Economic models of the feeder-swine market.

This study is concerned with the price-making forces of an intermediate good which is undergoing a rapid change in structure. Particular attention is given to the feeder pig pricing system. After developing theoretical models with different assumptions as to expectations, certain models are tested.

RICHARD T. F. KING, Ph.D. California (Berkeley) 1965. River basin projects and regional development: an examination of public investment criteria in the light of Mexican experience.

After a theoretical discussion of different approaches to evaluating large-scale public investment projects in underdeveloped countries, an examination of Mexican regional development problems and policies, and an analysis of the way in which river basin projects might stimulate regional economic development, several hypotheses are tested in the case of a sixteen-year-old Mexican project, the Tepalcatepec Project. As predicted by much development theory, agricultural development has been stimulated but not the intended industrialization.

JOHN A. KNECHEL, Ph.D. California (Berkeley) 1966. Dynamic analysis of capital management for California fluid milk producers.

Dynamic linear programming is applied to determine the optimum long-run (30-year) path of investment for a representative firm, given: the firm's initial resource organization; the relevant technical, institutional, and economic structures; and the relationship between the proprietary enterprise and the household. Neoclassical microeconomic analysis and capital management theory are critically appraised. Variations in the model analyze the sensitivity of capital growth to changes in technology, debt-equity structure, producer-distributor contractual arrangements, consumption behavior, time-discounting, and the objective criterion.

EMANUEL LANDAU, Ph.D. American 1966. The public control of air pollution· a case study in agriculture.

For developing effective air pollution control measures, estimates of benefit and costs are essential. The most manageable estimates of damages due to air pollution are estimates of damage to crops and livestock; at prices paid the farmer, this is currently estimated at $500 million a year. Although these estimates of national crop damage are undoubtedly inexact, there is a distinct possibility that, because of the prevailing concentration on observable damage only, they may represent substantial understatements.

ROBERT R. LANSFORD, Ph.D. Minnesota 1965. Personal attributes of farmers related to earnings.

UMA J. LELE, Ph.D. Cornell 1965. Efficiency of Jowar marketing: a study of regulated markets in Western India.

CHARLES H. LITTLE, Ph.D. California (Berkeley) 1965. The derivation of a model for predicting lumber grade recovery.

A set of equations is derived from the system of production functions with lumber grade outputs as the dependent variables and external physical characteristics of a log as the independent variables. Linear regression techniques are used to estimate the coefficients of the system, using data from a study on inland Douglas-fir. The system of equations is tested to see if it can be used to predict lumber grade recovery from the surface characteristics of the log.

SEBALD G. MANGER-CATS, Ph.D. Cornell 1966. Land tenure problems and economic development in Guatemala.

JITENDAR S. MANN, Ph.D. Minnesota 1966. The contribution of United States Public Law 480 to Indian economic development.

DAVID C. MAJOR, Ph.D. Harvard 1965. Decision making for public investment in water resource development in the United States.

ROGER A. MATSON, Ph.D. Colorado 1965. The pricing of irrigation water output on federal projects, with a case study of the Colorado-Big Thompson project.

T. SARMA MALLAMPALLY, Ph.D. Chicago 1966. An analysis of factors influencing increases in rice yields within Japan.

JIMMY L. MATTHEWS, Ph.D. California (Berkeley) 1966. Price determination and supply adjustment in the California and United States asparagus economy.

This study develops a dynamic recursive model of the asparagus economy to be used as a basis for prediction and projection of prices and quantities. The model considers a four-product outlet for the utilization of asparagus and four supply regions. Because of the simultaneity in the demand matrix, the model is partially recursive with simultaneous equation subsets. The basic use of the model is to investigate the probable implications of several alternative values of market variables which can be influenced either by the asparagus industry or other interest groups. Primary among these is the probable economic impact of terminating Public Law 78.

VERNON C. MCKEE, Ph.D. Iowa (Ames) 1966. Optimal land and water resource development—a linear programming application.

JOHN H. MERRIAM, Ph.D. Claremont 1966. The influence of property tax differentials on on the location of economic activity.

Effective rates of property taxation vary substantially among areas in the United States. This dissertation explores the potential effects of property tax differentials upon the location of economic activity. Numerous researchers, using correlation analysis, have examined levels of business activity and tax burden and have concluded that no significant relationship exists between the two. The location decision, however, is based on much more than relative tax levels, and a partial equilibrium examination of taxes and industrial growth does little to identify the effect of property tax differentials in a general equilibrium world. The case studies attempt to separate the influence of a property tax differential in a real world situation.

STEPHEN D. MESSNER, D.B.A. Indiana 1966. The application of benefit-cost analysis to selected urban renewal projects in Indianapolis, Indiana.

ROBERT G. MUELLER, Ph.D. Washington State 1966. Economies of size and management alternatives of commercial cow-calf ranches in Washington.

A study of cost variations in beef production due to kind, size, and management of selected cow-calf ranching operations in Northcentral and Northeastern Washington. The major cost reductions due to size of cow-calf ranches in the areas studied are achieved when the ranch is of sufficient size to handle 150-200 brood cows. In general, multiple-enterprise ranch organizations have higher net incomes than single-enterprise cattle ranches, as do ranches using range improvement practices to increase stocking rates.

RICHARD T. NEWCOMB, Ph.D. Minnesota 1965. The impact of technical change on the iron ore industry of Minnesota: 1917-1963.

Where high rates of depletion characterize production, normal measures of technical change understate its impact. The study employs disaggregation, explicit depletion parameters and aggregate production functions to estimate in two stages these separate impacts. In mining, technical change increases total-input productivity .036 annually. Depletion, observable in beneficiation, reduces this to a negligible rate of .01 annually, and is solely responsible for the decline in the aggregate average product of labor after 1940.

DONALD R. NICHOLSON, Ph.D. Cornell 1965. Milk supplies in relation to federal milk marketing orders.

DAVID W. NORMAN, Ph.D. Oregon State 1965. Natural resource characteristics as related to the pattern of agricultural income in certain specified areas of the United States.

The main objective of the study is an attempt to obtain a general exploration for the geographical pattern of agricultural income. This was undertaken in the belief that certain

possible determinants of agricultural income, specifically those related to natural resource characteristics, have not been satisfactorily considered in investigations up to the present time. Consequently, various determinants which other researchers have found important e.g., location, education, age, etc., were combined in one model, together with a number of variables designed to measure various characteristics of natural resources.

WILLIS PETERSON, Ph.D. Chicago 1966. Returns to poultry research in the United States.

The aim of this study is to identify downward shifts in the long-run farm supply schedule of poultry products arising from the use of new, more efficient inputs which have been created by poultry research (public and private) and to obtain a measure of the consequent saving in resources to society. The results indicate that on the average each dollar invested in poultry research in the United States since 1915 returns about 20 cents per year beginning at the date of investment and extending on into perpetuity.

EARL W. PHILLIPS, Ph.D. Colorado 1965. Income distribution as a factor in regional growth; with special reference to the Southeast United States.

YAKIR PLESSNER, Ph.D. Iowa (Ames) 1965. Quadratic programming competitive equilibrium models for the United States agricultural sector.

The dissertation consists of three parts: (1) an outline of a policy choice procedure with particular emphasis on the United States farm problem; (2) a development of a series of quadratic programming models whose solutions, if they exist, are partial or general competitive equilibria; and (3) an application of one of the models to the United States grain economy and a description of how the prescribed choice procedure can be carried out by solving an array of quadratic and linear programming problems.

MICHAEL V. E. RULISON, Ph.D. California (Berkeley) 1966. Agriculture resource adjustment and farm labor force decline, 1950 to 1959: a comparative interregional study.

This study deals with changes in the structure of the agricultural sector accompanying declining labor input during 1950-1959 in five U.S. economic regions containing nearly three-fourths of the farms and farm labor force. The most significant finding is the substantial reduction in differences between regions with regard to the structure of their agricultural sectors as indicated, for example, by the narrowing of the range of gross sales of farm products per farm.

M. T. R. SARMA, Ph.D. Chicago 1966. An analysis of factors influencing increases in rice yields within Japan.

This dissertation deals with the derivation of estimates of the effects of some factors which are generally considered to be of importance in bringing about the rapid increases of rice yields in Japan during 1949-61.

BARNEY K. SCHWALBERG, Ph.D. Harvard 1966. The Russian peasant economy before collectivization: an essay in agrarian economics.

MAHMOUD M. SHERIF, Ph.D. Iowa (Ames) 1965. Programmed supply functions for pork and beef in Iowa.

JOHN C. SPYCHALSKI, D.B.A. Indiana 1965. The administration and economics of the agricultural exemption in the Interstate Commerce Act.

WILLIAM P. STEPHENS, Ph.D. Minnesota 1965. Economic efficiency in the shipment of New Mexico feeder cattle.

JOE BRUCE STEVENS, Ph.D. Oregon State 1965. A study of conflict in natural resource use: evaluation of recreational benefits as related to changes in water quality.

The evaluation of social investment in water pollution control has been retarded by a lack of methodology with which to evaluate the direct benefits. The objective of this dissertation is to develop theory and methodology for estimating direct recreational benefits associated with the protection of sport fisheries.

DONALD C. TAYLOR, Ph.D. Minnesota 1965. Income improving adjustments and normative supply responses for hogs and beef in Southwestern Minnesota.

MILLAN I. VUCHICH, Ph.D. Pittsburgh 1965. Housing improvement as a market process.

ROBERT C. WELLS, Ph.D. Cornell 1966. An economic study of dairy farming in the Central Plain Region of New York, 1963-64.

RUSSELL C. YOUMANS, Ph.D. Purdue 1966. An empirical study of underemployed agricultural labor in selected areas of Minas Gerais, Brazil.

This study is an attempt to identify the extent of underemployed agricultural labor on commercial farms in five locations in the State of Minas Gerais, Brazil. Marginal productivities were computed from Cobb-Douglas functions for the various resources and used in evaluating the efficiency of labor use with respect to immediate wages and with respect to alternative employments in both the farm and nonfarm sector.

ABDEL ZAKI, Ph.D. Ohio State 1965. Economic development in the U.A.R. (Egypt) and the role of the agricultural sector.

The study examines the necessary environment for economic development and the contribution the agricultural industries make to economic development in a developing economy. Special attention is given to the Egyptian economy. It is concluded that for economic development to take place political, social, and economic changes should take place simultaneously to provide new ideology which would permit change in the behavior and attitudes of citizens in the country. Such changes have occurred in Egypt. In this country investments have increased but they have been more rapid in the industrial sector than in the agricultural sector. Credit facilities need to be expanded and the rate of mechanization should be such that it replaces only those human resources which are taken from the farm sector of the economy.

Labor Economics

LESLIE ASPIN, Ph.D. Mass. Inst. Technology 1966. Reinstatement under the National Labor Relations Act.

PETER S. BARTH, Ph.D. Michigan 1965. The labor force and labor force participation rates in Michigan.

SARA BEHMAN, Ph.D. California (Berkeley) 1966. Wage determination in a cyclical setting.

BHALCHANDRA J. BHATT, Ph.D. Wisconsin 1966. A case study in emerging industrial labor force in Bombay City.

JAMES J. BRADY, Ph.D. Notre Dame 1966. A case study of evolving standards in arbitration cases concerning layoffs.

HERRINGTON BRYCE, Ph.D. Syracuse 1966. Social origin as an obstacle or aid to mobility.

ARTHUR CAROL, Ph.D. New School 1966. The Manpower Development and Training Act of 1962: its effect in the Long Island labor market.

CHARLES D. DELORME, JR., Ph.D. Louisiana State 1966. A study of the attitude of organized labor toward monetary reform and monetary policy 1866-1965.

The development has been in three stages: 1866-1928, limited knowledge and understanding; 1929-1945, expansion and learning; 1946-1965, increased maturity and involvement. The views of the peak organizations, primarily the AFL, the CIO, and the AFL-CIO have been selected as representative of American organized labor. In view of the progress organized labor has made in its understanding in this area, no doubt the future will bring an even greater participation on its part.

PETER B. DOERINGER, Ph.D. Harvard 1966. The theory of internal labor markets.

SAMUEL ERLICHMAN, Ph.D. New School 1966. The attitude of trade unions towards productivity, the cases of Norway, Israel and Ghana.

MARY B. FITZPATRICK, Ph.D. Harvard 1965. Response to unemployment: Manchester, New Hampshire 1950-1959.

CHARLES F. FLOYD, Ph.D. North Carolina 1966. Public policy for depressed areas with special reference to North Carolina.

The dissertation seeks to identify areas of localized depression in North Carolina, to analyze the reasons for such depression, to describe existing public programs that attempt to alleviate such problems, and to make policy recommendations. Attention focuses on two broad types of programs: efforts to move jobs to workers, and efforts to enhance labor mobility. Although both have merit and may be pursued concurrently, the mobility approach is considered to be the more promising.

JOSEPH FORD, Ph.D. Fordham 1966. Manpower legislation and the problem of structural unemployment.

DONALD GARNEL, Ph.D. California (Berkeley) 1966. Teamsters and highway truckers in the West: the evolution of multiemployer bargaining in the Western highway trucking industry.

GEORGE HAGGLUND, Ph.D. Wisconsin 1966. Factors contributing to Wisconsin occupational injuries.

ISADORE B. HELBURN, Ph.D. Wisconsin 1965. An analysis of the industrial relations climate in profit-sharing firms.

GEORGE R. IDEN, Ph.D. Harvard 1966. The determinants of persistent high unemployment in major labor market areas of the United States from 1950-1963.

ALFONS JOACHIMOWSKI, Ph.D. New School 1966. The Polish wage structure: theory—problems—practice.

ARCHIE KLEINGARTNER, Ph.D. Wisconsin 1965. Professionalism and unionism: a comparative study of professional worker organizations.

MARVIN KOSTERS, Ph.D. Chicago 1966. Income and substitution parameters in a family labor supply model.

Expressions for income-compensated wage rate effects are derived in terms of coefficients from alternative regression models designed to estimate the effects of wage rates and income on labor supply. Regression analysis of hours of work for males suggests that the substitution effect for that component may be near zero. From a reinterpretation of regression results reported in other studies it appears that the substitution effect for married women is not negligible but substantially smaller than those studies suggest.

RUEBEN H. KROLICK, Ph.D. Stanford 1966. A study of the changing economic status of skilled occupations: railroad engineers and air line pilots.

The study analyzes for the period 1860 to 1965 the forces which first established favorable wage differentials for engineers and later narrowed them. A similar analysis is made for pilots who presently enjoy very favorable wage differentials. The overall objective was to evaluate the importance of such factors as technological change, union organization, collective bargaining, occupational skills and the labor market, industry growth, government policies and actions, and safety considerations, among others, on the changing patterns of wage differentials.

WILLIAM M. LANDES, Ph.D. Columbia 1966. The effect of state fair employment legislation on the economic position of nonwhite males.

ROBERT G. LANDOLT, Ph.D. Texas 1965. The Mexican-American workers of San Antonio, Texas.

MARTIN LAPINSKY, Ph.D. New School 1966. A study in occupational labor supply elasticities—the labor market for teachers.

WILLIAM H. LEAHY, Ph.D. Notre Dame 1966. The arbitration of discipline and discharge issues involving union representatives.

PHILIP MARCUS, Ph.D. Rutgers 1966. The economics of seasonal farm labor in New Jersey.

STANLEY H. MASTERS, Ph.D. Princeton 1966. The behaviour of output per man during recession, a study of underemployment.

Analysis of empirical data confirms that output per man declines in periods of recession. The author offers the hypothesis that specific investment of firms in their work force discourages layoffs. However, he attributes hitherto unexplained differences in estimated productivity changes to the use of different levels of aggregation. He argues that the Council of Economic Advisers, by ignoring time lags, overestimates the GNP gap and its underemployment component.

WARREN F. MAZEK, Ph.D. Pittsburgh 1965. Labor migration and depressed areas.

RICHARD U. MILLER, Ph.D. Cornell 1966. The role of labor organizations in a developing country: the case of Mexico.

JOSEPH D. MOONEY, Ph.D. Mass. Inst. Technology 1965. Displaced engineers and scientists: an analysis of the labor market adjustment of professional personnel.

MICHAEL H. MOSKOW, Ph.D. Pennsylvania 1965. Teachers and unions: an analysis of the applicability of collective bargaining for public school teachers.

WAYNE NEWKIRK, Ph.D. Louisiana State 1965. The legal and economic implications of the changing status of picketing.

DAVE M. O'NEILL, Ph.D. Columbia 1966. Occupational incidence of unemployment.

WILLIAM S. PEIRCE, Ph.D. Princeton 1966. Selective manpower policies and the trade-off between rising prices and unemployment.

Describes and analyzes particular labor markets (factory, office, craft workers, etc.) and shows how selective manpower policies affect prices and unemployment. These policies include government personnel policies and programs for training, mobility, and labor market information. The study includes a discussion of the institutional factors that impede successful application of selective manpower policies in many areas. "Success" is defined as their effectiveness in reducing increases in wages and prices.

MARTIN M. PERLINE, Ph.D. Ohio State 1965. A comparative analysis of the trade union press.

The primary purpose of this study is to determine whether the nature and scope of trade union publications have changed over the past quarter century. These publications are also studies to glean information as to whether the trade union movement, as suggested by many critics, has lost its zeal for social reform. Eleven trade union publications were used in the analysis—drawn from the years 1938-1939 and 1960-1961.

ANDREW J. PETRO, Ph.D. Michigan State 1965. Employment shifts and city size—Michigan and Indiana 1950-63.

The economic activity of a city participates as a unit in the national economy, and can gain or lose employment or gain at a slower rate. A shift technique is used to measure the relative changes. The selected cities in Michigan realized a favorable employment shift; Indiana's were unfavorable. The analysis indicates that the employment shifts and population were uncorrelated and that industry composition had little impact on the shifts. The source of the shifts is the locational advantages and the shifts occurred primarily in the manufacturing sector.

TREZZIE A. PRESSLEY, JR., Ph.D. Arkansas 1966. The role of programmed instruction in company training and development programs.

BRIAN QUINN, Ph.D. Cornell 1966. Some problems of producing a labor force in Africa.

The dissertation is an attempt, using interdisciplinary analysis, to explain the low level of labor commitment observed in several African countries south of the Sahara. The thesis explores the hypothesis that this problem is a manifestation of a broader social phenomenon—the impact of industrialization on the structure of preindustrial societies. Drawing upon recent theoretical literature in the fields of sociology, anthropology, and psychology, a close relationship is postulated between the structure of preindustrial African societies and the mental and emotional stability of the individual. Industrialization results in social disintegration and personality disorder, including poor performance in the work places. Statistical evidence is presented from countries in West, Central and East Africa relating industrialization with various indices of social disintegration and available evidence of increased mental disorders.

G. SOUNDARA RAJAN, Ph.D. Wisconsin 1965. A study of the registered apprenticeship program in Wisconsin.

ROBERT G. RICE, Ph.D. Columbia 1966. An analysis of private wage supplements.

SHERWIN ROSEN, Ph.D. Chicago 1966. Short-run employment variation on Class-1 railroads in the U.S., 1947-1963.

Two hypotheses are examined in attempting to explain negative correlation between skill and short-run employment variability on Class-1 railroads. One concerns costs of employment changes due to investments made by railroads in their employees, and leads to investigation of variation and timing of hours per man as well as the distributed lag

relationship between hours and employment. The second deals with employment effec of differences among employees in their substitutability with fixed factors, as measure with cross-section data. The data are consistent with both hypotheses.

PHILLIP TAYLOR, Ph.D. Arkansas 1966. An inquiry into the quantity and quality of offic manpower in the member banks of the eleventh district of the Federal Reserve Syste relative to the needs of these banks.

ALLEN L. TERRY, Ph.D. Alabama 1965. A study of the relationship between the econom period and the education and experience of personnel elected to top management posi tions in United States industry.

RICHARD S. UNDERHILL, D.B.A. Indiana 1965. Centralization in the Swedish labor marke

ANDREW WEINTRAUB, Ph.D. Rutgers 1966. An analysis of the cyclical pattern of strike in the United States, 1951-1961.

Strikes in the United States exhibit a cyclical pattern which corresponds to the refer ence cycle dates published by the National Bureau of Economic Research. The patter exhibited is similar to that of the "leading indicators." This study demonstrates the stron relationship between cyclical fluctuations in strikes and similar fluctuations in labor marke activity, emphasizing the role played by changing levels of employment.

ROBERT WELSH, Ph.D. Ohio State 1965. Management-labor relations in the retail food industry: emphasis, technological change.

The study pertains to the effect of technology on labor relations in the retail food in- dustry. This includes consideration of growth, development, changes, and trends in th industry which have affected the management-labor relationships. Among the objectives are ascertaining the present situation, over-all problems and trends in labor relations, deter- mining the specific management-labor problems associated with implementing technology and suggesting procedures which management should follow when implementing technologi- cal and operational changes.

RONALD L. WIGGINS, Ph.D. California (Los Angeles) 1965. Arbitration of industrial engineering cases.

ANDREW A. WILSON, Ph.D. Claremont 1966. Wage determination in the Los Angeles retail food industry.

The purpose of the dissertation is threefold: (1) to analyze and describe the environ- ment—economic and institutional—which characterizes the industrial relations system in the retail food industry; (2) to determine the wage policies of the retail unions and the food employers associations; (3) to measure and evaluate the effects of the union and food employers association on the wage structure in retailing. The scope of the study is limited to the collective bargaining experience and resulting wage structure of the retail clerks unions and the food employers associations in Southern California between 1939 and 1964.

Population; Welfare Programs; Consumer Economics

RALPH E. BERRY, JR., Ph.D. Harvard 1965. Competition and efficiency in the market for hospital services: the structure of the American hospital industry.

SAMUEL S. BOWLES, Ph.D. Harvard 1966. The efficient allocation of resources in educa- tion: a planning model with applications to Northern Nigeria.

RICHARD X. CHASE, Ph.D. Maryland 1966. Trends in poverty incidence and in its rate of reduction for various demographic groups: 1947-1963.

The study analyzes the response of poverty reduction to economic growth for demo- graphic groups that can be traced through the postwar period. Groups with the highest levels of poverty incidence have shown the lowest rates of poverty reduction; certain of these groups increased their rate of poverty reduction during a period of relatively slow economic growth, i.e., 1956-1963 (vis-à-vis 1947-1956); and the absolute number of poor has risen in certain high incidence groups.

JOEL B. COHEN, Ph.D. California (Los Angeles) 1966. Interpersonal response traits and consumer behavior.

DAVID J. DONALDSON, Ph.D. Stanford 1966. The economic functions of social insurance for old age.

For many reasons, including incentive effects, availability of information, economies of scale, the private insurance market for pensions and related risks is not perfect. Using the Pareto optimality criterion to evaluate theoretical institutions, a strong case for social insurance can be made. The same criterion can be used to make some recommendations about the form the social insurance should take.

WALTER W. DOTTERWEICH, JR., Ph.D. Pennsylvania 1966. Providing medical care for the aged in the United States.

JOHN P. FORMBY, Ph.D. Colorado 1965. The economics of discrimination—a normative approach.

THOMAS G. FOX, Ph.D. Syracuse 1966. A study of educational resource transformation within a large city high school system.

JOHN M. GLASGOW, Ph.D. Colorado 1965. Compulsory health education movement in the U.S.

LOIS S. GRAY, Ph.D. Columbia 1966. Economic incentives to migration: the Puerto Rican case.

GIORA HANOCH, Ph.D. Chicago 1965. Personal earnings and investment in schooling.

The study is concerned with earnings of males in the United States in 1959, with special emphasis on the relation between earnings and level of schooling, color, and region. It includes a theoretical analysis of the internal rate of return and the private demand for investment in schooling, as well as an empirical investigation based on a wide cross-sectional sample from the 1960 Census of Population. Adjusted life-time earning streams by age, estimated for eight schooling levels within race/region groups, serve as a basis for estimation and analysis of internal rates of return and demand schedules for investment in schooling.

SHIRLEY B. JOHNSON, Ph.D. Columbia 1966. An analysis of consumer durable expenditure of newly married couples.

RICHARD JOLLY, Ph.D. Yale 1966. Planning education in developing countries: an economic study of the manpower issues, with particular reference to Africa.

JOHN R. KERR, D.B.A. Indiana 1966. Future expenditure patterns of young families.

HERBERT J. KIESLING, Ph.D. Harvard 1966. Measuring the local government services: a study of efficiency of school districts in New York State.

SUN KEE KIM, Ph.D. Southern California 1965. A solution to the problem of food shortages in Korea.

DAVID T. KOLLAT, D.B.A. Indiana 1966. A study of unplanned purchasing in self-service food supermarkets.

PETER J. MCLURE, D.B.A. Indiana 1966. An analysis of consumers' images of major appliances and brands of appliances in terms of four forms of segmentation.

KONG K. RO, Ph.D. Yale 1966. A statistical study of factors affecting the unit cost of short-term hospital care.

CARLOS H. RUIZ-TAGLE, Ph.D. Harvard 1966. Population concentration and economic development: the Chilean case.

JOSEPH H. SCHMALTZ II, Ph.D. Chicago 1965. Mixing diets under demand uncertainty.

JAMES H. SCHULZ, Ph.D. Yale 1966. The future economic circumstances of the aged: a simulation projection of aged pension income and asset distributions—1980.

ROBERT A. SCHWARTZ, Ph.D. Columbia 1966. Private philanthropic contribution—an economic analysis.

NABEEL SHAATH, Ph.D. Pennsylvania 1965. Education high level manpower and the economic development of the United Arab Republic.

ALASDAIR M. SINCLAIR, Ph.D. Harvard 1966. Internal migration in Canada, 1871-1951.

BOONTAM SOMBOONSONG, Ph.D. Pennsylvania 1965. Diffusion of consumer durable goods in Thailand: a case study of the Bangkok-Thonbun Metropolitan Area.

MERLIN C. SPENCER, D.B.A. Indiana 1966. Family decision making under uncertainty: an

exploration of the development of food shopping patterns by families new in a community.

MEENAKSHISUNDER VENKATESAN, Ph.D. Minnesota 1965. An experimental investigation in the conditions producing conformity to and independence of group norms in consumer behavior.

An attempt to determine the conditions under which an individual yields to group norms in the consumer decision-making process. It further encompasses the conditions facilitating nonyielding behavior in a decision-making situation.

ARNOLD A. WEINSTEIN, Ph.D. Virginia 1965. Transitivity of preference: a comparison between age groups.

FINIS WELCH, Ph.D. Chicago 1966. The determinants of the return to schooling in rural farm areas, 1959.

The American Economic Review

Volume LVI DECEMBER 1966 Number 5

ECONOMIC-DEMOGRAPHIC INTERACTIONS AND LONG SWINGS IN ECONOMIC GROWTH

By RICHARD A. EASTERLIN*

The slackened growth rate of the economy in the cycles of 1953-57 and 1957-60 has stimulated increased interest in the hypothesis of long swings in economic growth or "Kuznets cycles"—fluctuations which, in the past, have typically extended over periods ranging from 15 to 25 years. Several writers have pointed out similarities between recent experience and earlier lapses from full employment growth.[1] Others, however, have taken sharp issue with such views [9, 27, 54]. And two recent methodological analyses have reached agnostic conclusions about the validity of the long-swings concept [6, 10].

The main support for the concept so far has been empirical in nature. A number of scholars working with processed and unprocessed time series relating to various economic magnitudes have been struck by substantial swings in the data.[2] Quite a few remarked on such swings well before there was any explicit formulation of the concept itself. A variety of countries are represented in this work—among them, the United States, Great Britain, France, Sweden, Germany, Japan, Australia, and Canada, as well as some underdeveloped countries [8]. But the gnawing question of "why" the swings has received much less study. The reason for this is apparent: satisfactory theoret-

* The author is professor of economics, University of Pennsylvania, and member of the research staff, National Bureau of Economic Research. This paper is part of a NBER study, conceived within the framework of a broader inquiry being conducted by Moses Abramovitz, and has not undergone the regular NBER review procedures. The project has been supported in part by a contract with the U.S. Department of Labor, Office of Manpower, Automation, and Training. The comments of Abramovitz and the referee of this *Review* are especially acknowledged. Helpful observations at various points in the preparation of the paper were also made by, among others, Gary S. Becker, Arthur I. Bloomfield, Irwin Friend, F. Thomas Juster, Irving B. Kravis, Robert E. Lipsey, Jacob Mincer, Marc L. Nerlove, Almarin Phillips, Sidney Weintraub, Maurice Wilkinson, and participants in the Purdue University Seminar on the Use of Economic Theory and Statistics in Economic History. The paper has benefitted from the excellent assistance of Chantal de Molliens Dubrin. The charts were drawn by H. Irving Forman.

[1] Cf. [3, 4, 9, 16, 17, 31, 32, 35, 36, 69, 72].

[2] For bibliography see [2-4, 22, 68] as well as that contained herein.

ical analysis has had to wait for fuller evidence on the nature of th swings themselves. And as such evidence has accumulated, repetitiv patterns have come to light and certain theoretical suggestions hav been advanced regarding the mechanism of the swings, most notably i the works of Abramovitz, Hickman, and Kuznets [2, 36, 41].

The purpose of the present paper is to develop more fully one o these possible mechanisms and to consider its relevance to past, pres ent, and prospective experience. The mechanism centers on interac tions between economic and demographic phenomena.[3] The fact tha demographic series frequently show pronounced longer term fluctua tions suggests that this may be a particularly auspicious area for in quiry. Moreover, demographic variables are largely or wholly omitted from extant theoretical models of economic fluctuations. Before dis missing long swings as a statistical illusion, the model-builder migh check the efficacy of his arsenal for the explanation of the observed waves of population growth, which, *ex post*, have been widely recog nized as relevant to economic experience.

The next section offers some brief comments on the nature of the empirical evidence relating to long swings and summarizes some of the principal characteristics which have been observed. The analytical model of economic-demographic relations is then developed, followed by discussion of its relevance to actual and prospective experience The discussion is throughout unfinished, in keeping both with the state of current knowledge and the intent of the paper. Whether long swings exist as an independent phenomenon is uncertain. The present writer has found the concept to provide a useful framework for analysis [20], and certain theoretical and empirical considerations militate in its favor. The aim of this paper is to note some of these considerations and thereby to contribute to further thinking and research.

I. *Some Observations on the Evidence*

Statistical Method. If one studies *unprocessed* annual data for incontrovertible evidence of long swings in all economic time series, he will not find it. In particular, most output series, in which processes of inventory change play an important role, exhibit such high annual variability that the longer term movement, if it exists, is far from obvious. On the other hand, there are series that move only sluggishly from year to year, but show pronounced longer term fluctuations. Some of these are illustrated in Figure 1. While one would not immediately

[3] Recent papers by J. Parry Lewis and Manuel Gottlieb overlap the scope of the present paper [33, 50]. A major study by Lewis centering on the building cycle provides a detailed examination of British experience [49]. I am grateful to Professor Lewis for the opportunity of seeing a pre-publication copy.

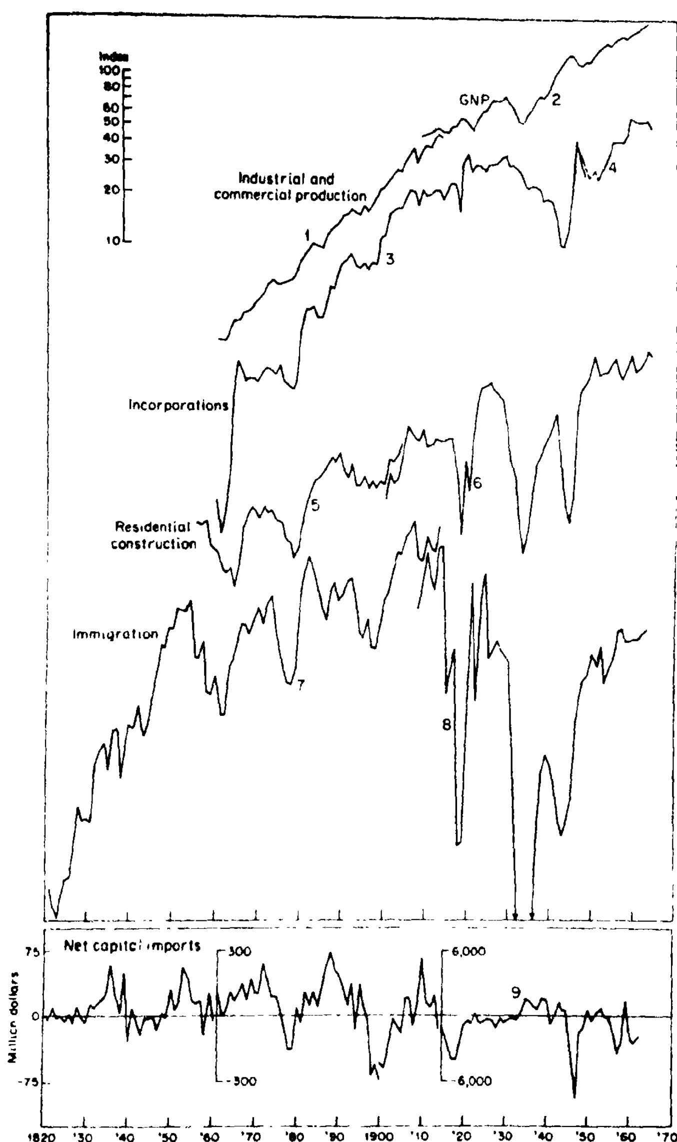


FIGURE 1. OUTPUT, INCORPORATIONS, RESIDENTIAL CONSTRUCTION, IMMIGRATION, AND NET CAPITAL IMPORTS, ANNUALLY, 1820-1964

Legend:

1. Index of industrial and commercial production, 1899 weights (Frickey).
2. Gross national product, billions of 1954 dollars (Commerce).
3. Aggregate index of incorporations (Evans).
4. Number of incorporations (Dun and Bradstreet).
5. Production of nonfarm housekeeping dwelling units (Gottlieb).
6. Number of nonfarm dwelling units started (Blank).
7. Gross alien immigration per thousand total population per year.
8. Net alien immigration per thousand total population per year.
9. Net capital imports, millions of current dollars.

identify long swings in the aggregate output series plotted there, such movements are manifest in the other series, particularly in the period before World War I, and show similar though not identical timing. These other series—residential construction, incorporations, immigration, and capital imports—derive from quite different statistical sources; hence the likelihood of a common bias in the estimation of the series is small.

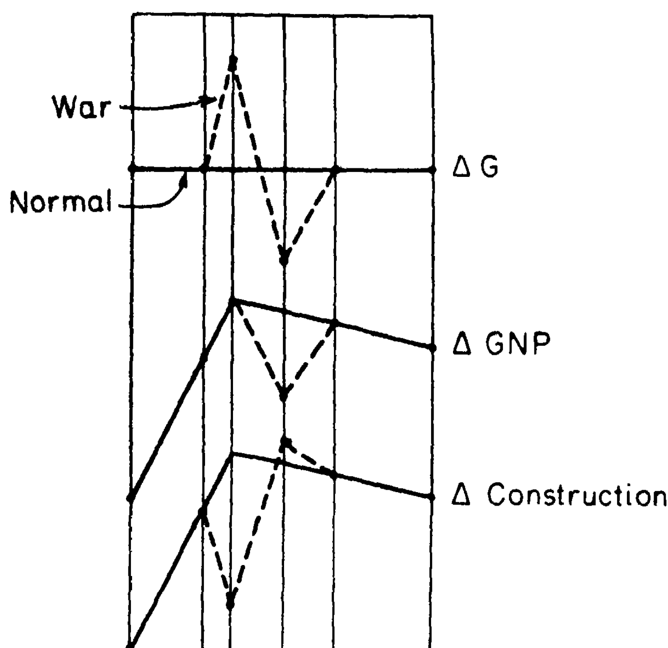
At this point one may plausibly ask: if one processes the data so as to reduce short term variability, do the longer term movements evident in the raw data for some series appear in others as well? The answer to this question is yes, the movements appear in a number of other series, though not all. The routes by which this answer is reached are almost as numerous as the investigators, each one of whom would doubtless have been grateful if supplied with a foolproof statistical technique for processing the data. However, while the different methods of statistical smoothing tend to result in a common answer to the basic question, they yield somewhat different conclusions on more demanding questions such as those of frequency, timing, and amplitude. Hence differences in method affect one's conclusions, and there is at the present time no widely accepted catalogue of long swing measures comparable to those of the NBER for the regular business cycle.

Moreover, for the period since 1914, statistical analysis of time series for evidence of long swings meets with an additional complication. The immense expansion and subsequent contraction of government expenditure associated with the two World Wars resulted in a major departure from the historical long-swing pattern.[4] A central feature of this pattern, one reason for which is suggested in Part II, was concurrent swings in the growth rates of GNP and aggregate construction. However, the two wars, with their attendant restrictions on construction, forced a break in the pattern, with GNP moving in positive conformity to the expansion and subsequent contraction of government expenditure, and construction in inverse conformity. The result is to interject an additional fluctuation in the usual long-swing relation, differing in timing between the two series, as illustrated by the broken line in idealized fashion on the following page.

This additional fluctuation appears, of course, not only in these two series, but in others as well. It is only eliminated by a statistical procedure which averages together the intervals both of expansion and contraction of government expenditure, such as Kuznets' decade averages, perhaps the most severely criticized method.[5]

[4] It is possible that the Civil War is another case of this sort, but the data at present are not sufficiently complete to determine this.

[5] This paragraph perhaps meets the question raised by several discussants of Hickman's



One implication of these observations is that the issue of the "reality" of long swings is unlikely to be resolved, pro or con, by purely statistical studies, valuable and necessary though these are. To the extent one concentrates on output series relating primarily to the war-torn twentieth century, as has been true of the studies reaching agnostic conclusions, the more one is likely to be unconvinced. On the other hand, by emphasizing periods less disturbed by war and series in which the swings are prominent in unprocessed data, one may build a stronger case, though this inevitably leaves in doubt the question of current relevance. In any case, it is clear that statistical analysis needs to be complemented by economic analysis: are the observations on long swings consistent with a model of relationships based on economic theory? It is to the solution of this question that the present paper seeks to contribute.

If this point be granted, some tolerance on the side of methodology is perhaps warranted for the analyst of long swings. He is trying to piece together evidence on the nature and causes of a phenomenon, the presumptive duration of which lies in the neighborhood of two decades; observations on four or five swings therefore require series span-

paper [19, 27, 31], namely, whether experience since the 1930's is to be interpreted as one long swing or two shorter swings. The additional fluctuation is directly due to the movement in government spending (cf. [36, p. 493]), and is thus an interruption in the basic long swing.

ning a period of some 80 to 100 years. To secure observations for periods undisturbed by major wars the investigator is forced still further back into the nineteenth century. There are few continuous annual series spanning such a long stretch of years. If severe criteria of reliability are applied, perhaps there are none. In his effort to piece out the pattern, the investigator falls back on observations for some variables at more intermittent intervals, quinquennial or even decennial, which are to be used jointly with annual series for others. Under these circumstances, doctrinaire insistence on one technique or another seems idle; inevitably a fair amount of individual judgment is required, judgment influenced, among other things, by considerations of the reliability of the data and the desire to exploit the fragmentary evidence as fully as possible. Such considerations are often evaluated best by the individual closest to the data.[6] This is not to say that anything goes; undoubtedly improvements can and should be made in statistical procedures, and research along these lines is helpful to the long-swings analyst.[7]

Characteristics of Long Swings. Despite difficulties of the type just described, growing evidence points tentatively to certain repetitive features of Kuznets cycles. Those particularly relevant to the present inquiry may be briefly enumerated, bearing in mind that the evidence is not always as complete as one might desire and relates chiefly to U.S. experience.

1. A swing in the growth rate of nonagricultural output, and, as it comes to be dominated by nonagricultural activity, GNP, tends to be followed with a lag by a swing in additions to the stock of both capital and labor.

2. It is accompanied by a swing in the rate of change of the unemployment rate (inverted), and correspondingly lagged by a swing in the *level* of the unemployment rate [2, 3, 36].

3. A swing in the growth rate of construction tends to coincide with that of nonagricultural output; the level of construction correspondingly lags [1-3, 36].

4. Swings typically occur also in the growth of output and inputs in manufacturing, mining, public utilities, and a number of service sectors, but not in agriculture (except in the case of areas undergoing new settlement) and government. Within the sectors exhibiting Kuznets cycles, a tendency is apparent for industries more dependent on busi-

[6] To illustrate, Kuznets' use of overlapping decade averages arose from his reluctance to depart far from the decennial census which provided the benchmarks for the 19th century output estimates.

[7] Research along these lines is currently being conducted by Michio Hatanaka and E. Philip Howrey in connection with the Econometric Research Program, Princeton University.

ness investment demand to lead those more tied to household consumption and investment demand [22].

5. Although farm output does not usually show long swings, there is a tendency for farm prices and income to do so [14, 22, 55].

6. Spatially, swings in the nonfarm sector are widely diffused among the various geographic divisions of the country. They characteristically take the form of an urban development boom. However, any single urban area does not necessarily participate in every swing [22].

7. The swings have typically been characterized by waves of immigration and internal migration [21, 24, 61-66].

II. *The Analytical Model*

Although the intellectual progenitors of the model suggested here are as numerous as the references cited in this and other papers by the present writer, particular acknowledgement is due to contributions by Kuznets [41-45], Abramovitz [2-4], Isard [37, 38], and Burnham Campbell [15, 16]. The model proceeds in three steps: (1) the impact of aggregate demand on labor-market conditions, (2) the effect of labor-market conditions on the number and spending behavior of households, and (3) the reaction of the latter on aggregate demand.

Aggregate Demand and the Labor Market. Assume, to start with, that growth of GNP in the last business cycle or two occurred at less than the full employment growth rate and that the economy has accumulated excess capacity with regard to labor. One manifestation of this would be a tendency toward a rising unemployment rate at successive business cycle peaks (or troughs). Assume, next, that for some reason the rate of growth of private investment rises, producing a corresponding upsurge in the growth of aggregate demand, and that the higher level is sustained for some time. The spatial distribution of this increased demand and the employment opportunities created thereby would not necessarily conform to that of the existing labor supply. On the contrary, in a developing economy the locus of investment opportunities tends to be predominantly nonfarm, whereas the increase of the labor force through natural growth of the working age population is disproportionately farm.

The increased growth in aggregate product demand would have a corresponding effect on the demand for labor in the centers of economic activity favorably affected. Initially, one would expect the increased labor input requirements thereby created to be met locally through an appropriate movement in the rate of change of the unemployment rate, and perhaps via a rise in the growth rate of hours. Increasing tightness of the labor market would tend to be reflected too in an increase in the rate of change of wage rates. The longer this process continues, the

more likely it becomes that labor force growth in the favored ε be enhanced by migration in response to the new employment nities. The origins of the migration would be lower wage aı where in the country (notably the older farm areas) and/oı Labor force growth might also be raised by an increase in lal participation rates of reserve labor groups such as females, yoı older persons. The relative role of the several possible sourc creased labor input in satisfying the new demand would deper supply elasticities of the groups involved.

Reaction on Household Growth in Centers of Increased To the extent that increased labor input requirements are me employment or hours changes, one would usually expect the provided to household growth to be relatively mild, si beneficiaries of the increased flow of income are most like members of established households in the area. To be sure, tl ened growth rate in the immediately preceding cycle(s) an quently higher average unemployment rate, may have led to s ferring of marriage by young persons or, for those already mar living with their parents, postponing the establishment of a household. For these, the progressive improvement of laboı conditions would lead to implementation of decisions previo ferred. The longer the previous period of slack the more size quantitative effect of this would probably be. Moreover, if tl market impact were for some reason particularly favorε those in the household-forming ages, plans might be advan household growth accelerated through a general reduction in t age age of marriage and of household formation.

Typically, however, one would expect a more immediate a able reaction on household growth in the centers of increased if the increased labor input requirements were met via migratic for the latter add directly to household numbers at destination case the growth in persons employed is more nearly matched b responding growth in the number of households. This is the significance of the migration response: in contrast to other soı labor input it carries a much more sizeable implication for ho growth in the areas of increased labor demand.

Reaction of Household Growth on Aggregate Spending. Hoı newly established in an area generate demands, not only for l but for urban services generally as well as for the furnishing o home. They thereby tend to create an urban development booı residential construction, new municipal investment in services ated therewith, and new business investment in retailing and activities. (The mechanism might of course involve an interı stage in which new migrants displace previous residents in esta

areas and the latter are those immediately involved in new real estate development.) Thus the rise in households, typically associated with the migration flow, leads to further growth not only in consumer spending but in private and public investment as well, and it is this induced rise in the growth of aggregate demand which tends to sustain and prolong the expansion.

Why Kuznets Cycles Rather Than Business Cycles? There would be a multiplier effect on consumer spending, and possibly new business investment induced thereby, arising from already established households whose head or other members may have benefited from the new employment and income opportunities. A reaction of this type is standard equipment in theorizing about the regular business cycle. Why should the present chain of events, operating through household growth, result in a more protracted boom?[8]

One possible answer is based on the composition of the induced demand when household growth intervenes between the labor-market response and increased spending flowing therefrom. This view would emphasize the central importance of housing and real estate development and the associated stimulus to local government spending on municipal services and to business spending on retail services, many of which require a long gestation period to bring to fulfillment. This argument rests at bottom on special characteristics of the industries benefiting from the induced demand—on product supply conditions.

A second, and perhaps more compelling answer, is based on demand considerations. Marriage, household formation, and migration are critical decision points in the life cycle of an individual. Typically, they involve commitment to new and higher spending behavior over several years as a new home is established and a family started or settled, a commitment reflected in the substantial indebtedness to relatives and financial institutions typically incurred during this period of the life cycle.[9] On this basis, one might reason that in a population that experiences a substantial rise in the growth of households there will occur not only a shift in the composition of consumption but a rise in the propensity to consume as well. This would be sustained over several years, carrying over from one business cycle to the next, and would become exhausted only gradually. In effect, such long term spending commitments are geared to a notion of permanent income, and tend to be rather insensitive to short term income changes.

It is this feature of longer term spending commitments which may

[8] Charles Tiebout [67] has drawn a similar distinction between a long-run and short-run multiplier, with population growth playing a central role in the former.

[9] The most thorough study I have encountered of spending behavior in relation to life cycle stage (which is not synonymous with age) is that by Lansing and Morgan [46], though even in this the stages are very broad. Consumer panel studies should generate still more pertinent data. Ferber's article [28] provides a valuable survey.

constitute the analytical key to Kuznets cycle phenomena. The frequent association of such commitments with series most noticeably marked by long swings has been pointed out particularly by Abramovitz. Because the feature has so far received insufficient attention, his statement merits repetition in full:

> One common attribute of all these processes of resource development involving the movement of people from country to country and place to place, the formation of households and the birth of children, the foundations of business, and the investment of capital in highly durable forms is that they involve long-term decisions and commitments. Hence they pick up speed and come to fruition slowly and when they slow down, they are not easily or quickly set in motion again. They give rise, therefore, to long waves of resource development and output growth. These processes involving long-term decisions, on the other hand, respond only sluggishly to the impact of the ordinary short and mild business contractions. By contrast, the most prominent feature of short business cycles is a fluctuation in shorter term investment, particularly inventory investment [3, p. 414].

While the present concern is with household behavior, it is worth underscoring Abramovitz's point that various types of private business investment also involve longer term commitments. Perhaps the most notable of these arise in connection with the establishment of new businesses, a phenomenon which, judging from the long term series on new incorporations (Figure 1) exhibits long swings in the unprocessed annual data.[10] (With regard to the future, the growth in importance as spending units of various government bodies and private nonprofit institutions, both potentially significant sources of long term commitments, suggests the need for study of them as well.) This line of reasoning obviously leads to the general view that *Kuznets cycles may arise from mechanisms, such as that under discussion here, which result in a bunching of long term spending commitments.*[11]

The Effect of Major Wars on Kuznets Cycles. The analytical model sketched above starts with a private investment boom, but any source of sustained increase in the growth of aggregate demand could initiate the process, including government expenditure. In the case of major wars, however, the process is interrupted, because the war itself places constraints on new household formation and construction. At the same

[10] Research on investment spending by new firms (or, more generally, in relation to the "life cycle" of business units) is scarce. One valuable study is that by Bridge [13]; cf. also [25, 70].

[11] A discussion of spending behavior inevitably leads to the question of financing, and thus to the role of monetary and financial variables in long swings. While this subject is clearly "off-bounds," it may be noted that money supply, interest rates, prices, capital imports (Figure 1), and similar variables do show long swings [cf. 2, 18, 41, 49, 68].

time, a protracted war sets the stage for a postwar Kuznets cycle boom, by building up substantial backlog situations not only with regard to private and public investment, but household formation and consumer spending as well. Thus, a major war would tend, on the one hand, to interpose an additional fluctuation during which the growth of GNP and aggregate construction pursue divergent paths, and, on the other, if one smooths this fluctuation statistically, to yield a Kuznets cycle movement of more extended duration.

Are Kuznets Cycles Self-Generating? While the interaction between economic and demographic phenomena emphasized here is chiefly of a multiplier type, there are clearly elements of induced investment on the basis of which one might postulate a self-generating model of the multiplier-accelerator variety. Also, the positive association between labor market tightness and growth of aggregate demand implies the possibility of labor cost changes which may react on investment spending in an adverse manner, tending to reverse the movement. On the other hand, irregular events such as wars, legislative acts (e.g., regarding immigration and wages), and speculative capital movements have affected observed long swings. The relative weight of endogenous versus exogenous factors in initiating the swings is in my view an open question requiring further research.

But the value of the present model does not hinge on resolution of the self-generating issue. As Irma Adelman [5] and others have shown, the Klein-Goldberger model of the regular business cycle is so highly damped that it requires stochastic shocks to produce oscillations of the business cycle variety. The principal value of that model lies in suggesting mechanisms which tend to produce oscillatory movements in response to such shocks. Similarly, while the present analytical scheme leaves open the question whether Kuznets cycles may be wholly self-generating or require either small or strong shocks, it does suggest a mechanism that tends to produce cumulative upward or downward movements over periods longer than the ordinary business cycle.[12]

Long Swings and Secular Growth. How, if at all, are Kuznets cycles related to the primary trend? Is the primary trend itself merely the path traced out by averages of successive Kuznets cycles? The answer suggested here is no. The primary trend reflects processes of technological change and of human resource and institutional development

[12] Clearly, the present analysis does not agree with the view proposed in a subsequent paper [7] by Mrs. Adelman that a shocked Klein-Goldberger *business cycle* model may be sufficient to account for observed long swings. In this connection, it is worth noting that the simulated long swings that she obtained were of smaller amplitude than those actually observed, and that the Klein-Goldberger model does not incorporate economic-demographic interactions of the type described here.

which lie outside the long-swing mechanism itself. However, in an economy where the free market system plays an important role in resource allocation, the pace at which new technology is adopted and diffused tends to be modified by a Kuznets cycle mechanism. This is because efficient use of the new techniques flowing from the Industrial Revolution and its sequel has required increased geographic concentration of productive resources. This has created an imbalance between the geographic distribution of the growth of labor demand, arising from the industrialization process, and that of labor supply, which typically reflects the widespread population dispersion characteristic of an agricultural society. Successful adoption of modern industrial technology has required, therefore, redistribution of the population in conformity with the needs of the new technological basis of production, and the provision of the requisite goods and services for this relocated population. The induced stage of the Kuznets cycle mechanism, an urban development boom, is essentially the accomplishment of this requirement in a free market economy by a multiplier-type process.[13]

By way of contrast, this formulation may be compared with Isard's [37, 38; see also 58]. He sees the swings as arising from successive transport innovations, and the urbanization facet as a consequence of the impact of such innovations on the location of economic activity. The present view rests basically on the geographic imbalance between labor demands and supplies created by modern technology generally, and does not require a new transport innovation either to initiate the swing or to account for urbanization, though such innovations may, of course, play a part in particular times and places.

An implication of the present view is that various characteristics of long swings tend to change secularly. The growth opportunities constituting the focus of investment at an early stage of development will differ from those at a later stage; hence variations will tend to occur through time in the industrial characteristics and geographical distribution of the swing. For example, agricultural settlement or exports might loom large among the investment opportunities in the early growth stages; at a later time, domestic manufacturing may predominate. Similarly the potential sources of labor supply tend to change secularly. Immigrants may move more readily from one country at one time; from another, at a different time. The reservoir afforded by the farm population in older agricultural areas grows to a point but eventually begins to be depleted, and is perhaps increasingly replaced by

[13] The above aims to identify those conditions which characteristically underlie the observed association between modern economic growth and population redistribution. In certain cases, of which the United States is one, modern economic growth may, in addition, involve the opening up and settlement of new territory, and thus a further stimulus to population redistribution.

some of the nonfarm centers gradually being bypassed in the development process. Secular growth in family income and the appearance of innovations such as the automobile will alter the composition of induced household consumption expenditure—recent suburban versus earlier urban residential development is an illustration.

The analytical scheme suggested here bears a surface resemblance to Schumpeter's [57], for it implies that growth in a free market economy tends to proceed in irregular surges. However, the duration of the movement differs from any contemplated by Schumpeter, as does the underlying mechanism of economic-demographic interactions. Moreover, with regard to secular growth, the present view emphasizes the independent significance of processses such as technological change and human resource development. However, Alvin Hansen, in modifying Schumpter's conception to incorporate the building cycle [34], moved in the Kuznets cycle direction. This step was carried logically forward by Isard, who arrived at a view similar in a number of respects to that advanced here. In so doing, Isard explicitly rejected Hansen's secular stagnation thesis, which one suspects, could be shown to run, in one form or another, a conforming Kuznets cycle of its own in economic discussion. Indeed, in striking contrast to the contemporary professional consensus, Isard, writing in 1942, predicted a postwar boom. His reasoning, however, gave undue weight to transport innovation which as has been noted, is not an essential ingredient of the present analysis.

The present analysis may also bear on the problem of delimiting a single phase in the emergence of modern economic growth as an "industrial revolution," "take-off" [56] or "great spurt of industrialization" [30]. The application of such concepts to actual experience has frequently confronted one with the difficulty of deciding among competing periods for such a designation. And, where identified, the duration and other characteristics of the period sometimes correspond to those of Kuznets cycles. It is at least a plausible hypothesis that such an interval, seized upon as a unique phase may be an early and particularly prominent long swing.

III. *Recent Experience in the Light of the Model*

Viewed in historical perspective, there are a number of similarities between recent and longer term experience, broadly consistent with the crude analytical model sketched above. There are differences, too, however, and on the demographic side, these are so serious as to raise doubts about the model's validity. The present section, after noting some of the similarities, focuses on recent departures with regard to demographic aspects and attempts to reconcile these with the model.

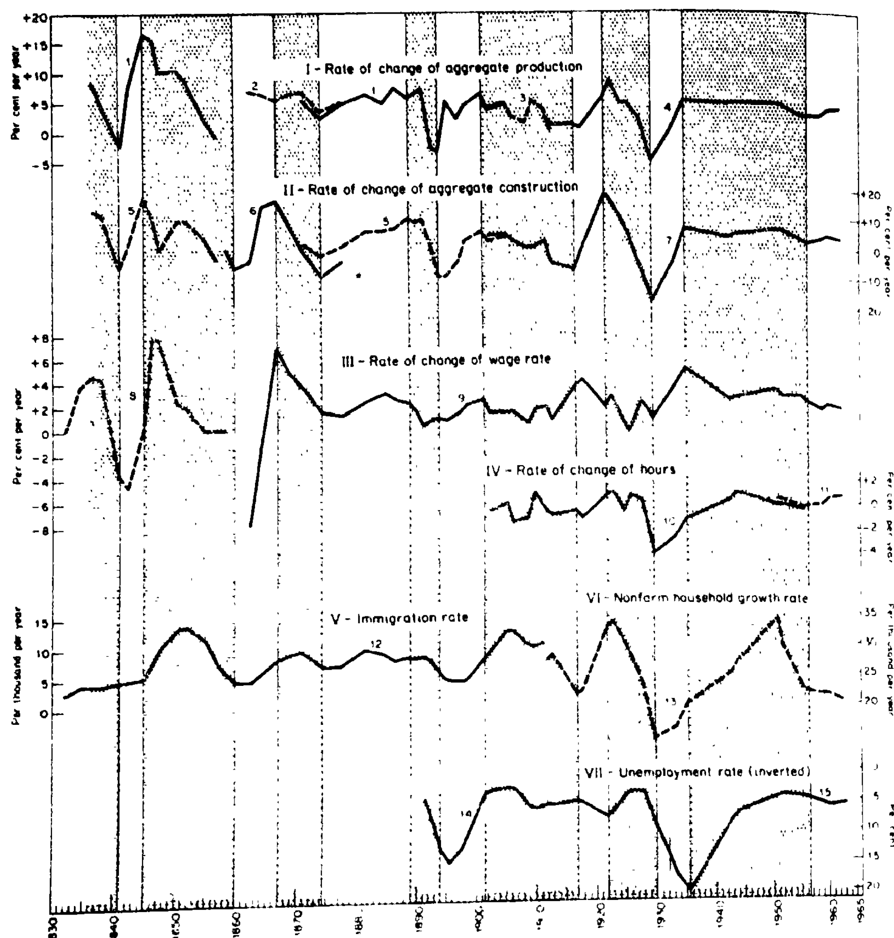


FIGURE 2. AVERAGE (CENTERED) DURING NBER REFERENCE CYCLES OF ANNUAL RATE OF CHANGE OF AGGREGATE PRODUCTION, AGGREGATE CONSTRUCTION, WAGE RATE, AND HOURS, AND OF LEVEL OF IMMIGRATION RATE, NONFARM HOUSEHOLD GROWTH RATE, AND UNEMPLOYMENT RATE, 1830-1964

Note: To facilitate visual comparison of the series, vertical lines have been drawn through the long swing peaks and troughs of the rate of change in aggregate construction, and the area from peak to trough has been shaded.

(Legend continued on following page)

Parallels between Recent and Earlier Experience. The analytical framework suggests the likelihood of longer term fluctuations with roughly similar timing in output growth, labor-market conditions, and household growth, particularly in the nonagricultural sector. For various reasons, other, perhaps independent, movements might occur in these series, but a minimum expectation would be that there would be some semblance of a common longer term rhythm. Is this the case?

Figure 2 assembles series relating to a century or more of U.S. expe-

Legend:

I. Rate of change of aggregate production, per cent per year.
   1. Nonperishable commodity output, 1860 dollars (Gallman).
   2. Index of industrial and commercial production, 1899 weights (Frickey).
   3. Gross national product, 1929 dollars (Kendrick).
   4. Gross national product, 1954 dollars (Commerce).

II. Rate of change of aggregate construction, per cent per year.
   5. Gross new construction, 1860 dollars (Gallman).
   6. Index of physical volume of construction (Abramovitz).
   7. Gross new construction, 1929 dollars (Kuznets).

III. Rate of change of wage rate, per cent per year.
   8. Money daily wages, Erie Canal (Smith).
   9. Real hourly wage, manufacturing, 1957 cents (Long).

IV. Rate of change of average hours of work per week in manufacturing, per cent per year.
   10. Jones.
   11. Bureau of Labor Statistics.

V. Gross alien immigration rate per thousand total population per year.
   12. Immigration and Naturalization Service.

VI Rate of change of nonfarm households per thousand nonfarm households per year.
   13. Bureau of the Census.

VII. Unemployment rate, per cent of civilian labor force.
   14. Lebergott.
   15. Bureau of Labor Statistics.

---

rience on growth of aggregate output and construction, indicators of changes in labor-market conditions (immigration pre-1914, hours, unemployment, wages), and in household growth (immigration pre-1914 and nonfarm households). A relatively mild smoothing technique has been employed. The values are averages for successive business cycles as identified by the NBER reference chronology, computed both for trough-to-trough and peak-to-peak cycles and plotted at the cycle midpoints. Only one departure was made from this procedure, a departure warranted by the analytical considerations presented previously. The special fluctuations associated with World Wars I and II have been smoothed by ignoring the wartime business cycle peak and first postwar trough.[14] In this way, the cycle average, by balancing the rise in government expenditure against its subsequent decline, becomes a more nearly postwar "normal."

While certain variations in timing exist, it is possible, as suggested by the shading in the figure, to identify swings in the growth rate of aggregate output and construction, labor-market conditions, and household formation, on a roughly one-to-one basis, thus lending some empirical plausibility to the model sketched above.[15] Some of the

[14] Thus, in the case of World War II the peak-to-peak average is for 1937-48 rather than for 1937-44 and 1944-48; and the trough-to-trough average for 1938-49 rather than 1938-46 and 1946-49.

[15] The existence of long swings in the rate of change of wages has been pointed out and analyzed by Clarence Long [51]. A statistically significant association between the annual rate of change of wages and that of unemployment was found by Bowen and Berry [11] in a study covering 1900-58.

swings are quite pronounced, such as those terminating in the 1890's and 1930's; others are milder, such as those ending around World War I and late 1950's. The parallel between the recent period and the experience in the first part of this century has been previously pointed out by Hickman [36]. As noted earlier, a tendency is apparent for the swings in the immigration rate, nonfarm household growth rate, and level of unemployment rate to lag the growth rate of aggregate output, though not without exception.

Several other similarities between recent experience and previous long swings may be noted (for evidence, see [22, 24, 63-66]. The growth rate of manufacturing capital shows a swing, with the capital goods industries tending to precede those oriented toward construction and urban development, as in the pre-1914 period. Spatially, the swing continues to center in nonfarm areas, appearing even more pronounced when more up-to-date census definitions are employed (e.g., metropolitan area rather than urban) and to exhibit wide diffusion among the different geographic divisions of the country. The postwar period has been characterized by an urban and suburban development boom. There has also occurred a swing in internal migration.

Differences in Demographic Aspects. There have, of course, been differences between recent and previous characteristics of long swings.[16] On the demographic side, these relate, not so much to movements in the aggregate variables, that is, growth of total population, total labor force, and total households, but to those in the underlying components of change. Disaggregation of population change into the component parts of births, deaths, and migration is a familiar one. An analogous procedure has been applied here with regard to labor force and household growth as well, as is suggested by the following tabulation:

Population	*Labor Force*	*Households*
1. Mortality rate	Mortality and aging	Mortality and aging
2. Net immigration rate	Net immigration	Net immigration
3. Fertility rate	Labor force participation rate	Household headship rate

With regard to labor force growth, the above components have often been identified in the literature, though they are not as rigorously established as in the case of population. With regard to household growth, I owe recognition of the possibility of the present partition scheme to Burnham Campbell [16] (see also [26]). A "headship rate" is defined, along lines identical with that for a labor force partic-

[16] For a more general discussion, see [3, 4, 36].

ipation rate, as the proportion of the population in a given demographic group (e.g., classified by age and sex) who are household heads.

All three of the aggregative demographic variables share in common mortality and migration components. The addition of the aging component to mortality in the case both of labor force and households is necessary because these variables are defined with reference to a population above some minimum age, and thus receive additions in the course of a given period as those initially below the limit grow older. Each aggregative variable also has its distinctive component—fertility rates in the case of population change; labor force participation rates, for labor force; and headship rates, for households. If one were dealing with a geographic subdivision of the national total—for example, the nonfarm sector—internal migration would of course be included with international migration as a component of change.

The alignment in the tabulation of the components underlying the three aggregative variables is designed with a view to facilitating the subsequent discussion. From the viewpoint of demography as such, the components of labor force and household growth on line 1 are analogous to natural increase of the population, i.e., the difference between components 1 and 3 of population growth.[17] The present grouping, however, aims to bring out parallel features with regard to long-swings analysis. Thus, the components on line 1 are largely exogenous to a given long swing; those on lines 2 and 3 are responsive, with line 2 representing a component common to all three variables (migration), and line 3, components which are distinctive. Moreover, since in the subsequent discussion the components on line 3 are frequently referred to collectively, it is convenient to designate each as the "rate" component of its respective aggregate, though this terminology is imprecise.

Figures 3-5 show, for the nationwide values of each of the aggregative variables, the relevant components of change for the period since 1870 or 1880.[18] The basic data—chiefly the decennial censuses—are

[17] A more elaborate analysis would, in fact, further subdivide the components of labor force and household growth on line 1 into, respectively, entries versus exits and formations versus dissolutions. A brief investigation, chiefly for 1930 onward, suggests that the movements shown here in the net balance are overwhelmingly due to the positive elements (entries and formations), just as those in natural increase are typically a reflection of births.

[18] An alternative, or at least complementary, disaggregation would have been desirable for the nonfarm sector alone, but was not feasible with the time and resources available.

A brief word on method: The treatment of population growth is of course standard; in the case of the other two variables, familiar issues, variously characterized as "index number" or "standardization" problems are involved. The solutions adopted here are not claimed to be definitive, though they seem logically consistent with the general analytical framework, but alternative procedures would not alter the present conclusions. With regard to the disaggregation of labor-force growth in a given decade, component 1 was obtained by multiplying the population change of the group due to sources other than current migration by its beginning-of-decade participation rate; the result is the change

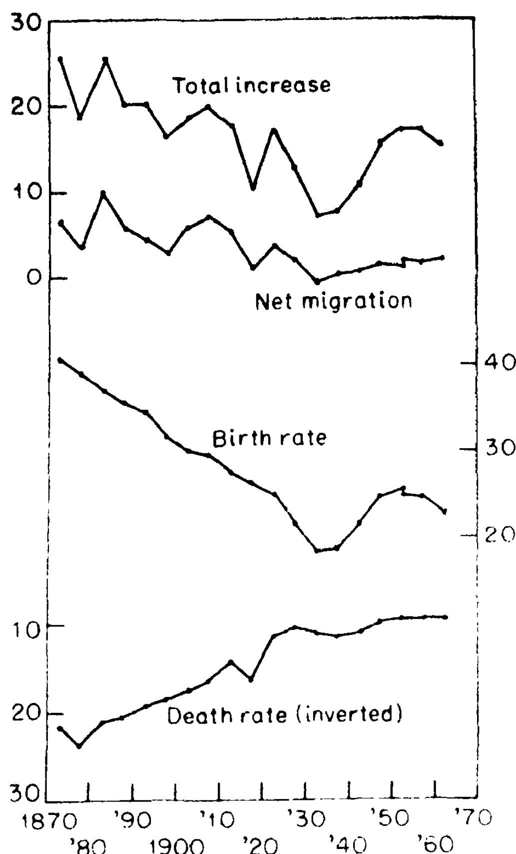


FIGURE 3. AVERAGE GROWTH RATE (CENTERED) OF POPULATION BY COMPONENT OF CHANGE, QUINQUENNIALLY, 1870-75 TO 1955-60, AND 1960-64
(per thousand per year)

too crude to permit a highly refined analysis relating to such questions as timing and amplitude; on the other hand, they clearly show the swings, and permit certain broad inferences pertinent to the issue under discussion, namely, a comparison between recent and earlier experience. All three figures show the predominant role of net immigration in the swing in the aggregate throughout most of the period

in labor force that would have occurred in the absence of migration or participation rate change. The contribution of participation rate change for each age-sex group is obtained by multiplying the end-of-decade population attributable to sources other than current migration by the excess of the end- over beginning-of-decade participation rates. The migration contribution is obtained as the product of the end-of-decade participation rate and population change due to net immigration during the decade. Thus computed, the components sum exactly to the actual labor-force growth. From 1940 on an identical procedure was used in disaggregating household growth except that headship rather than participation rates were used. Prior to 1940 it was necessary to follow a much more approximate procedure for households, described in the appendix.

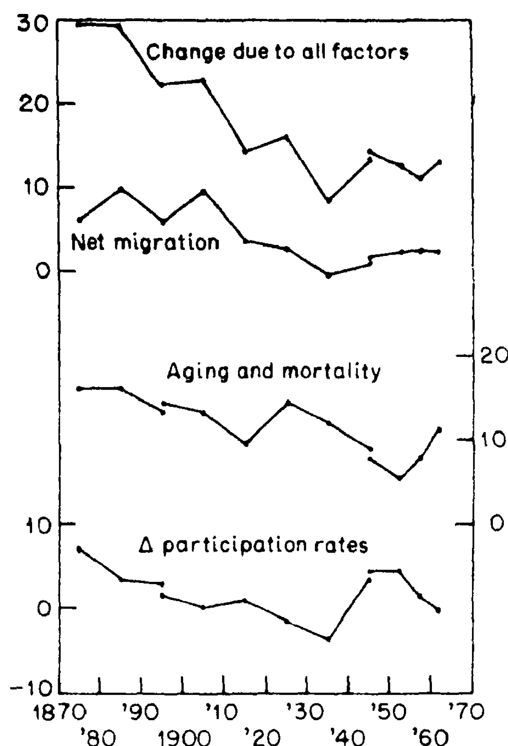


FIGURE 4. AVERAGE GROWTH RATE (CENTERED) OF LABOR FORCE BY COMPONENT OF CHANGE, DECENNIALLY, 1870-80 TO 1940-50, AND 1950-55, 1955-60 AND 1960-64
(per cent per decade)

covered, but for the swing from the 1930s through the late 1950s, the distinctive "rate" component for each variable takes charge. Indeed, the movement since 1940—up and then down—of the "rate" component in the three figures is startlingly alike. Clearly this raises a question whether the analytical model suggested in the previous section is indeed relevant to current experience. In that model migration played a key part, filling the dual role of labor-market response and source of household growth; what happens to the model when migration no longer enters into the swing in substantial magnitude?

In part, the answer to this is that migration in the form of *internal* migration does still enter, and to this extent the model is pertinent in its typical form. As noted, recent experience shows a continuation of the historical swings in internal migration [24, 63-66]. On the other hand, although temporal comparisons of the amplitude of internal migration are difficult to make, there seems to be little evidence that the current swing in the rate of internal migration reached greater dimensions than previously, as would be necessary if it were to compensate

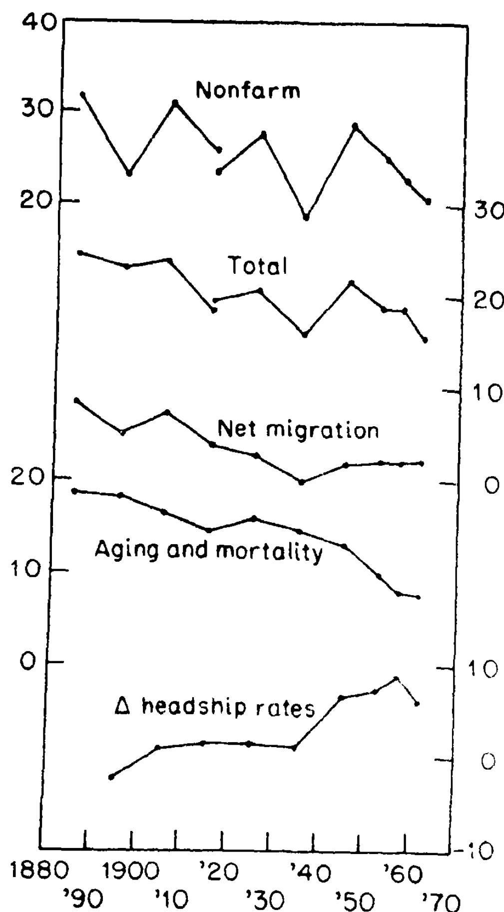


FIGURE 5. AVERAGE GROWTH RATE (CENTERED) OF NONFARM HOUSEHOLDS, AND OF TOTAL HOUSEHOLDS BY COMPONENT OF CHANGE, DECENNIALLY, 1880-90 TO 1940-50, AND 1950-55, 1955-60, AND 1960-64
(per cent per decade)

for the damped movement in international migration. Moreover, the actual swings in labor force and household growth, even at the nonfarm level, were undoubtedly due in substantial part to the unprecedented movements in the distinctive "rate" components, as well as to internal migration. Reconciliation of these "rate" movements with the analytical model is therefore essential in accounting for the quantitative features of the current swing.

Reasons for Recent Demographic Differences. The nature of the reconciliation is suggested by the model itself. It turns on the suggestion that the character of the labor-market response to a Kuznets cycle in the growth of aggregate demand depends on the nature of labor sup-

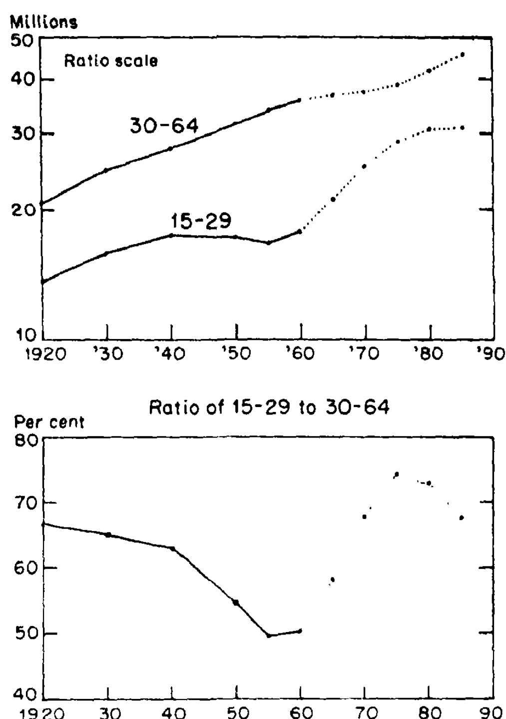


FIGURE 6. MALE POPULATION AGED 15-29 AND 30-64, ACTUAL AND PROJECTED, DECENNIALLY 1920 TO 1950, QUINQUENNIALLY, 1955 TO 1985

ply conditions. In the recent swing these conditions were unique in several respects. First, the historical response via immigration was severely curtailed by restrictions imposed since the mid-'twenties, as well as by World War II itself. Second, the contribution to labor force growth of the aging and mortality component reached an unprecedented low (Figure 4). This was an echo effect of the exceptionally rapid decline of the birth rate in the 'twenties and early 'thirties, and was particularly concentrated in the younger working ages, where, so far as one can judge, for the first time in recorded experience, the absolute number of persons aged 15-29 leveled off and actually declined slightly (Figure 6). The relative number, in relation to those aged 30-64, showed a substantial decline (Figure 6, lower panel). Finally, the educational level of younger persons compared to older was disproportionately higher in the recent period than usually (Figure 7). This arose from the rapid expansion of secondary education in the 1920's and 1930's, which temporarily enlarged the educational gap between young and old. While historical evidence is presently lacking, one suspects that it would be necessary to go fairly far back in the nineteenth

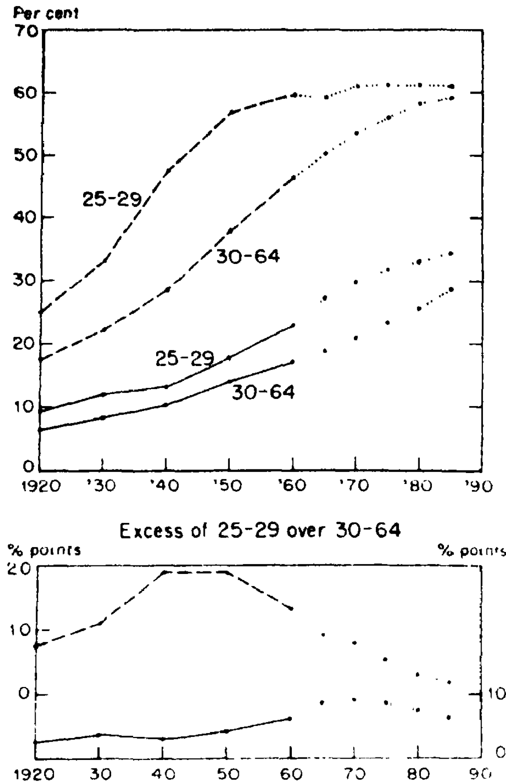


FIGURE 7. PROPORTION WITH 9-12 AND 13 OR MORE YEARS OF SCHOOL COMPLETED, POPULATION AGED 25-29 AND 30-64, ACTUAL AND PROJECTED, DECENNIALLY 1920-1960, AND QUINQUENNIALLY, 1965 TO 1985

– – – – – – 9-12 years completed

———————— 13+ years completed

century to find a comparable period of rapid acceleration in universal schooling.

In short, in the most recent Kuznets cycle upswing, the relative number of those in young working ages was unusually low, and the relative quality (so far as this depends on formal schooling) was unusually high. These young adults are precisely the ones who in the past played an important part in the quantitative adjustment of labor to the swing in the growth of aggregate demand, as is evidenced by their predominance in migration flows, whether internal or international. The unique labor supply conditions in the recent swing, however, resulted in an exceptionally tight labor market for those in the younger age groups, and gave rise to unusually favorable income growth (and, one suspects, as experience produced evidence of labor-market tightness to those involved, expectations of such growth). In other words, for younger persons, the labor market response took the form predomi-

nantly of a price rather than quantity adjustment.[19] This unusually favorable income situation for younger persons, in turn, induced a sharp reduction in age at marriage and household formation, and a sharp upswing in fertility. This accounts for the abrupt upsurge in the headship and fertility rates. At the same time, the quantitative shortage of younger persons redounded to the benefit of the only labor reserve group available in substantial numbers—older married women who were unencumbered by child-dependency responsibilities.[20] As a result, a substantial increase in the total labor force via participation rate change occurred for the first time. Thus, as in the past, an upswing in the growth of aggregate demand was accompanied by labor-market changes and an associated rise in the rate of growth of households which tended to sustain and prolong the boom. Because of the unique labor supply conditions, however, the mechanism of the association was substantially altered, with migration playing a less important role and the "rate" components of change in the demographic variables playing a leading one.

IV. *The Future: Implications of an Echo Effect*

One of the intriguing features of population growth is that it contains within itself a mechanism capable of generating longer term swings. Thus, writing in 1937 about German experience, August Losch states [53, p. 650]:

> While the movement of population shows unimportant and irregular [short term] fluctuations which seem to be mere reflections of economic, sanitary and other conditions, we find quite clearly great waves, the main cause of which are the great wars. The deficit of births during a war and the surplus of births in the immediate postwar period repeat themselves about thirty-three years later, when the new generations are at their time of highest fertility. For the same reason thirty-three years later a third wave occurs.

Recognition of this leads naturally to the thought that long swings in economic activity may have their origins in the peculiar structural mechanism of population growth. The present analysis of past U. S. experience, however, does not support this view. Fluctuations in demographic variables have typically arisen from movements in immigration or the "rate" components of change rather than from an echo effect

[19] Historical evidence on income by age is even harder to come by than that relating to quantity adjustments, but various indications in support of this statement are provided in two of my associated papers [20, 23]. The recent estimates of income by age for 1941 by Dorothy S. Brady [12] provide an invaluable prewar benchmark.

[20] Elaboration of the statements relating to fertility and household formation is provided in the papers just cited; participation rate change is the subject of a paper now in preparation.

of a surge in births, operating through the aging and mortality component.[21] Such demographic fluctuations were induced by, rather than initiated, changes in economic activity, although in turn they had important feedback effects. This is not to say that no echo effects at all existed. One can, in fact, trace them in detailed data, but in the past they have been mild, reflecting the fact that before 1940 long swings in fertility were confined to the rate of change rather than to the level of that variable.

Now, however, things have changed. The new demographic developments accompanying the most recent upswing, notably, the dramatic baby boom, have in turn set up an echo effect, currently being felt in the form of a sharp upsurge in the young adult population. What, if any, are the implications of this echo effect for future long swings? Has the economy, perhaps, shifted to a new situation in which long swings are at least partly built-in via a natural demographic cycle, even though this was not the case in the past?

A satisfactory analysis of this question, as of so many on which this paper has touched, calls for a more complete model of long swings. Nevertheless, some tentative impressions may be gained by looking in some detail at the impact of the echo effect on labor force and household growth currently and in the coming decade. Moreover, this is of interest in its own right because of its possible bearing on attempts to assess more generally the outlook for the next decade.[22]

The Next Decade. To start with labor supply conditions, the echo effect of the baby boom is currently yielding its fruits in the form of an unprecedented growth in the labor force from the aging and mortality component. Since the first half of the 1950's, the rate at which this source has been contributing to labor force growth has risen from around 5 to 11 per cent per decade, and the projected figure for the last half of the 'sixties is almost 14 per cent. This means that the component of aging and mortality alone will yield in the late 60's a growth rate of the total labor force as high as that *due to all components* in the 1940's and higher than that in the 1950's. The impact of this is concentrated among the younger age groups, with the result that the relative number of those aged 15-29 to 30-64 is now undergoing a rise

[21] The rise between 1910-20 and 1920-30 in the aging and mortality component, which contributed to a corresponding movement in labor-force and household growth, appears to contradict this observation (Figures 4 and 5). This is primarily due, however, not to an earlier upsurge in the birth rate, but to the fact that the 1910-20 level of this component was depressed sharply below normal by high mortality among young persons resulting from the flu epidemic of 1918. Thus, with reference to Figure 3, the movement is chiefly a reflection, not of a prior fluctuation in the fertility curve, but of a concurrent dip in the mortality curve (inverted), centering on 1910-20.

[22] Cf., e.g., [60]. Interestingly, this NICB study, while not adopting a long swings framework as such, looks at the next decade on the basis of experience in the past two, viewed not as a succession of short cycles, but as a major boom and ensuing retardation.

greater and faster than the preceding decline (Figure 6). Moreover, an independent but significant development is that the advantage of young over old in high school education has declined sharply in the last decade and will decline somewhat further in the next (Figure 7). With regard to college education, the advantage of the young rose somewhat through 1965, but shows little prospect of further increase. On the whole, therefore, a substantial reversal of the preceding period's situation with regard to relative quantity and quality of younger versus older persons is now in progress.

The marked impact of the aging and mortality component on labor force growth suggests that the labor requirements of an upswing in the growth of aggregate demand might for the first time be met very largely from this component rather than from either increased immigration or participation rate change. (One would expect also, of course, a response via internal migration, perhaps taking more the form of a movement among nonfarm centers as the farm reservoir becomes even further depleted.) Would this rather different labor market response be accompanied by an upsurge in household growth as in the past?

One's initial reaction is to say "of course," for the same demographic echo effect that is operating to raise labor force growth is also acting to increase household growth. On investigation, however, several mitigating circumstances come to light.

First, there is a difference in the timing and shape of the impact of the demographic movement on labor force versus household growth. The average age at labor force entry is lower than that of household formation, so that an echo effect tends to raise labor force growth in advance of household growth. This is illustrated in Figure 8, where the contributions of population change to labor force and household growth since the 1930's are compared.[23] Note that the movement in household growth tends to lag that in labor force growth, troughing and then peaking roughly five years later. Moreover, the rise in household growth is somewhat smaller in amplitude and less precipitous than that in labor force growth. This is because among young males the rise in rates of household headship not only centers at a later age than that in labor force participation, but is also less abrupt. As a result, the temporal distribution of an echo effect on household growth is more attenuated than that on labor force growth. Thus while the baby boom does produce an echo effect tending to raise household as well as labor force growth, the former tends to lag the latter and be slightly more spread out over time.

Second, for an echo effect in the population component of change to

[23] For convenience of exposition, immigration has here been combined with the aging and mortality effect. The point remains valid if confined to aging and mortality alone since the movements during the period shown are due almost entirely to this component.

raise the *total* growth rate of households—that due to all sources—it is necessary that the headship rate component not decline in compensating fashion. Since the analysis for 1940-55 has already suggested the possibility of a mechanism causing the population and "rate" components to move in at least partly offsetting fashion, it is essential to appraise the outlook for the rate component as well as for the population component. This is facilitated by Figure 9, which for labor force and households repeats the curves of Figure 8 and adds those showing growth due to all sources, actual through 1960-64 and projected through 1975-80.[24] For each variable, the vertical difference between the two curves represents the contribution of the rate component.

The startling feature of Figure 9 is that while the echo effect is currently producing and is projected to produce a sharp rise in the growth rate of labor force, an upsurge of comparable magnitude in that of households, even with a lag, is not foreseen. In the case of labor force, to offset completely the echo effect, the contribution of the "rate" component would have to drop to a negative value, that is, participation rates would, on the average, have to decline. This is not the case with regard to households, however. The contribution of the "rate" component has been so high in the past that a drop to a lower positive value, implying merely a retardation in the average rise of headship rates, is sufficient almost wholly to offset the echo effect.

Is a slowing of the rise in headship rates, such as that implied by the projection, likely? Actual experience in the first half of this decade compared with 1955-60 does show such a tendency. Although further study is needed, two considerations suggesting the likelihood of its continuation may be noted, one endogenous and one exogenous. First, even under relatively high employment conditions, the change in relative quality and quantity of young workers now taking place means that they are unlikely to enjoy an excess demand situation comparable to that of 1940-55. Hence, it seems doubtful that a significant contribution to household growth through increases in headship rates will occur among this group. Second, an important factor raising household growth in the past fifteen years has been the noticeable rise in headship rates among the older population, that aged 55 and over, and it is questionable that this will continue at the same pace. A principal factor in this has been the rapid extension of social security coverage. The percentage receiving OASDI benefits in the relevant population groups provides a rough though imperfect indication:[25]

[24] For households, series B projections are used, but the use of series A would not alter the present conclusions, since from 1965 on the growth rates implied by the two projections differ very little.

[25] I am grateful to Francisco Bayo, of the Social Security Administration, for the absolute data on beneficiaries, on the basis of which these percentages were computed.

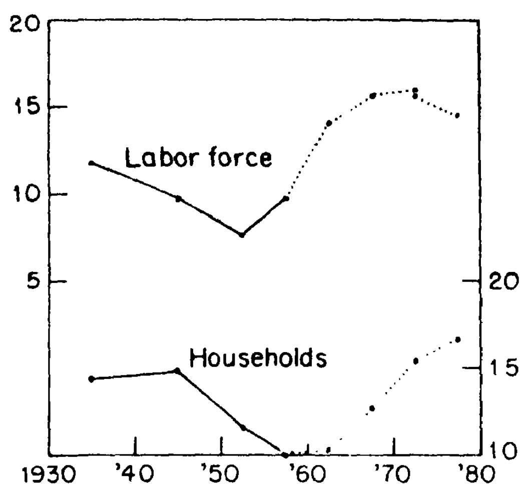


FIGURE 8. CONTRIBUTION OF POPULATION CHANGE TO LABOR FORCE AND HOUSEHOLD GROWTH, ACTUAL AND PROJECTED, DECENNIALLY, 1930-40 AND 1940-50; QUINQUENNIALLY, 1950-55 TO 1975-80
(per cent per decade)

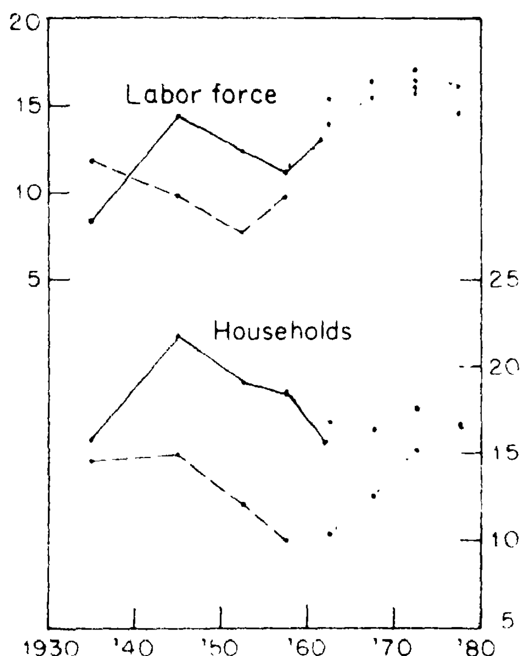


FIGURE 9. AVERAGE GROWTH RATE (CENTERED) OF LABOR FORCE AND HOUSEHOLDS DUE TO ALL SOURCES AND TO POPULATION GROWTH ALONE, ACTUAL 1930-40, '40-50, '50-55, '55-60, AND '60-64, AND PROJECTED, QUINQUENNIALLY, 1960-65 TO 1975-80
(per cent per decade)

——————— Growth due to all sources
— — — — — Growth due to population change

	1950	1963	1975 (projected)
Males, aged 60-64	0	16	22
Males, aged 65+	25	78	89
Females, aged 60-64	0	29	29
Females, aged 65+	17	72	90

Although projections relating to such politically sensitive areas are patently hazardous, the figures provide some warrant for the view that the economy has largely passed through the period when the adoption of social security would have its largest impact on headship rates among the older population. While some additional growth due to this source is probable, its quantitative importance seems likely to be less than in the immediate past.[26]

To sum up, although a more thorough investigation is needed, the present analysis makes clear that in the current and coming decade, contrary to what one might expect, an upswing in household growth is neither a simultaneous nor automatic accompaniment of a surge in labor force growth arising from the echo effect of the baby boom. True, there is an echo effect exerting an upward pressure on household growth, but it lags that in labor force growth and is more attenuated. In addition, there are circumstances, partly endogenous and partly exogenous, which may dampen this upward pressure by slowing the rise in headship rates among young and old, thereby reducing the contribution to household growth from the "rate" component source. It is far from clear, therefore, under such new circumstances of labor supply, where an echo effect supplies the labor requirements arising from an upswing in the growth of aggregate demand, that a substantial reinforcing stimulus to the boom from an upsurge in household growth like that in the past will occur.

Bearing on Longer Term Future. Though on first glance it is tempting to pronounce that emergence of an echo effect has created a new "built-in" long-swing mechanism, a more careful look at current changes and those in prospect over the next decade creates doubt that any such simple generalization is warranted. However, more study is

[26] This discussion bears on Edward F. Denison's criticism [19, p. 531] that ". . . the long-wave background gets [Burnham] Campbell no farther than others who have based forecasts of housing starts on the normal increase in households that would result from population changes in relevant age-sex groups. . . ." The present analysis suggests that not only the population component of household change should be studied, but also the "rate" component, and that the long swing approach may be of value in clarifying the movements in the "rate" component. The reference to OASDI, however, shows clearly that other factors are pertinent and that more intensive study is needed of the determinants of headship rate changes, including factors such as school enrollment and marital status. Clearly, throughout this paper, no attempt has been made to consider the possible bearing of the Viet Nam War.

needed of the determinants of household growth, including attention to "gross" changes (formations and dissolutions) and the expenditure implications thereof, by age. (Clearly, the expenditure implications of social security's forestalling dissolution of a home are likely to be quite different from those arising from accelerated household formation among young persons.) The relative weight of endogenous versus exogenous elements in labor force and household growth needs clarification too. For example, while the present analytical scheme suggests with regard to both labor force and household growth that there is a tendency for movements in the "rate" and population components to be at least partly offsetting, the dampening at present appears to be greater for household growth. Is this attributable wholly to exogenous circumstances or is it partly inherent in the mechanism? Moreover, perhaps undue attention has been given here to demographic reactions from the side of household growth. In commenting on this paper, Abramovitz points out that, in addition to such considerations, accelerated growth in labor force, for whatever reason, may act, at least permissively, to sustain a protracted boom by keeping the "natural rate" of output growth above the "warranted rate" for a longer period, thus postponing the time when an output ceiling may act as a brake on long term capital investment.

Certainly the emergence of demographic echo effects is a new phenomenon which must be reckoned with in assessing the longer term future. For example, another possibility, though not the only one, is that, on the one hand, an echo effect creates a tendency toward waves in labor force growth with a duration longer than Kuznets cycles, and, on the other, upswings in the growth of aggregate demand initiated by nondemographic factors occur with the frequency of past Kuznets cycles. Under such circumstances, the impact of the demand movement on the labor market and the reactions stemming therefrom could be highly variable, depending on whether the flood, ebb, or some intermediate stage of labor force growth was prevailing at the time. In this connection, it should be noted that the current decline in the crude birth rate has, in only eight years, wiped out the increase since 1940. (This is not true, however, of age-adjusted fertility measures.) Beyond 1975, this will produce a shift in relative numbers of young and old in a direction similar to that of the 'forties and early 'fifties (cf. Figure 6). If aggregate demand conditions are favorable, this creates the possibility of economic-demographic interactions at that time like those of the post-World War II era.

Finally, there is nothing in this analysis which implies that Kuznets cycles in the economy's growth rate are not susceptible to elimination via appropriate policy measures. If the emphasis on the central role of

aggregate demand is correct, then, as Abramovitz and Hickman have also suggested, manipulation of effective demand should make it possible largely to eliminate discernible long swings. But the analysis does imply that determination of the appropriate policy measures may call for more attention to variables and relationships of the type suggested here than has been customary in the past.

V. *Summary*

The present paper, building on the work of numerous predecessors, suggests that one of the mechanisms responsible for long swings in economic growth may involve interactions among aggregate demand, labor-market conditions, and household growth, with a feedback effect from the last to the first. An upswing in the growth of aggregate demand and the growing labor-market tightness induced thereby engenders accelerated marriage, migration, and household growth. These are critical decision points in the life cycle, entailing spending commitments extending over several years as a new home is established and a family started or settled. In effect, such commitments are geared to a notion of permanent income and thus tend to be relatively insensitive to short term income changes. A bunching of such commitments serves to cushion the economy against the usual business cycle and becomes exhausted only gradually.

Such a bunching of long term spending commitments may be the key to Kuznets cycle phenomena. This applies not only to decisions of households but other spending units as well, though no attempt is made here to examine changes in the population of business or governmental units. The investigation stops short too of exploring financial aspects of the swings as well as possible accelerator-type relationships. Whether Kuznets cycles may be self-generating is left open; no more is claimed than that a plausible mechanism has been identified which once initiated, for whatever reason, might help to sustain the economy over a period longer than the usual business cycle.

The paper touches too on the relation of this possible long-swing mechanism not only to business cycles but to secular development as well. Such swings are prevalent in the historical development of free market economies, it is suggested, because typically the adoption of modern industrial technology has required redistribution of the population from rural to urban areas and the provision of the requisite goods and services for this relocated population. The induced stage of the Kuznets cycle mechanism, an urban development boom, is essentially the accomplishment of this requirement in a free market economy by a multiplier-type process.

The analytical scheme creates the expectation that series on aggre-

gate output and construction, labor-market conditions, and household growth, particularly with regard to the nonagricultural sector, would show some semblance of longer term fluctuations with roughly similar timing, and U. S. experience over a century or more bears this out. The post-World War II boom and subsequent slackening in the economy's growth rate form a consistent part of this record. However, on the demographic side some important differences appear. In the recent period, swings in population, labor force, and household growth were chiefly due, not to an immigration swing as was typically the case in the past, but to movements in, respectively, the birth rate, labor-force participation rates, and headship rates. These new developments are attributed to the unusual labor supply conditions prevailing in this period. Young adults—those who in the past played a central role in the quantitative adjustment of labor to a swing in the growth of aggregate demand—were in exceptionally short supply and their educational advantage over older persons was unusually high. As a result they enjoyed an exceptionally favorable labor-market situation, and this, in turn sharply accelerated marriage, household headship, and fertility. Thus an upswing in the growth of aggregate demand induced via its labor market impact an increase in household growth, but through a different channel than in the past. At the same time, the quantitative shortage of younger persons redounded to the benefit of the only secondary labor force group available in substantial numbers—older married women unencumbered by small-child responsibilities—and led to an unprecedented rise in their labor-force participation rates.

The unique demographic developments of the most recent swing, notably the dramatic baby boom, have created a demographic situation in the current and coming decade different, in turn, from both the recent and more distant past. A sharp upsurge in the young adult population is occurring as an echo effect of the baby boom. Whereas the interpretation of previous U. S. experience presented here sees demographic swings as primarily induced by, rather than initiating, economic movements, the appearance for the first time of a substantial echo effect raises the question whether the economy has perhaps shifted to a new situation in which long swings are partly built in via a natural demographic cycle. An exploratory investigation for the current and coming decade suggests that with an echo effect primarily supplying the labor requirements of an upswing in the growth of aggregate demand, it is uncertain whether a substantial reinforcing stimulus from an upsurge in household growth will also occur. This is because, while the echo effect does exert an upward pressure on household growth, it occurs with a lag, and is dampened by circumstances reducing the rise in headship rates among young and old. Since these circumstances are

partly endogenous, involving an unfavorable shift in the relative number of young adults as a concomitant to the echo effect, any generalization about a built-in demographic long-swing mechanism seems premature in the absence of more intensive study. An alternative possibility, though not the only one, is for swings in the growth of aggregate demand to occur in conjunction with a longer duration movement in labor force growth via the echo effect. This would engender a variety of demand-supply combinations in the labor market, with correspondingly variable reactions stemming therefrom.

As for policy, the present analysis suggests that more attention to variables and relationships of the type suggested here may be desirable. If the emphasis on the central role of aggregate demand rather than supply conditions in initiating these movements is correct, then appropriate stabilization measures might eventually lead to the virtual extinction of Kuznets cycles.

Appendix: Sources and Methods for Figures 1-9

Figure 1

Series 1. [29, p. 127].

Series 2. *1900-28,* [89, pp. 138-39, T. I-16, 1. 5]; *1929-62,* [73 (Jan. 1964), p. 210, col. 1]; *1963,* [73 (Jan. 1965), p. 192, col. 1].

Series 3. *1860-1941,* [25, p. 34]; *1942-1948,* NBER files.

Series 4. NBER files.

Series 5. [1, pp. 151-52, series 21a].

Series 6. [1, pp. 151-53, series 21]. Extrapolated from 1959 to 1964 by annual per cent change in [73 (Jan. 1965), p. 234, col. 4].

Series 7. Immigration: [86, series C 88]. Population: *1820-68,* [86, series A-2]; *1869-1899,* [41, unpublished annual estimates underlying pp. 624-6, Table R-37, col. 9]; *1900-07,* [84, no. 250, p. 6], adjusted to include armed forces overseas by unpublished annual estimates underlying [41, *ibid.*, col. 8].

Series 8. Immigration: *1908-1945,* [45, p. 96, immigrants minus emigrants], extended to 1961 via [86, 87, series C-140 and C-156]. Population: *1908-16, 1920-29,* same as for series 7 for 1900-07; *1917-19, 1930-61,* [84, no. 250, p. 6, tables 2, 3].

Series 9. *1820-1957,* [86, sum of series U 185-190]; *1957-62,* [88, 1963, p. 854].

Figure 2

Series 1. Preliminary unpublished annual estimates (as of April 1964) in millions of 1860 dollars by Robert E. Gallman, underlying [71, pp. 27, 34]. Nonperishable commodity output is the sum of consumers' semi-durables and durables, manufactured producers' durables, and gross new construction.

Series 2. [29, p. 127].

Series 3. [40, pp. 293-4, col. 11].

Series 4. Same as for Figure 1, series 2.

Series 5. Same source as for series 1.

Series 6. [1, pp. 141-2, series 5, segment I].

Series 7. *1900-58,* [1, pp. 142-145, series 2]; extrapolated from 1954-58 to 1957-60 and 1958-61 assuming change in rates between successive periods was same as that shown by [73 (Jan. 1964), p. 210, col. 7, and, for 1960-64, 73 (Jan. 1965), p. 192, col. 7].

Series 8. [59, p. 303].

Series 9. *1900-58,* [51, pp. 150-51, col. 5]; *1959-64,* col. 1 of same source was extrapolated on basis of absolute annual change in [74 (March 1966), p. 59, col. 3], and col. 4 via [73 (Jan. 1965), p. 244, col. 1].

Series 10. [39, p. 375].

Series 11. [74 (April 1965), p. 33, col. 2]

Series 12. Same as for Figure 1, series 7.

Series 13. [86, 87, series A-243]; 1964 value from [83, no. 130, p. 1].

Series 14. *1890-99,* [47, p. 522]; *1900-32,* [90, p. 215].

Series 15. [74 (March 1966), p. 23].

Figure 3

The break in series in 1950-55 is due to the shift in sources described below.

1870-1955

Except as noted subsequently, the basic source was [42, p. 37, t. 1, col. 4; p. 39, t. 3, col. 5; p. 41, t. 5, col. 7; and p. 43, t. 6, col. 5 (underlying unrounded quinquennial estimates were used)]. However, for net migration, 1910-40 data were from [45, pp. 95-96, t. B-1]. For 1940-55, estimates for all series were revised somewhat, the chief differences from the original source being to include deaths to armed forces overseas and to keep the scope of the net migration estimate the same as for the pre-1940 period.

The sum of the components does not exactly equal total increase, because net migration refers to alien arrivals less departures, and thus includes some nonmigratory movements. Pure migration estimates are not available prior to 1910.

1950-64

[84, no. 302, p. 8]. Estimates for fiscal year were divided by Jan. 1 population to obtain annual rates, which were then averaged for periods shown.

Figure 4

For method of computing component contributions to total change, see text, footnote 18. Computations were done for most detailed classifications available (identified below for each period) with higher levels of aggregation being obtained by summation. The break in series in 1890-1900 is chiefly due to the replacement of a regional by an age classification in the calculations; in 1940-50, by a shift from census to Current Population Survey data.

1870-1900

The series covers all population classes except nonwhites other than Negroes. For 1870-1890, the calculations were done separately for native born and foreign born in each of the four census regions (no separation by sex was

possible); for 1890-1900, for native white, Negroes, and foreign born white, by sex, in each of the four regions. An overlap comparison for 1890-1900 of the pre- and post-1890 methods yielded similar results with regard to total U.S. change and the two sets of calculations were therefore treated as continuous. The sources of the basic data necessary for the calculations were as follows.

Labor force. *1870, 1880,* census reports. *1890, 1900,* tabulations of census data prepared by Ann R. Miller, University of Pennsylvania Population Studies Center. For the present study, the 1870 data were adjusted for underenumeration in the South as estimated by Alba M. Edwards [79, p. 141]; the 1890 data, for underenumeration of children aged 10-15 as described in [78, pp. lxvi-lxxi, cviii, clv].

Population. All dates from census reports. Data for 1870 were adjusted for underenumeration in South as estimated by Edwards, *op. cit.*

Migration. [48, Table P-1].

1890-1950

All classes of the population are covered. The calculations were done separately for each sex-age class of native whites, nonwhites, and foreign born whites. Through 1930 the population aged 10 and over is included, thereafter, that 14 and over. An overlap for 1920-30 indicates the negligible effect of eliminating those aged 10-13. Other age classes used were 14-19, 20-24, 25-44, 45-64, 65+.

Labor force. *1890-1940,* from unpublished unrounded data underlying [52, Appendix A]. *1950,* the estimate was built up from the census reports, and the total differs slightly from that shown by both Long and the census because it was obtained by summing components which included estimates. The latter were necessitated by lack of complete count data and/or sufficient component detail in the census.

Population. *1890-1940,* tabulations of census data by Everett S. Lee, University of Pennsylvania Population Studies Center. *1950,* data taken directly or estimated from census report.

Migration. [48, Table P-1]. Migration for those aged 14-19 was assumed same as that for those aged 15-19. Age groups 15-19 and 20-24 were from unpublished underlying data.

1940-64

Covers total population including armed forces overseas. The calculations were done separately for each sex-age class, but no color-nativity or geographic components were distinguished. The age classes were 14-19, 20-24, ten year groups through 55-64, and 65+, except that for the period after 1960, the 14-19 were subdivided into 14-15, 16-17, and 18-19.

Labor force. *1940,* [82, no. 2 (no date), p. 11]. Unpublished age detail comparable to 1950 was kindly provided by Gertrude Bancroft, Bureau of Labor Statistics. To these figures were added 150,000 members of the armed forces who were outside the continental United States in 1940. See *ibid.*, p. 1, and [74, vol. 6, no. 1 (July 1959), p. 1, n. 1]. The age-sex distribution for

his group is from [84, no. 98 (Aug. 13, 1954), pp. 7 and 15]. *1950,* [82, no. 85 (June 1958), p. 5]. Published estimates were adjusted upward in same way as for 1940. For explanation, see references cited for 1940. *1955,* [77, p. 53]. *1960 (excluding Alaska and Hawaii),* [74, vol. 7, no. 7 (Jan. 1961), p. xii, T. 2]. *1960 (including Alaska and Hawaii),* [75, p. 4]. *1964,* [76, p. 130].

Population, *1940,* [84, no. 98 (Aug. 13, 1954), p. 15]. *1950,* [*ibid.*, no. 146 (Nov. 12, 1956), p. 10]. *1955,* [77, p. 52]. *1960 (excluding Alaska and Hawaii),* unpublished estimates prepared with reference to 1950 census. These data, provided by Bureau of Labor Statistics, are consistent in latter regard with the 1960 labor force estimates excluding Alaska and Hawaii. Alaska and Hawaii were eliminated by deducting 1960 census data for these states. *1960 (including Alaska and Hawaii),* [75, p. 4]. *1964,* [76, p. 130].

Migration. *1940-50,* migration total from [84, no. 223 (Jan. 26, 1961), p. 4]. Age-sex distribution was assumed to be in same proportions as in [*ibid.*, no. 98 (Aug. 13, 1954), p. 18]. A small deduction was made to allow for mortality among migrants between year of immigration and end of decade, based on survival rates employed in [*ibid.*, no. 187 (Nov. 10, 1958), p. 13]. *1950-60,* total and age-sex distribution of migrants (excluding Alaska and Hawaii) surviving to end of decade from unpublished estimates made by Gladys Bowles, kindly provided by Bureau of Census. For all age groups, the decade total was distributed between quinquennia according to the proportions shown for total migration in [*ibid.*, no. 302 (Mar. 11, 1965), p. 8]. *1960-64,* migration total from *ibid.* Age-sex distribution was assumed same as that of migrants used in 1964 population projections [84, No. 286 (July 1964), p. 27].

Figure 5

1880-1940

Total households. *1880-1920,* [86, series A-255]; *1910-1940,* [*ibid.*, series A-242].

Nonfarm households. *1880-1920,* total households less farm households [85, p. 6]; *1910-1940,* [86, series A-243]. The break in 1910-20 reflects a shift from census levels to estimates consistent with those of the Current Population Survey.

With regard to components of change, although estimates are available for the total number of households at each census date, lack of detail by sex and age for several of the censuses prevented use for the period before 1940 of the partition technique described in the text for households post-1940 and for labor force. After some experimentation, the following procedure for approximating the components was adopted:

a. Contribution of aging and mortality. For each decade the absolute change due to aging and mortality in the population of both sexes aged 25 and over was weighted by .44, and in the population 20-24 by .10, the weights representing the approximate headship rates for these population groups. Summation of the results gave an estimate of the absolute change in households due

to this population component. This was then converted to a percentage change by dividing by the average of the initial and terminal number of households for the decade.

b. Contribution of net migration. Assumed the same as the contribution of net migration to the percentage change in population aged 25 and over.

c. Contribution of headship rate change. Assumed the same as the change in the age-standardized proportion of males aged 14 and over married. The absolute change over the decade was converted to a percentage basis by dividing by the average of the initial and terminal marriage proportions for the decade.

Estimates of the components of population change by sex and age needed for (a) and (b), prepared for the University of Pennsylvania Study of Population Redistribution and Economic Growth, were kindly provided by Dr. Hope T. Eldridge. The headship rate weights of .10 and .44 are approximately those for the age groups 20-24 and 25+ in the 1890 and 1930 censuses. The age-standardized percentage of males married was from [86, series A-216].

The rationale of the procedures is that the contribution to household growth of the two demographic components (a and b) tends to conform closely to their contribution to growth of the population of household forming age. For component (a) an allowance is needed for differences among age groups in household headship. With regard to the rate component, marriage and household headship among males tend to be closely associated. Hence the movement in the former provides an approximation to the latter.

An indication of the validity of the approximation may be obtained by comparing the sum of the components with the total for each decade 1870-1940. The results correspond fairly closely for each period, and, of particular importance for the present purpose, the general pattern of change over time is quite similar. Another test is provided by applying the approximation technique to 1940-50 and comparing the results with those of the more refined procedure:

Household Growth (per cent) Due to:

	All Sources	Sum of Components	Aging and Mortality	Net Migration	Headship Rate Change
Refined procedure (1940–50 data in Figure 5)	21.7	21.7	13.1	1.8	6.8
Approximation	—	23.1	14.4	0.7	8.0

Although there are small differences, the approximation procedure correctly shows that the source of the sharp rise in household growth from 1930-40 to 1940-50 is due to the movement in the rate component, and that the demographic components played a negligible part in this rise.

1940-64

The method followed for estimating components of change was the same as that for labor force, and is described in the text, footnote 18. Household data refer to the civilian noninstitutional population plus members of the armed forces living off post or with their families on military reservations; population data, to the total population, including armed forces overseas. Calculations were done separately for each sex-age class of the total population. Age classes were 14-24, 25-29, 30-34, ten year groups through 55-64, and 65+. Alaska and Hawaii are omitted in calculations referring to periods through 1960, but are included in those for 1960 on.

Households. *1940,* [81, p. 1-458]. *1950,* [83, no. 33 (Feb. 12, 1951), p. 15]. *1955,* [*ibid.*, no. 67 (May 2, 1956), p. 11]. *1960,* [*ibid.*, no. 106 (Jan. 9, 1961), p. 13]. An estimate excluding Alaska and Hawaii was obtained by deducting 1960 census data for these states. *1964,* [*ibid.*, no. 139 (June 11, 1965), pp. 17-18].

Population and migration. Same as for Figure 4, 1940-64.

Figure 6

1920-30 [80, p. 1-93]; *1940-50* [84, no. 98, p. 15]; *1955.* [*ibid.*, no. 265, p. 25]; *1960-85* [*ibid.*, no. 286, p. 42, series C].

Figure 7

1920-40, [80, p. 1-238]. The 1920 and 1930 values were inferred from those for the appropriate cohorts as reported in 1940, the first date at which data on educational attainment were collected. *1950* [*ibid.*, p. 1-236]; *1960-85,* [84, no. 305, pp. 7-10].

Figures 8 and 9

For periods through 1960-64, values are same as those in Figures 4 and 5, except that the "population growth" component is the contribution due to net migration plus that due to aging and mortality. For sources and methods see those for Figures 4 and 5.

The method by which the quinquennial projections from 1960-65 through 1975-80 were partitioned is that described in the text, footnote 18. Scope and classification detail were the same as those for the 1960-64 period described under the head of 1940-64 calculations of Figures 4 and 5. Sources of basic data were as follows.

Labor force. *1965-75 (based on 1962 population projection),* [75, p. 4]. *1970-80 (based on 1964 population projection),* [76, p. 130].

Households. *1965-80 (based on 1962 population projection),* [83, no. 123 (April 11, 1963)]. Unpublished distributions by age and sex were provided by the Bureau of the Census.

Population. *1965-75 (1962 population projection),* [75, p. 4]. *1980 (1962 population projection),* [84, no. 251 (July 6, 1962), p. 4, series II]. *1970-80 (1964 population projection),* [76, p. 130].

Migration. 1960-75 (1962 population projection), quinquennial projection from [84, no. 187 (Nov. 10, 1958), p. 13]. In accordance with assumption used in census projections, all migrants were assumed to survive to the end

of the quinquennium in which they immigrated. *1960-80 (1964 population projection),* annual projection in [84, no. 286 (July 1964), p. 27] was cumulated and aged to end of quinquennium, assuming no mortality among migrants.

REFERENCES

1. M. ABRAMOVITZ, *Evidences of Long Swings in Aggregate Construction Since the Civil War,* Nat. Bur. Econ. Research Occas. Paper 90, New York 1964.
2. ———, "The Nature and Significance of Kuznets Cycles," *Econ. Develop. and Cult. Change,* Apr. 1961, *9,* 225-48.
3. ———, "Historical and Comparative Rates of Production, Productivity and Prices," *Employment, Growth and Price Levels,* Hearings before the Joint Economic Committee, 86th Cong., 1st sess., Pt. 2, Washington 1959, pp. 411-66.
4. ——— and P. A. DAVID, "Preliminary Notes on the Postwar and Long-Term Growth Experience of the United States," Research Center in Economic Growth, Stanford University, Jan. 1964, mimeo.
5. I. ADELMAN, "Business Cycles—Endogenous or Stochastic?," *Econ. Jour.,* Dec. 1960, *70,* 783-96.
6. ———, "Long Cycles—Fact or Artifact?," *Am. Econ. Rev.,* June 1965, *55,* 444-63.
7. ———, "Long Swings—A Simulation Experiment" in F. Balderston and A. C. Hogatt, ed., *Proceedings of a Conference on Simulation,* Cincinnati 1964.
8. G. L. F. BECKFORD, "Secular Fluctuations in the Growth of Tropical Agricultural Trade," *Econ. Develop. and Cult. Change,* Oct. 1964, *13,* 80-94.
9. E. M. BERNSTEIN, "The Postwar Trend Cycle in the United States," *Quart. Rev.* (Model, Roland, and Co.), 1st Quart. 1963, pp. 1-10; partly reprinted in *Economic Growth: An American Problem,* Englewood Cliffs 1964, pp. 116-24.
10. R. C. BIRD, M. J. DESAI, J. J. ENZLER, and P. TAUBMAN, "Kuznets Cycles in Growth Rates: Their Meaning," *Internat. Econ. Rev.,* May 1965, *6,* 229-39.
11. W. G. BOWEN and R. A. BERRY, "Unemployment Conditions and Movements of the Money Wage Level," *Rev. Econ. Stat.,* May 1963, *45,* 163-72.
12. D. S. BRADY, *Age and the Income Distribution,* U.S. Dept. Health, Education, and Welfare, Soc. Sec. Admin. Div. Research and Stat., Res. Rept. No. 8, Washington 1965.
13. L. BRIDGE, "The Financing of Investment by New Firms," in Univ.-Nat. Bur. Comm. for Econ. Research, *Conference on Research in Business Finance,* New York 1952, pp. 65-75.
14. A. F. BURNS, *Production Trends in the United States since 1870,* Nat. Bur. Econ. Research, New York 1934.

15. B. O. Campbell, *The Housing Life Cycle and Long Swings in Residential Construction: A Statistical and Theoretical Analysis*. Doctoral dissertation, Stanford Univ., 1961.
16. ———, "Long Swings in Residential Construction: The Postwar Experience," *Am. Econ. Rev.*, May 1963, *53*, 508-18.
17. D. J. Daly, "Long Cycles and Recent Canadian Experience," in Royal Commission on Banking and Finance, *Appendix Volume*, Ottawa 1964, pp. 279-301.
18. ———, "The Scope for Monetary Policy—A Synthesis." Unpublished paper prepared for the Conference on Stabilization Policy, Univ. of Western Ontario, August 30 to September 1, 1965.
19. E. F. Denison, "Discussion," *Am. Econ. Rev.*, May 1963, *53*, 530-32.
20. R. A. Easterlin, *The American Baby Boom in Historical Perspective*, Nat. Bur. Econ. Research Occas. Paper 79, New York 1962.
21. ———, "Influences in European Overseas Emigration before World War I, *Econ. Develop. Cult. Change*, Apr. 1961, *9*, 331-51.
22. ———, "Long Swings in U.S. Demographic and Economic Growth: Some Findings on the Historical Pattern," *Demography*, 1965, *2*, 490-507.
23. ———, "On the Relation of Economic Factors to Recent and Projected Fertility Changes," *Demography*, 1966, *3*, 131-53.
24. H. T. Eldridge and D. S. Thomas, *Population Redistribution and Economic Growth, United States, 1870-1950*. Vol. III, *Demographic Analysis and Interrelations*. Philadelphia 1964.
25. G. H. Evans, Jr., *Business Incorporations in the United States, 1800-1943*, Nat. Bur. Econ. Research, New York 1948.
26. D. E. C. Eversley and V. Jackson, *Problems Encountered in Forecasting Housing Demand in an Area of High Economic Activity: Headship Rates in Relation to Age Structure, Fertility, Education, and Socio-economic Groups*. Unpublished paper presented at U.N. World Population Conference, Belgrade, September 1965.
27. R. Fels, "Discussion," *Am. Econ. Rev.*, Proceedings, May 1963, *53*, 533-34.
28. R. Ferber, "Research on Household Behavior," *Am. Econ. Rev.*, March 1962, *52*, 19-63.
29. E. Frickey, *Production in the United States, 1860-1914*. Cambridge 1947.
30. A. Gerschenkron, *Economic Backwardness in Historical Perspective*. Cambridge 1962.
31. R. A. Gordon, "Discussion," *Am. Econ. Rev.*, Proceedings, May 1963, *53*, 537-40.
32. ———, "What Kind of a Business Cycle in the Years Ahead?," *Commercial and Fin. Chronicle*, Sept. 5, 1963, Vol. 198, No. 6296, pp. 3 ff.
33. M. Gottlieb, "Fluctuations in Marriage and Migration Experience in Long Swings in Economic Growth," presented at U.N. World Population Conference, Belgrade, Sept. 1965.
34. A. Hansen, *Fiscal Policy and Business Cycles*. New York 1941.

35. B. G. HICKMAN, "The Long Cycle: Hidden Force in the Economy," *Dun's Rev.*, Dec. 1963, pp. 48 ff.
36. ———, "The Postwar Retardation: Another Long Swing in the Rate of Growth?," *Am. Econ. Rev.*, Proceedings, May 1963, *53*, 490-507.
37. W. ISARD, "A Neglected Cycle: The Transport-Building Cycle," *Rev. Econ. Stat.*, Nov. 1942, *24*, 149-58.
38. ———, "Transport Development and Building Cycles," *Quart. Jour. Econ.*, Nov. 1942, *57*, 90-110.
39. E. B. JONES, "New Estimates of Hours of Work per Week and Hourly Earnings," *Rev. Econ. Stat.*, Nov. 1963, *45*, 374-85.
40. J. W. KENDRICK, *Productivity Trends in the United States*, Nat. Bur. Econ. Research, New York 1961.
41. S. KUZNETS, *Capital in the American Economy: Its Formation and Financing*, Nat. Bur. Econ. Research, Princeton 1961.
42. ———, "Long Swings in the Growth of Population and in Related Economic Variables," *Proc. Am. Philos. Soc.*, Feb. 1958, *102*, 25-52.
43. ———, "Quantitative Aspects of Economic Growth of Nations. I. Levels and Variability of Rates of Growth," *Econ. Develop. and Cult. Change*, Oct. 1956, *5*, 1-94.
44. ———, *Secular Movements in Production and Prices*. New York 1930.
45. ——— and E. RUBIN, *Immigration and the Foreign Born*, Nat. Bur. Econ. Research Occas. Paper 46, New York 1954.
46. J. B. LANSING and J. N. MORGAN, "Consumer Finances over the Life Cycle," in L. H. Clark, ed., *Consumer Behavior*, Vol. II, *The Life Cycle and Consumer Behavior*, New York 1955, pp. 36-51.
47. S. LEBERGOTT, *Manpower in Economic Growth: The American Record since 1880*. New York 1964.
48. E. S. LEE, *et al.*, *Population Redistribution and Economic Growth, United States, 1870-1950*. Vol. I, *Methodological Considerations and Reference Tables*. Philadelphia 1957.
49. J. P. LEWIS, *Building Cycles and Britain's Growth*. New York 1965.
50. ———, "Growth and Inverse Cycles: A Two-Country Model," *Econ. Jour.*, March 1964, *74*, 109-18.
51. C. D. LONG, "The Illusion of Wage Rigidity: Long and Short Cycles in Wages and Labor Costs," *Rev. Econ. Stat.*, May 1960, *42*, 140-51.
52. ———, *The Labor Force under Changing Income and Employment*, Nat. Bur. Econ. Research, Princeton 1958.
53. A. LOSCH, "Population Cycles as a Cause of Business Cycles," *Quart. Jour. Econ.*, 1937, *51*, 649-62.
54. I. MINTZ, "Discussion," *Am. Econ. Rev.*, Proceedings, May 1963, *53*, 534-37.
55. D. C. NORTH, *The Economic Growth of the United States, 1790-1860*. Englewood Cliffs 1961.
56. W. W. ROSTOW, *The Stages of Economic Growth*. Cambridge 1961.
57. J. A. SCHUMPETER, *Business Cycles*. New York 1939.

58. N. J. SILBERLING, *The Dynamics of Business.* New York 1943.
59. W. B. SMITH, "Wages Rates on the Erie Canal, 1828-1881," *Jour. Econ. Hist.*, Sept. 1963, *23*, 298-311.
60. A. T. SOMERS, "The Economic Environment of the Middle Sixties," *Conference Board Record,* Sept. 1964, *1* (9).
61. B. THOMAS, *Migration and Economic Growth.* Cambridge, 1954.
62. ————, "Wales and the Atlantic Economy," *Scottish Jour. Pol. Econ.*, Nov. 1959, *6*, 169-92.
63. D. S. THOMAS, "Age and Economic Differentials in Interstate Migration," *Population Index,* Oct. 1958, *24*, 313-24.
64. ————, "International Migration," in P. M. Hauser, ed., *Population and World Pol.*, Glencoe 1958, pp. 137-61, 274-75.
65. ————, "Some Aspects of a Study of Population Redistribution in the United States, 1870-1950." United Nations, *Proceedings of the World Population Conference, 1954 Rome, 31 Aug.-10 Sept., 1954 Papers,* Vol. 2, New York 1956-1957, pp. 667-713.
66. ——— and K. C. ZACHARIAH, "Some Temporal Variations in Internal Migration and Economic Activity, United States, 1880-1950," International Union for the Scientific Study of Population, *International Population Conference, New York 1961,* Vol. 1, London 1963, pp. 525-32.
67. C. M. TIEBOUT, *The Community Economic Base Study,* Suppl. Paper No. 16. New York 1962.
68. J. G. WILLIAMSON, *American Growth and the Balance of Payments, 1820-1913.* Chapel Hill 1964.
69. ———, "Dollar Scarcity and Surplus in Historical Perspective," *Am. Econ. Rev., Proceedings,* May 1963, *53*, 519-29.
70. V. ZARNOWITZ, "Cyclical Aspects of Incorporations and the Formation of New Business Enterprises," in G. H. Moore, ed., *Business Cycle Indicators,* Vol. I, Princeton 1961, pp. 386-419.
71. CONFERENCE ON RESEARCH IN INCOME AND WEALTH, *Output, Employment, and Productivity in the United States after 1800,* Stud. in Income and Wealth, Vol. 30, New York 1966.
72. ECONOMIC COUNCIL OF CANADA, *First Annual Review: Economic Goals for Canada to 1970.* Ottawa 1964.
73. *Economic Report of the President,* Washington 1965, 1966.
74. U.S. BUREAU OF LABOR STATISTICS, *Employment and Earnings.* Washington 1959, 1961, 1965, 1966.
75. ———, *Interim Revised Projections of United States Labor Force, 1965-75,* Special Labor Force Report 24, Washington 1962.
76. ———, *Labor Force Projections for 1970-80,* Special Labor Force Report 49. Washington 1965.
77. ———, *Population and Labor Force Projections for the United States, 1960 to 1975,* Bull. 1242. Washington 1959.
78. U.S. BUREAU OF THE CENSUS, *Census of Population. 1900 Special Report, Occupations. Washington* 1904.
79. ————, *Comparative Occupation Statistics for the United States, 1870*

to 1940, by Alba M. Edwards, *Census of Population: 1940*. Washington 1943.

80. ———, *Census of Population 1950. United States Summary*. Washington 1953.
81. ———, *Census of Population 1960. United States Summary*. Washington 1963.
82. ———, *Current Population Reports, Labor Force Bulletin*, Ser. P-50.
83. ———, *Current Population Reports, Population Characteristics*, Ser. P-20.
84. ———, *Current Population Reports, Population Estimates*, Ser. P-25.
85. ———, *Farm Population: 1880 to 1950*, by Leon E. Truesdell. Tech. Paper 3. Washington 1960.
86. ———, *Historical Statistics of the United States, Colonial Times to 1957*. Washington 1960.
87. ———, *Historical Statistics of the United States, Colonial Times to 1957; Continuation to 1962 and Revisions*. Washington 1965.
88. ———, *Statistical Abstract of the United States*, Washington 1963.
89. U.S. Office of Business Economics, *U.S. Income and Output*. Washington 1958.
90. Universities-National Bureau Committee for Economic Research, *The Measurement and Behavior of Unemployment*. Princeton 1957.

THE POSSIBILITY OF A SOCIAL WELFARE FUNCTION

By James S. Coleman*

Ever since 1951, when Kenneth Arrow published his monograph showing the paradox that social welfare functions generate, there has been interest in overcoming or bypassing this paradox.[1] The paradox is that, given certain simple and very persuasive conditions for collective rationality, there can be no decision rule which meets these conditions. The conditions, simply stated, are these:

1. Between three alternatives, the social welfare function must give rise to a unique social ordering, no matter how individual members of the group choose to order the three alternatives.
2. The social ordering must correspond positively, or at least not negatively, to changes in the ordering of any one individual.
3. The elimination of any one alternative shall not affect the ranking of the other alternatives in the social welfare function.
4. Voters can choose freely among all alternatives.
5. No one individual's choice can dictate the social ordering independently of the choices of others.

It is easy to show that even with three voters and three alternatives, a situation can exist in which these conditions are not met by a simple majority vote. For consider three voters, X, Y, and Z, and three alternatives, A, B, and C. The rankings given by X, Y, and Z are:

Rank	X	Y	Z
1	A	B	C
2	B	C	A
3	C	A	B

Now if we eliminate alternative C, A is preferred by X and Z, and

* The author is professor of social relations at The Johns Hopkins University. He is grateful to a number of persons, especially James Buchanan, Carl Christ, Arthur Stinchcombe, and Gordon Tullock, for comments on an earlier draft.

[1] See Kenneth Arrow [1]. Jerome Rothenberg [9] discusses in detail the main lines of the work subsequent to Arrow in his review and synthesis of the literature on social welfare. Some of these approaches are in the same spirit as the present paper, though none, so far as I know, take the position taken in this paper, or arrive at similar conclusions. Hildreth [5] says, for example "Suppose that in Case I (individual) i barely prefers (alternative) X to Y, and j desperately prefers Y to X. In Case II let i desperately prefer X to Y and j barely prefer Y to X. By Arrow's Condition 3, the social ordering between X and Y must be the same in Case I as in Case II. Admittedly our facilities for distinguishing bare preferences from desperate preferences may often be questionable, but we have to decide whether or not this justifies us in excluding all variations in degrees of preference from consideration."

therefore wins. Thus we say that the social welfare function generates a social order AwB (where "w" means A wins over B). When we eliminate alternative A, B is preferred by X and Y, and therefore wins. Thus BwC. Finally, if B is eliminated, C is preferred to A by Y and Z, and therefore wins. Thus CwA. However, this produces an inconsistent social ordering, since AwB, BwC, but CwA.

I want to suggest in this paper that this approach to a social welfare function leaves out of consideration precisely those elements which are most crucial in empirical cases of social choice, and that it is this omission which creates the apparent paradox. This omission allows the problem to be conceptualized in such a way that the third condition above appears to be very "reasonable," while from the point of view of individual rationality it is not.

Quite simply, I suggest that this approach, by looking at only the surface characteristics of social decision rules, neglects the most important element that nearly all such rules contain: a means for allowing the expression of relative intensities of preference, or of differences between utilities associated with alternatives. I suggest, in other words, that Arrow's impossibility theorem is relevant only to those social choice mechanisms in which it is not possible to express relative intensities, and that when it is possible to express such intensities, condition 3 is inconsistent not only with "collective rationality" but with individual rationality. That is, the elimination of an alternative, though it be very low in his ordering, may change the rational individual's behavior.[2] I will also suggest some of the most important mechanisms, in actual societies, through which individuals may express intensities.

I suggest that the appropriate way to examine such problems of individual choice and social welfare is akin to the method of studying utility under risk. That is, when the outcome of a decision is uncertain, then each individual attaches to each possible outcome a subjective probability, and thus to the decision an expected utility. If he has various kinds of behavior that he can carry out, then he will act in such a way as to maximize the expected utility.

In any collective decision or "social choice," the outcome is not determined by the individual's action alone. Thus in order to behave rationally, he must estimate the expected utilities of each action open to him. These expected utilities are contingent not only on the utility of each outcome to him, but on the probability he estimates it to have under each action open to him. Consequently, the behavior of indi-

[2] I should note here that the approach of devising a social welfare function which should meet criteria of "collective rationality" is not one which is in general consistent with positive economic theory. It is much more appropriate to look for decision functions which are defined in terms of the rational behavior of individuals, such as the principle of Pareto optimality.

viduals in collective decisions must be governed by rationality under uncertainty or risk, not by rationality under certainty. It follows from this that the rational individual must express not only the ordering of his utilities for the possible outcomes, as he does in rationality under certainty, but also the relative sizes of utility differences between various outcomes, as he must do in rational behavior under risk.

It could be said that this approach violates the spirit of Arrow's approach, because Arrow was concerned merely with the ordering of tastes in the community, and showed that no aggregation of these ordered tastes could be consistent, in the sense of meeting his conditions for reasonableness or social rationality. The flaw in this argument is that citizen sovereignty, which is one of Arrow's conditions, implies the *expression* of these tastes or preferences, the expression that is normally embodied in voting, with the voting rule serving as the aggregation device for the society. Once this is allowed, then as indicated above, it becomes irrational for an individual to express only his preferences, and not something more which would maximize his expected utility. This "something more" can be variously characterized as his intensity of preference or the relative sizes of utility differences between outcomes.

Thus it is not correct to establish a relation between individual choice and social choice which uses as a model individual choice under certainty. As a subsequent example will indicate, only under a very special condition does the rational individual's behavior become like that of the rational actor under certainty.

It should be pointed out that the use of intensities of preference in relation to social choice, as derived from conceptions of utility under risk, is nothing new. Rothenberg, in his review of work subsequent to Arrow's, discusses in detail the use in social welfare functions of expected utility deriving from von Neumann-Morgenstern's definitions of utility under risk [9]. But in all these approaches, the expected utility has been imported, so to speak, from some supposed external measurement, and does not derive from the very behavior itself in the collective decision.[3] Such an approach leads these authors to question whether it is "legitimate" to introduce intensities through use of cardinal utilities. (For example, Rothenberg says, "Assuming that we can 'measure' preference intensities with a tolerable degree of precision, is this the kind of factor which we *ought* to treat on a par with preference ordering in the context of social choice?" [9, p. 139]. This approach is a carry-over from an earlier and more naive view of social welfare, in which it was assumed that the economist, as advisor to the policy-maker, could assess the

[3] Peter Newman [7] has, however, explicitly recognized that individual behavior in collective choice must be governed by rationality under uncertainty, rather than certainty.

tastes of individuals through some sort of measurement, and then tell the policy-maker what was best for the people—always in terms of their own tastes. In fact, however, it is the political process itself which, together with the existing structure of collective decision-making, expresses these tastes. Only more recently in work like that of Duncan Black [2], Downs [4], Buchanan and Tullock [3], and Olson [8], and earlier in the individualist school of public finance stemming from Wicksell, have economists given the individual the same autonomy to express his interests in collective decisions that they have long allowed him in market behavior.[4]

I. *The Rational Individual in Collective Decisions*

The simple example of three individuals and three alternatives given earlier can be used to illustrate how rational individuals may be expected to behave in collective decisions, and some of the implications for the system. We will consider the individuals in the following circumstances:

a. They can only vote, and have no information about others' votes.

b. They can vote, but they also have some information that allows them to estimate probabilities of others' votes.

c. They can not only vote on the social decision, but also have another decision on which to make a choice. They can make any agreements they desire in the negotiations preceding the vote.

A. *Only Voting on a Single Issue with No Information*

Let us think of a quantity U_{xa}, which is the utility of outcome A to individual X. The properties that this quantity will obey depend on the kind of behavior allowed to X. That is, although we may consider that in reality X does have some "amount of utility" which he associates with A, we will treat this "amount" as if it had only limited properties, depending on the behavior which we allow the individual. The wider the range of behavior allowed, the nearer this "amount" will come to have the properties of a real number.

If individual X does not know the preferences of Y and Z, then we can calculate the expected utility of this decision for him as follows, under a majority rule.[5] Under this complete absence of knowledge about Y's and Z's preferences, all possible combinations of Y's and Z's vote have equal subjective probability for him. Thus a table can be set up as indicated below:

[4] Wicksell's and Lindahl's approaches to the theory of public finance are probably as closely allied in spirit to the present paper as any of the subsequent approaches.

[5] Such a decision rule as a majority rule would need an additional rule in case no alternative attained a majority. Such a rule we posit to be a random mechanism with probabilities for each alternative proportional to the number of votes it received. In this case, $p_a = p_b = p_c = \frac{1}{3}$.

Votes of		Outcome if X	
Y	Z	Votes for A	Probability
A	A	A	1/9
A	B	A	1/9
A	C	A	1/9
B	A	A	1/9
B	B	B	1/9
B	C	1/3 A, 1/3 B, 1/3 C	1/9
C	A	A	1/9
C	B	1/3 A, 1/3 B, 1/3 C	1/9
C	C	C	1/9

The outcomes are similar, but in favor of B and C, if he were to vote for B or C respectively. If we label X's vote for A, B, or C, X_a, X_b, and X_c, respectively, the expected utility of this decision for him, if he knows nothing about Y's and Z's preferences, is:

$$E(U_x \mid X_a) = 17/27\ U_{xa} + 5/27\ U_{xb} + 5/27\ U_{xc} \tag{1}$$

$$E(U_x \mid X_b) = 5/27\ U_{xa} + 17/27\ U_{xb} + 5/27\ U_{xc} \tag{2}$$

$$E(U_x \mid X_c) = 5/27\ U_{xa} + 5/27\ U_{xb} + 17/27\ U_{xc} \tag{3}$$

Clearly, if $U_{xa} > U_{xb} > U_{xc}$, then subtraction will show that $E(U_x|X_a) > E(U_x|X_b) > E(U_x|X_c)$, and he will carry out action X_a if A is an available alternative and X_b if only B and C are available. Since this is the only action available to him, the only information we have concerning the utilities as a result of his behavior is an ordering. (Alternatively, the only property we need to attribute to the utilities to predict such behavior is their order.)

B. *Vote on a Single Issue, with Information about Others' Preferences*

If X has the same behavior alternatives available to him, but has in addition information about the ordering that Y and Z give to A, B, and C, and assumes they will vote their preference, his assessment of the situation is this:

Votes of		Outcome if He Votes for:			
Y	Z	A	B	C	Probability
B	C	1/3 A, 1/3 B, 1/3 C	B	C	1

Under the belief that Y and Z will vote for B and C respectively, his expected utilities are:

$$E(U_x \mid X_a) = 1/3\ U_{xa} + 1/3\ U_{xb} + 1/3\ U_{xc} \tag{4}$$

$$E(U_x \mid X_b) = U_{xb} \tag{5}$$

$$E(U_x \mid X_c) = U_{xc} \tag{6}$$

Now his behavior is not fully determined by the order among U_{xa}, U_{xb}, and U_{xc}. For if U_{xc} is small, relative to U_{xb} and U_{xa}, so that

$$(7) \qquad 1/3\, U_{xa} + 1/3\, U_{xb} + 1/3\, U_{xc} < U_{xb}$$

then he will choose to take action X_b rather than X_a. This situation is exactly that of the hypothesis used in measurement of utility under risk, where his behavior in a choice between risky alternatives is used to provide information about the size of utility differences between different alternatives. Rearranging the inequality above gives the following relation, if he chooses alternative B:

$$(8) \qquad U_{xa} - U_{xb} < U_{xb} - U_{xc}$$

That is, if he chooses B although he prefers A, the utility difference between alternatives A and B is less than that between B and C.

Of course, the same analysis may be applied to the other individuals Y and Z, with the result that each person might vote for his second choice because he assumes that the others will vote their first choices. However, such a peculiar result does not make the behavior less rational for him. It means merely that he incorrectly assessed the probabilities of the others' votes.[6] If he had knowledge, or some degree of belief, that those votes would be cast for other than alternatives B and C respectively, then the appropriate subjective probabilities would modify the coefficients in equations (4), (5), and (6). The important point is that when he no longer estimates the probabilities of others' votes to be equal for each alternative, the degenerate case of equations (1)–(3) no longer holds, and his behavior will depend on the size of the utility differences between pairs of outcomes, as in (8).

Thus, even in this case of actions limited to a single vote, a comparison of utilities beyond simple ordering can come about. If he takes action X_b (a vote for B), this implies equation (7), or rearranged,

$$U_{xb} < 1/2\, U_{xa} + 1/2\, U_{xc},$$

under the expected utilities indicated in equations (4), (5), and (6). That is, if he prefers A to B to C, but votes for B, knowing that Y will vote for B and Z for C, then this means that B is "closer to" A in utility

[6] A game-theoretic definition of rationality would include an estimate, in the absence of other information, of the probabilities associated with others' actions. In game theory, the estimate is that the other persons will use the same rules one does oneself. This would require an assessment of double contingencies by each individual, and require the addition of some arbitrary rule, such as von Neumann's minimization of maximum loss, in order to make the behavior determinate. Because any such rule is arbitrary, it is outside the scope of what one wants within the definition of rationality. And because the assumed situation of complete absence of information about the others' actions, on which this definition of rationality would depend, is only one point in the space of all possible information states, there is still less reason to accept the von Neumann definition.

for him than C is to B. Note that because the individual must calculate expected utilities of the outcome, no outcome is "irrelevant," so long as there is some subjective probability of its occurrence. In fact, in this case, the utility of C for X plays a crucial role in determining whether he will vote for A or B. Thus the elimination of C might have—and legitimately so from the point of view of individual rationality as well as social welfare—an effect upon the outcome.[7]

C. *Another Collective Decision, with Vote Exchanges*

The behavior available to the individual in the above cases is not enough to express much about the utilities that various alternatives have for him. Neither does it correspond to the behavior available to members of decision-making bodies, such as city councils, legislatures, town meetings, or social groups. In any such body, the individuals, in attempting to maximize their expected utilities, will use whatever power they possess. The principal kind of behavior that is available to them in nearly all cases is a sequence of decisions. The essential properties that such a sequence introduces are shown by introducing a single new decision, with alternatives labelled similarly to the first. The orders by X, Y and Z are:

Rank	X	Y	Z
1	A'	B'	C'
2	B'	C'	A'
3	C'	A'	B'

Now considering again individual X in the situation where he knows the various orders of the others (that is, there is full communication, obtained through discussion), he has expected utilities under a wide range of possible actions. For now he has the power to induce an action on the part of another person in response to an action on his part. For example, suppose for him the utility differences between A', B', and C' are very small, while the utility differences between A, B, and C are very large. Then he might consider an exchange of votes with Z such that A and C' would win.[8] His utility under this possible action could

[7] An incidental point may be made here concerning the apparent reasonableness of the condition of independence of the social ordering of alternatives from the elimination of other alternatives. This appears reasonable, I suggest, because of a too direct analogy between individual rationality and collective rationality. Rather than the concept of collective rationality as a parallel to individual rationality, it is appropriate to look at collective decisions as an outcome of the operation of individual rationality. We can then ask the question: in a given situation and with a given decision rule (that is, including the whole set of actions we are allowing individuals), how closely does the outcome come to maximization of social welfare? This is obviously a more difficult question in collective actions than in individual ones. It will be discussed explicitly later in the paper.

[8] The one explicit recognition of such vote-exchange behavior by economists is in Buchanan

then be compared with his expected utility under the conditions of no exchange (assuming his subjective probabilities are 1 that Y and Z will vote for B and C respectively):

$$(9)\quad U_{xa} + U_{xc'} \overset{?}{>} 1/3\, U_{xa} + 1/3\, U_{xb} + 1/3\, U_{xc} + 1/3\, U_{xa'} + 1/3\, U_{xb'} + 1/3\, U_{xc'}$$

Rearranging,

$$(10)\quad U_{xa} - U_{xb} + U_{xa} - U_{xc} \overset{?}{>} U_{xa'} - U_{xc'} + U_{xb'} - U_{xc'}$$

If the indicated utility differences for A, B, and C were greater than the indicated ones for A', B', and C', then he would indeed prefer to make the exchange, rather than not. Similarly, he could consider an exchange with Y, to give outcomes A and B'. If inequality (10) holds, then he will also want to compare the two possible exchanges:

$$U_{xa} + U_{xc'} > U_{xa} + U_{xb'}$$

This inequality does not hold if $U_{xb'} - U_{xc'} > 0$, as the initial order implies. Thus for him the bargain with Y has highest utility. But it may be a bargain which he cannot make, since Y would have to accept the utility of his least preferred alternative, A. That is, $U_{ya} + U_{yb'}$ may not be large enough, compared to the alternative actions, for Y to make this exchange. Considering the possible alternatives for Y as exchanges with X or Z (for in the general case of unequal utility differences between alternatives, some action which involves exchange will always have a higher utility for an individual than an equal chance of all alternatives), there are two cases: B and C', or B' and C. Thus if either

$$(11)\quad U_{yb} + U_{yc'} > U_{ya} + U_{yb'}$$

or

$$(12)\quad U_{yb'} + U_{yc} > U_{ya} + Y_{yb'}$$

then Y will prefer to make an exchange with Z. But Y's initial preference ordering gives $U_{yc} > U_{ya}$, so that at least (12) will hold. This will not insure, of course, that Z will be satisfied with such a trade.

This kind of behavior need not lead, of course, to a stable solution which, in a game-theoretic sense, dominates all others. The outcome may depend on many chance factors, such as who talks first to whom. However, the basic point is that each person will attempt to maximize

---

and Tullock [3, Ch. 10]. However, political scientists are so fully aware of this behavior that they have common names for it: "horse-trading," "log-rolling," "making a deal," and other terms that would be appropriate in economic markets.

his expected utility. He will do so by taking that action for which the increment in expected utility—the analog to marginal utility for a situation of this type—is greatest. Ordinarily this action will be one of exchange: increasing his control over a decision for which control makes a great deal of difference in his expected utility, and in return giving up to another person control over a decision for which control makes little difference in his expected utility. If we consider, in the first analysis, his actions to be limited to pairwise exchanges of control, then he will order decisions i in terms of $\Delta U_{xi} = E(U_{xi} | v_{ix}) - E(U_{xi})$, where v_{ix} is an additional vote gained by X on decision i by this action. The relative sizes of ΔU_{xi} for all issues i express his *interest* in gaining a vote on each issue. He will also order possible actions in terms of the decrement in expected utility that the loss of his vote to a given individual k on a given decision j would mean: $\Delta U_{xkj} = E(U_{xj} | v_{jk}) - E(U_{xj})$, where v_{jk} is the transfer of control over his vote on decision j to individual k. Thus, with such a situation and limited to pairwise exchanges, each rational individual will try to carry out an exchange increasing control over decision i in return for losing control over decision j such that ΔU_x is maximized:

$$\overset{*}{\Delta U_x} = \max_{\substack{i,j,k \\ i \neq j}} (\Delta U_{xi} - \Delta U_{xkj})$$

It is useful to consider a particular example of utilities associated with different alternatives which would generate exchange attempts in the case of the above 2-decision system. Assume the following utilities associated with each alternative by each person. (These numbers are to be compared only for a given individual; the scaling for different individuals is arbitrary, since no interpersonal comparison of utility is implied. Also, the full properties of an interval scale are not assumed, but only the "distances" between utilities associated with different alternatives in the same decision. It is more convenient, however, to use specific numbers, even though all the properties of these numbers are not required for the behavior under discussion.)

	X	Y	Z
U_a	10	8	9
U_b	5	10	8
U_c	1	9	10
$U_{a'}$	10	8	9
$U_{b'}$	9	10	6
$U_{c'}$	8	9	10

In this situation gaining control of the first decision clearly has higher expected utility increment for X than does gaining control over the second decision. For if his assessment of the probabilities of A, B, and C is $\frac{1}{3}$ each without control, then if we call ΔU_{x1} his utility increment in decision 1 through gaining control over it,

$$\Delta U_{x1} = U_{xa} - (1/3\ U_{xa} + 1/3\ U_{xb} + 1/3\ U_{xc}),$$
$$= 10 - 5.33 = 4.67$$

and

$$\Delta U_{x2} = U_{xa'} - (1/3\ U_{xa'} + 1/3\ U_{xb'} + 1/3\ U_{xc'}),$$
$$= 10 - 9 = 1.0$$

The loss of utility through giving up his vote to Y or Z can be evaluated as follows, where ΔU_{xy1} is his increment (or decrement) in utility from decision 1 through giving up his vote to Y:

$$\Delta U_{xy1} = 5.0 - 5.33 = -0.33$$
$$\Delta U_{xz1} = 1.0 - 5.33 = -4.33$$
$$\Delta U_{xy2} = 9 - 9 = 0$$
$$\Delta U_{xz2} = 8 - 9 = -1$$

Thus in evaluating the interest of X in particular exchanges (the expected utility of the exchanges) by $\Delta U_{xi} + \Delta U_{xkj}$, two exchanges have sharply higher utility than any others:

$$\Delta U_{x1} + \Delta U_{xy2} = 4.67 - 0 = 4.67$$
$$\Delta U_{x1} + \Delta U_{xz2} = 4.67 - 1 = 3.67$$

A similar evaluation would show that for Y both decisions 1 and 2 are of equal interest, and a trade with Z on either will maximize Y's expected increment (at 1.0). For Z, control over decision 2 is of most interest, and an exchange with X gives highest expected increment in utility (at 1.67). Thus in this situation, X and Z would make an exchange, giving X control over decision 1 and Z control over decision 2. The outcomes would be A for decision 1, and C' for decision 2.[9]

[9] If Y was very perceptive, then he would make an offer to Z which would be of higher expected utility to Z (assuming that Z could be sure of Y's honoring the offer). Y could offer to vote for both C and C', receiving nothing in return. This would improve his situation over the X–Z coalition, and improve Z's as well. This in turn would lead to an offer by X to vote for both B and B', and then to an offer by Z to X to return the original agreement or to vote for A and A'. This possibility of instability, and the various types of possibilities that could occur for other configuration of utilities, need not concern us here—for we are not concerned with the outcome, but with the question of what is rational behavior for the participants. Such behavior depends only on subjective probabilities of others' behavior, together with utilities of various outcomes. In any case, these indeterminacies seem to arise in the few-person case, and our interest is principally in larger systems.

The above discussion should be sufficient to show that when individuals are released from restrictive conditions upon behavior and information (i.e., voting on only one issue and complete absence of information about others' behavior), then rational behavior demands that every alternative which has some subjective probability of occurrence can affect their behavior, though it may be least preferred. As a consequence, when such behavior is allowed, the condition which Arrow states, independence of outcome from "irrelevant" alternatives, can hardly be imposed. For since no alternatives are irrelevant in the expected utility calculations which govern behavior, it is hardly reasonable to expect that any will be irrelevant to the outcome.

Given, then, the freedom from Arrow's impossibility theorem, we can proceed to investigate the structural conditions that will allow expression of more than the simple preferences that voting on a single issue implies.

The principal theoretical approach in the literature makes use of successive adjustments on a single proposal. Knut Wicksell introduced this conception into public finance by asking how taxes could be assessed in such a way that the optimum amount of public services would be provided, and the allocation would be such that marginal benefits equalled marginal costs for each.[10] Wicksell's solution was theoretically most elegant, though it had flaws which made it impossible to carry out in practice. His thesis was: (a) always to vote on a combined proposal which included not only the amount and mode of public expenditure, but also the distribution of taxes or other means of paying for the expenditure; (b) then to require the measure to pass not by majority but by unanimous vote. In this way, the amount of expenditure and distribution of burden would be so adjusted that those who value the activity least would pay least (otherwise they could not be induced to vote for it), and more generally, the amount and distribution would be continually adjusted until each voter's marginal costs equalled his marginal benefits. Such a measure would be approved unanimously, and only such approval, Wicksell held, would assure that benefits were greater than costs for all. Such a principle assumes, of course, that all persons have perfect calculating ability, that there is some means of efficiently carrying out the multiple adjustments in a proposal necessary to reach this state of perfect adjustment, and perhaps most important of all, that people will reveal true preferences. That is, it assumes that all persons are constrained, by fear of retaliation in kind, from understating their interest in the public activity.

However, Wicksell, and all those who have followed in this direction

[10] For Wicksell's contribution see Wicksell [10], and for a discussion of the general approach (including Lindahl's extension of it) see Richard Musgrave [6, pp. 71–80].

(with the exception of Buchanan and Tullock, in *Calculus of Consent*, Chapter 10), limit their attention to the single issue, and limit the resources available to the individual to his vote on this issue. Yet an economic man will be willing to spend various other resources, such as votes on other issues, etc., in proportion to his expected utility gain from their expenditure. Thus if those who are not interested in the decision can extract from the former such resources, down to the last bit that the former are willing to give, the latter will be recompensed, through a much simpler and more efficient mechanism than an attempt to adjust the tax or the expenditures so that it fits perfectly the utility structure of the population. This implies, at the extreme, that it is not important if there are tax loopholes, if these loopholes are made evident to all at time of passage of the bill, and those who will gain from them are made to pay, in terms of other legislation, the full amount that they are willing to pay in order to gain the loopholes.

Thus it becomes fruitful to develop further the possibilities which arise as a consequence of an expanded range of behavior among persons involved in collective decisions—a range of behavior which includes the employment of various resources, especially control over other issues, in the form of votes on other decisions which involve this collectivity. To be sure, such an approach would be academic if the proposed behavior was uncommon or impossible in decision-making bodies. But exactly the contrary is true. Numerous observors of legislative bodies have suggested that the very essence of legislative behavior is exchanges which bring about support on different issues; and there is evidence to suggest that when a single decision dominates a political or social system, so that its importance makes such exchanges impossible, the decision-making process breaks down; and not only is there no "social welfare function," there is overt conflict. Often, in actual collectivities, such as legislatures, there are a number of other resources that various legislators have, and can use in exchanges. One of these is partial control of positions within the legislature, such as committee membership. All these resources are used in the same way that goods are used in a barter market: in exchange for control over those events whose outcome one is most interested in. Many of these resources act to make unequal the power of different legislators, and thus give unequal degrees of power to their constituents. It is useful to conceive, however, of an ideal simple collectivity in which each actor's only resources are his votes on other collective decisions. With such a conception, one can raise the question of what conditions would allow the system of exchange to approach more nearly the free markets which in pure economic competition allow individuals to maximize their utility, relative to their initial resources.

II. *Extensions to Many Decisions and Many Individuals*

From the three-person, two-decision example above, two kinds of extensions are possible, affecting the result in different ways. One is an extension to a greater number of decisions, and the other is an extension to a greater number of individuals. I shall consider first the former.

In this example, it was worth a great deal, in X's private utility calculations, to gain alternative A rather than B; and worth still more to gain A rather than C. In contrast, it was worth little to Y, relative to his other utility comparisons, to gain B rather than C, or C rather than A. Yet for the very small gain of C in place of A, Y could make an offer to Z which might overcome X's potential agreement with Z. For a gain to Y that was a smaller gain than several others for him, he could counter an agreement of X's which would make most difference to X of all possible agreements. Thus even though this agreement was of utmost importance to X, he had no way of implementing his feelings, though it would obviously be worth more to him relative to his other interests to gain control of this decision than to either Y or Z (again, relative to Y's and Z's other interests).[11]

An extension in number of decisions destroys this inequity, by giving each of the persons a large amount of resources which he can employ or fail to employ on a decision, depending on its importance to him. For example, assume that there were a hundred other issues, each with three alternatives, A_i, B_i, C_i, and each ranked as in the first decision by X, Y, and Z. Suppose also that the relative utilities were in each case except the first, 10, 9, and 8, for each of the three members. Then individual X, to whom the first decision is of such importance, could offer to Z enough to express this importance. He could, for example, offer Z his vote on decisions 2 and 3, an offer which would be impossible for Y to counter and still make an expected profit, but one which would still leave X with an expected gain on these decisions considered all together.

More generally, the extension from two decisions to a great many allows even more flexibility of adjustments in exchange than this example indicates. In the extreme case, it approaches in one sense a market situation, in which persons have various resources, divisible into small units, and also many different wants. Each person's wants, of course, are scaled by the totality of his resources; but he can pay out various quantities of resources in order to get those things he wants

[11] Note that this is still a step away from interpersonal comparison of utility, since each's interests in a particular action are assessed only relative to his other interests. Although this paper will not examine the question in detail, the logic contained here implies that the only basis for an interpersonal comparison is a scaling of his interests by the resources he has in the system—that is, by the degree of control over various events given to him by the constitution of the collectivity.

most. In terms of political preferences, this means that those decisions for which the alternatives differ most for him in utility are the ones over which he can gain control through giving up his partial control over those of less importance to him. It allows, in short, an expression of "intensity" such that the outcome of the decision will tend to be the one which maximizes aggregate utility.[12]

This extension from a few decisions to a great many leads toward a market in one way, but not in another. With a very small number of persons, there can still be a large difference between the "price" paid for a vote (in terms of other votes) and its value to the purchaser. If the decisions that are of great interest to him are of little to the other persons, he can gain control over them with little sacrifice on his part, and then tend to gain more in utility from the set of decisions than his power would warrant. He, in effect, would be in the role of monopsonist, who can drive the price down to what he is willing to pay.

An extension in the direction of a great number of other individuals engaged in the set of decisions moves this in the direction of a free market, in which prices are established by competition among both buyer and seller.

A large and complex society, where there is a complex web of interdependence among various activities, tends to generate both the extensions discussed here. The extension of decisions occurs simply through the extension in time of the society; the extension in number and utility-variations of individuals occurs through the complexity of the economic and social structure. It is true, of course, that the particular form of government distributes power over particular decisions in special ways, which may neglect the interests of some persons and give great weight to the interests of others. But that is a matter which can be more fully studied when the fundamental points discussed in this paper are better understood. The major point to be made here is that, just as a free market with pure competition can be conceived in economic exchange, and used as a theoretical model from which actual systems can be examined, a similar model of pure competition can be conceived in collective decisions. Although actual social systems deviate from this model, it can nevertheless serve as a basis from which the deviations can be studied.

III. *An Ideal System of Collective Decisions*

The system of collective decisions differs in several important respects from economic markets. One respect, which we shall not investigate, is that occasioned by time: collective decisions are made in a sequence,

[12] It must be clearly understood that the aggregating function is one in which the sum of each person's utility differences between outcomes over all decisions are weighted by his power over the decisions. Thus the total sum of each person's utilities as used in aggregating is proportional to the sum of his power. The precise definition of this power is given later in the paper.

over an extended period of time, while in some economic markets, time need not be taken into account. Though time, together with discounting, mechanisms of credit-extension, insurance, etc., must play an important part in the further development of the present theory, the elements will be left unexamined here.

Another way in which this case differs from economic markets is in the lesser flexibility in exchange—for example, the relative absence of successive exchange of the same goods. Since the entities being exchanged are promises of an action, these promises are not as negotiable as economic goods, or money by which such goods are counted. Another aspect of the lack of flexibility is the fact that in this case, power leads to realization of interests; but the interest gained cannot be reconverted to power in the same way that economic goods, once gained, can be reconverted in a perfect market to the money which bought them. As a consequence of this last point, and of the lack of a measure of value such as money, it is necessary to devise a different way to express the value of power over a particular issue. The equation to be presented below is consistent with the framework of economic markets, but it is applicable in collective decisions as well, which do not generate their own measure of value.

Let us first define a quantity which is analogous to ΔU_{xi} above, which was the interest of X in gaining a vote on decision i. It was defined as the difference in expected utility due to that vote. Now, let us define analogously r_{ix} as the difference in expected utility given that the decision is under his control (which is merely the utility of the most-preferred alternative), and the unconditional expected utility:

$$r'_{ix} = U_{xa} - E(U_{xi}) \tag{13}$$

where A is the alternative most preferred by X in decision i. This we will call, for heuristic purposes, X's *interest* in the decision. This is assumed to be exogenously given, dependent on the structure of social and economic activities, and the content of decision i.

Also exogenously given is the formal control structure over the issue. In the case of m decisions decided by a vote among n individuals, each person has control of $1/n$ on each of the m decisions. This will be labelled c_{ij}, the control of individual j over issue i. The total resources of individual j consist of his control over all issues, but weighted by the value or "importance" of each decision. If the value of each decision is v_i, then the power of individual j is

$$p_j = \sum_{i=1}^{m} v_i c_{ij}$$

where

$$\sum_{j=1}^{n} c_{ij} = 1.$$

The total control over each decision is 1, that is, $\sum_{j=1}^{m} c_{ij} = 1$. Now given these definitions, the value of a given decision where there is pure competition is the sum of each actor's interests times the power of that actor:

$$v_i = \sum_{j=1}^{n} r_{ij} p_j \tag{14}$$

or

$$v_i = \sum_{j=1}^{n} \sum_{k=1}^{m} r_{ij} v_k c_{kj}. \tag{15}$$

This recursive definition says in effect that the value of a decision is equal to the sum of the interests of each individual in the decision, but weighted by the power of the individual. In turn, the individual's power depends upon the value of those issues over which he has some control.

Solution of equation (15) for the set of v_i can give the value or price which any individual should have to pay to gain control over each decision.[13] Such prices would not be exact in imperfect competition, but where there are enough competitors and enough decisions, the price should come to that point. This gives both the value of an individual's resources (i.e., votes) in the market, and the price he can expect to pay for decisions he wants. It establishes the rate of exchange which should obtain as the market approaches rationality.

IV. *More Fundamental Revision*

In a sense, the above discussion has gone at the problem backwards, showing how a framework of exchange in a free market is possible in collective decisions just as in the exchange of economic goods. This is necessary for historical reasons. But having done so, it is more profitable to attempt to restate the general theory in a way that is appropriate to collective decisions and then show that exchange of private goods is a degenerate case. The basic elements of such a theory will be indicated below, but only to show the way in which such a generalization could proceed.

Consider the world as consisting of a set of *events*. Each event has two or more possible outcomes, and these outcomes have consequences for individuals, which the individuals evaluate differentially. Each event is also characterized by a distribution of control over the outcomes. The sum of such control is less than or equal to 1.0, and it may be conceived

[13] In solving this set of equations for v_i, it should be noted that the row constraint on c_{ij} means that there are only $m-1$ independent v_i's. That is, the values are determined only relative to some arbitrary sum, say

$$\sum_{i=1}^{m} v_i = m.$$

as the probability that the person holding the control can determine the outcome.

Then in a market of exchange of economic goods, the goods themselves are the events. Consumption of the good by a given individual is one outcome of the event. Possession of the good constitutes control over the event. Thus this is a simple situation in which the consequences of a given outcome are zero for all individuals other than the individual who consumes it, and control over the event is always 1.0 for the person in whose possession the goods lie, and 0 for all others.

The fact that makes some events come under multiple control in collective decisions is their multiple consequences, or "externalities." Whatever outcome is chosen, it will have consequences for many individuals. Thus many have an interest in gaining control of it, and various forces (in *other* collective decisions) move such events into the area of collective decisions, with a certain structure of control (such as one vote for one person). Thus collective decisions are events in which control is formally distributed over more than one actor (though it may come into the hands of a single actor, through exchanges). They tend also to be events in which any outcome has consequences for many actors. Because of the divided control, a much more extensive calculation—using expected utility, rather than certain utility—is necessary in carrying out rational behavior. Nevertheless, the structure is the same, and the private-goods exchange case can be treated as a special case in which the control over an event is always 0 or 1. The equations of exchange in the collective decision case should thus reduce to those of the private goods case when the matrix of control takes on a special form.

V. *Conclusion*

One might be tempted to develop these ideas further in a game theoretic direction, since obviously game theoretic considerations are relevant. One referee has suggested that the points developed above are more relevant to game theoretic analysis of political behavior than to Arrow's and subsequent work on a social welfare function. This, however, misses the major import of the paper. Arrow's work, by attempting to derive social choice as a function of individual preference, begins to shift the idea of a social welfare function away from a normative concept to a positive one. I. M. D. Little and others of a normative persuasion have recognized this danger and rejected Arrow's approach as irrelevant to welfare economics. Having begun the shift, however, one cannot stop halfway, but must incorporate in the theory that behavior which the rational actor would carry out. Arrow's theorem depends upon individual rationality under certainty, which allows no expres-

sion of intensity of preference, but only ordering among alternative outcomes. It is clear, however, first, that rational behavior in collective decisions requires rationality under uncertainty or risk, which opens the possibility of expression of intensity of preference; and second, that in actual groups, the existence of a sequence of decisions gives actors the resources that allow the expression of such intensity, even if imperfectly and incompletely.

When one takes this perspective, political behavior can be viewed as a generalization of market behavior, and the functioning of legislatures or other bodies in collective decisions as an extension of a market. In this "market," each actor has only partial control over any given outcome, rather than complete control over a goal, thus requiring the more complex calculations of rational behavior under risk. This extension of rationality has been developed as part of game theory; but to categorize the present results as work in game theory trivializes them by limiting their use to micro-systems, in which game-theoretic solutions can be calculated. It would be as if the indeterminacy of outcome in a two-person bargaining situation had constrained economists from developing the concept of a perfect market, and the power that brings to economic analysis.

Similarly, in collective decisions, disregarding the confusion and indeterminacy of game-theoretic solutions in a micro-system (for small-number interaction cases are always the most difficult for analysis), one can develop an idea of a perfect system of collective decisions, and thereby derive the value of control over a given decision, and the power of a given actor, and can ultimately develop for such decisions the tools of economic analysis.

References

1. K. J. Arrow, *Social Choice and Individual Values*. New York 1951.
2. Duncan Black, *A Theory of Committees and Elections*. Cambridge, England 1958.
3. J. M. Buchanan and Gordon Tullock, *The Calculus of Consent*. Ann Arbor 1962.
4. Anthony Downs, *An Economic Theory of Democracy*. New York 1957.
5. Clifford Hildreth, "Alternative Conditions for Social Orderings," *Econometrica*, 1953, *21*, 89–90.
6. R. A. Musgrave, *The Theory of Public Finance*. New York 1959.
7. Peter Newman, *The Theory of Exchange*. Englewood Cliffs 1965.
8. Mancur Olson, Jr., *The Logic of Collective Action*. Cambridge 1965.
9. Jerome Rothenberg, *The Measurement of Social Welfare*. Englewood Cliffs 1961.
10. Knut Wicksell, "A New Principle of Just Taxation," in R. A. Musgrave and A. T. Peacock, eds., *Classics in the Theory of Public Finance*, New York 1958, pp. 72–118.

A RESTATEMENT OF THE QUANTITY THEORY OF MONEY

By MAURICE ALLAIS*

Up to 1950, there was no attempt to derive a formulation of the demand for money. The Newcomb-Fisher equation of exchange, Walras' formulation, and those of the writers of the Cambridge School—Marshall, Pigou, and Keynes—have had little value other than as purely formal frameworks for the description of the facts.[1] A new formulation which has an operational and general significance is proposed in this paper: the hereditary, relativistic, and logistic formulation of the demand for money. The significance of this formulation can be fully appreciated only by considering it against the background of the results which were obtained some ten years earlier.

I. *Earlier Results*

During 1953–54, Cagan [10] and Allais [3], both working independently of each other, tried to find an operational formulation of the quantity theory. They both reached the same formulation, although their starting point presentation and terminology were different.[2,3] This formulation was:

$$(1.1)\quad \text{(a)}\ \frac{M}{P} = \phi(u) = \phi_0 e^{-Ku} \quad \text{(Cagan)} \qquad \text{(b)}\ \frac{M}{D} = \phi(u) = \phi_0(1 - Ku) \quad \text{(Allais 1954)}$$

$$(1.2)\quad \text{(a)}\ \frac{du}{dt} = \bar{\chi}(x - u) \qquad \text{(b)}\ u = \frac{\int_{-\infty}^{t} x(\theta) e^{-\bar{\chi}(t-\theta)} d\theta}{\int_{-\infty}^{t} e^{-\bar{\chi}(t-\theta)} d\theta}$$

$$(1.3)\quad \text{(a)}\ x = \frac{1}{P}\frac{dP}{dt} \quad \text{(Cagan)} \qquad \text{(b)}\ x = \frac{1}{D}\frac{dD}{dt} = \frac{1}{P}\frac{dP}{dt} + \frac{1}{Q}\frac{dQ}{dt} \quad \text{(Allais)}$$

* The author is professor of general economics, Ecole Nationale Supérieure des Mines; professor of theoretical economics, Statistical Institute of the University of Paris and research director, French National Center for Scientific Research.

[1] For an account of the historical development of the quantity theory see [8, pp. 27–39 and 153–54].

[2] For clarity, Cagan's formulation is given here in the notation used in this paper. The correspondence between the notations is as follows:

Cagan (1954)	C	E	α	β	$e^{-\gamma}$
Allais (1954)	x	u	K	$\bar{\chi}$	ϕ_0

[3] As far as I am aware, the formulation (1.2a) was suggested to Cagan by Milton Friedman, who later used it in his theory of permanent income [12, pp. 142–45]. Cagan's research was brought to my attention by Friedman in a discussion we had in July 1954 when I described to him the interesting results I had reached using the formulation (1.2b) in my research on the theory of the business cycle [3].

where D is total outlay, P the price level, Q the activity index, and ϕ_0, K, and $\bar{\chi}$ are constants. Total outlay D can be considered, at least as a first approximation, proportional to national income R at current prices, a property which is expressed by the relation

$$\frac{D}{R} = \text{a constant.} \tag{1.4}$$

For hyperinflations, it is approximately true that

$$\frac{D}{P} = \text{a constant.} \tag{1.5}$$

If the case of hyperinflation is excluded, u is small and

$$e^{-Ku} \approx 1 - Ku. \tag{1.6}$$

It follows from this that the formulations (1.1) employed by Cagan and Allais were not only the same, but they also applied in the same way to concrete economic data. Allais called the quantity u the psychological rate of expansion; Cagan called it the coefficient of expectation. Cagan laid most stress on the differential equation (1.2a); Allais on the equivalent integrated expression (1.2b). For Cagan, future expectations were the important factor; for Allais, the memory of past events; but these were only two facets of one and the same mathematical formulation. For both Cagan and Allais, the only way to calculate the coefficient u was to consider the approximation

$$u_n = \frac{\bar{x}_n + k\bar{x}_{n-1} + \cdots + k^p\bar{x}_{n-p} + \cdots}{1 + k + \cdots + k^p + \cdots} \tag{1.7}$$

with

$$\bar{x}_n = \frac{1}{p}\left[l_n D_n - l_n D_{n-1}\right] \tag{1.8}$$[4]

where p is the length of the interval of elementary time considered, with

$$k = e^{-p\bar{\chi}}. \tag{1.9}$$

Relation (1-7) indicates that the coefficient u is merely the weighted average of earlier rates of growth of total outlay, with weighting coefficients which decline exponentially with distance in time. For Allais, however, this formulation was merely one component of a more general theory of monetary dynamics based on the relation

[4] The notation ln represents the natural logarithm.

$$\frac{dD}{dt} = \lambda[M - M_D] \tag{1.10}$$

where λ is a constant and M_D the desired cash balances. But because the difference $M-M_D$ is always small, Allais could determine the function ϕ statistically by taking the ratio M/D and assuming

$$M_D \approx M. \tag{1.11}$$

Cagan's formulation assumed implicitly that

$$M = M_D. \tag{1.12}$$

Overall, both authors followed the Walras-Cambridge school formulation. The terminology employed by Allais places him in the Walrasian line of thought, whereas Cagan's terminology is in the tradition of the Cambridge school. Fundamentally, however, since Allais considered the ratio

$$\frac{M}{D} = \frac{M}{PQ}, \tag{1.13}$$

his formulation was that of the Cambridge school, whereas Cagan, considering the ratio M/P, was using the Walrasian formulation. Leaving aside differences in terminology, the coefficient used by both authors was the weighted average of earlier rates of growth of total outlay, with weighting coefficients declining exponentially with distance in time.

Overall, the results obtained by Cagan in 1954 were excellent, while Allais' results (1954) for France and the United States were quite remarkable, and in general provided a satisfactory fit of the data. The only significant exception was the period 1820–1870 for France, but the figures used for money in circulation and national income were highly questionable. However, Cagan had only succeeded with his formulation in representing the central period of the hyperinflations studied. It was still beyond the power of his formulation to deal satisfactorily with the beginning and terminal stages. Furthermore, he had been no more successful than Allais in finding a single formulation capable of dealing with all the cases studied.[5] The main results reached by the Allais (1954) and Cagan (1954) formulations are presented in Table 1.

The values found for $\bar{\chi}$ and K are *very different*, and the different laws cannot be brought into any consistent relationship with each other. But two remarks may be made: (1) the values of the product $K\bar{\chi}$ are remarkably steady and are all in the neighborhood of 0.75; (2) the rate of forgetfulness $\bar{\chi}$ is the greater, and the coefficient K the smaller, the higher

[5] Cagan [10, pp. 43–46 and 55–57].

TABLE 1—SUMMARY OF CAGAN'S AND ALLAIS' RESULTS: 1954

(Series including deposits)

	Country	Period	N[a]	ρ[b]	$1-\rho^2$	$\bar{\chi}$	K	$\bar{\chi} K$	Average u of the u_n
Allais (1954)	France	1820–1848 1848–1867 1871–1913	29 20 43	0.520 0.320 0.970	0.730 0.900 0.060	0.005 0.006 0.022	211 169 44	1.05 0.98 0.92	
	United States	1871–1935 1918–1941	65 24	0.950 0.977	0.100 0.045	0.008 0.007	57.2 67.7	0.48 0.48	0.034
	Average			0.747	0.367	0.010	109.8	0.78	
Cagan (1954)	Germany Austria Greece[c] Hungary I Hungary II Poland U.S.S.R.[c]	Sept. 1920–July 1923 Jan. 1921–Aug. 1922 Jan. 1943–Aug. 1944 July 1922–Feb. 1924 July 1945–Feb. 1946 Apr. 1922–Nov. 1923 Jan. 1922–Feb. 1924	35 20 20 20 8 20 26	0.992 0.989 0.980 0.926 0.998 0.972 0.971	0.015 0.021 0.040 0.142 0.004 0.056 0.057	0.20 0.05 0.15 0.10 0.15 0.30 0.35	5.46 8.55 4.09 8.70 3.63 2.30 3.06	1.09 0.43 0.61 0.87 0.54 0.69 1.07	0.168 0.094 0.261 0.127 0.485 0.254 0.395
	Average			0.976	0.048	0.20	5.11	0.76	0.255
	General average			0.880	0.181	0.112	48.73	0.77	

[a] N = number of observations.

[b] ρ = coefficient of correlation between observed and calculated values of M/R (Allais) or $\ln[M/P]$ (Cagan).

[c] Excluding deposits.

the average value $\bar{u}$ of the rate of expansion during the period considered. Further, since the value of relative desired cash balances ϕ is the smaller the higher the value of u, it may be said that the coefficient of forgetfulness is the higher the lower the value of relative desired cash balances, or the greater the velocity of circulation.[6]

II. *The Hereditary, Relativistic, and Logistic Formulation of the Demand for Money*

A. *The H.R.L. Formulation*

The H.R.L. formulation of the demand for money, the fruit of fifteen years of research,[7] is based essentially on the following three ideas.

1. Monetary theory requires a formulation based on a general theory which can bring all monetary phenomena into the same logical framework, whether they relate to equilibrium situations, to cyclical fluctuations, or to hyperinflations.

2. At any given moment, the relative demand for money—in other words, the demand for money in relation to the national income—depends only on the historical development of the nominal value of national income. There is thus a "hereditary" effect, with the influence of past events becoming attenuated as they fall farther back in time.

3. Provided that a psychological time scale is considered such that the rate of forgetfulness per unit of time is constant, the "hereditary" expression of the demand for money is the same at all places and times.

The postulates of the H.R.L. formulation were derived from the results of the previous researches only after a long prior analysis.[8] This paper is limited to their enunciation, and the deduction of the consequences flowing from them.

B. *Relativistic Postulate* (*Postulate I*)

In relation to physical time, it is assumed that an instanteous coefficient of forgetfulness, $\chi(t)$, can be defined such that the effect of attrition of the memory of past events between τ and t can be represented by the coefficient

$$\exp - \int_{\tau}^{t} \chi(u)du \tag{2.1}$$

which has an exponential form. A psychological time scale t' can then

[6] For more details see [8, pp. 159–80].

[7] Allais [1] to [8].

[8] The starting point of the formulation below was my books *Economie et interêt* [1] and *Les fondements comptables de la macro-économique* [4] and the research which I undertook between 1952 and 1955, which is described in three successive papers [2] [3] [5]; for more details see [6] [8] [9].

be defined such that

$$\chi(t)dt = \chi' dt'. \tag{2.2}$$

where χ' is a constant; i.e., when measured in relation to psychological time, forgetfulness per unit of time is constant. In other words, by reference to this psychological time scale, memory is invariant.[9]

This leads to the relation

$$t' = h(t) \tag{2.3}$$

linking psychological and physical time.

Writing $D(t)$ for total outlay per unit of physical time by all economic agents,

$$\begin{aligned} D(t) &= \sum_i p_i(t)q_i(t) \\ &= P(t)Q(t) \end{aligned} \tag{2.4}$$

where p_i and q_i represent the price and volume of the various basic transactions i, and where P and Q are appropriate indices of price and of economic activity. When referred to the psychological time scale, total outlay per unit of time is $D'(t')$, whence

$$D'(t')dt' = D(t)dt. \tag{2.5}$$

However, it is assumed that the macroeconomic quantity taken into consideration by economic agents in the process of monetary decision-making is total outlay $D(t)$, whatever the time-scale referred to. In other words, it is assumed that $D(t)$ is an invariant psychological datum, independent of any time-scale reference.[10] This hypothesis was initially developed on the basis of psychological considerations, and its retention is attributable to the success of its application in the analysis of the empirical series.

When expressed in terms of the psychological time scale, the rate

$$x'(t') = \frac{1}{D(t')} \frac{dD(t')}{dt'} \tag{2.6}$$

will be referred to as the instantaneous rate of increase of total outlay. Where physical time units are employed, this rate is given as[11]

$$x(t) = \frac{1}{D(t)} \frac{dD(t)}{dt} \tag{2.7}$$

[9] This assumption had already been formulated in my 1955 paper [5, §26, pp. 273–74, relation 48].

[10] See note (11) below.

and of course, from (2.6) and (2.7)

$$x'dt' = xdt. \tag{2.8}$$

The first postulate thus signifies that there exists a psychological time t' which differs from physical time t. Measured against this psychological time scale, the rate of forgetfulness per unit of time is assumed to be independent of the period under consideration. This condition defines psychological time. Furthermore, the quantity of money M and total outlay per unit of physical time $D(t)$, are taken to be accepted by economic agents as invariant psychological data, independent of the time scale used for reference.[12]

C. Hereditary Postulate (Postulate II)

Decisions by economic agents are assumed to be taken in relation to past changes in total outlay, represented by an index which is termed "the rate of psychological expansion." In relation to the psychological time scale, the "psychological rate of expansion" is assumed to be a weighted average of all preceding rates of expansion and is defined by the relation

$$z' = \frac{\int_{-\infty}^{t'} x'(\tau')e^{-\chi'(t'-\tau')}d\tau'}{\int_{-\infty}^{t'} e^{-\chi'(t'-\tau')}d\tau'} \tag{2.9}$$

where χ' is a constant. This relation specifies the "hereditary" nature of the link that exists between the psychological rate $z'(t')$ and the instantaneous rates $x'(\tau')$.[13] The weighting coefficients

$$e^{-\chi'(t'-\tau')}$$

decline exponentially with time when referred to the psychological time scale. It follows from relation (2.9) that

$$\frac{dz'}{dt'} = \chi'[x' - z']. \tag{2.10}$$

---

[11] And *not*, it cannot be too strongly stressed

$$x'(t') = \frac{1}{D'(t)} \frac{dD'(t)}{dt'}.$$

To consider D and not D' means that psychologically it is D which constitutes an invariant concept from the point of view of appreciation of the economic situation. On this point see [8, pp. 80–81].

[12] On relativistic effects in the social sciences, see [8, pp. 23–25].

[13] On hereditary effects in the social sciences, see [8, pp. 21–22].

Postulate II is thus equivalent to the assumption that the formulation (1.2) used by Cagan and Allais in 1954 is valid, but that it is valid only when considered in relation to the psychological time scale. The parameter z' is therefore a magnitude which may be compared with the parameter u of Section I. Similarly χ' is comparable to the coefficient $\bar{\chi}$ of that section.

Using the definition of z in the physical time scale

$$z(t) = z'(t'); \tag{2.11}$$

then from the relation (2.3) and (2.8), and using physical time, we have

$$\frac{dz}{dt} = \chi'\left[x - \frac{\chi}{\chi'} z\right]. \tag{2.12}$$

The behavior of economic agents insofar as their desire to hold money is concerned can be measured by

$$\phi_D = \frac{M_D}{D}, \tag{2.13}$$

i.e., by the ratio between desired money balances M_D and total outlay per unit of physical time, D. This ratio is referred to throughout as "relative desired money balances"; it can also be designated as the relative demand for money.[14] It is assumed that relative desired money balances ϕ_D are a function of z:

$$\phi_D = \phi(z). \tag{2.14}$$

Thus, Postulate II signifies that the monetary behavior of economic operators, when referred to a psychological time scale, is a function of a psychological rate of expansion. This rate is found as the weighted average of the instantaneous rates of increase x' of total outlay D, measured against the psychological time scale, with the weighting coefficients declining exponentially with the passage of time, and the coefficient of forgetfulness χ' being constant.

D. *The Postulate of Invariance of the Function of Relative Desired Money Balances When Referred to the Psychological Time Scale (Postulate III)*

The constant ϕ_0 and the function $\varphi(z)$ are defined by the relation

$$\phi_0 = \phi_D(0)^{15} \tag{2.15}$$

[14] In earlier papers, [1] to [7], I used the term "real value of wanted cash balances," but this suggested that ϕ_D was derived by dividing M by P, whereas in fact it is the quotient resulting from division by the product PQ. This was a possible source of confusion and I have altered the terminology.

[15] It follows from this that ϕ_0 is not the initial value of ϕ, but the value of ϕ for $z=0$.

and

$$\varphi(z) = \frac{\phi(z)}{\phi_0}. \tag{2.16}$$

It is assumed that φ is an invariant function of z, i.e., that it is independent both of circumstances and of the institutional framework. This function is designated as the "function of desired money balances."

Thus, Postulate II signifies that, subject to application of a given coefficient, relative desired money balances are a function of the psychological rate of expansion which is independent of time and place.

E. *The Postulate of a Constant Velocity of Circulation of Desired Money Balances when Referred to a Psychological Time Scale (Postulate IV)*

When related to a psychological time scale, the velocity of circulation V'_D of desired money balances can be defined by the relation

$$D' = M_D V'_D \tag{2.17}$$[16]

where D' represents total outlay per unit of psychological time. It is assumed that, as for the coefficient of forgetfulness χ', the velocity V' is constant.

When the physical time-scale is referred to, we have

$$D = M_D V_D \tag{2.18}$$

where V_D is the velocity of circulation of desired money balances M_D measured in relation to physical time, and from (2.13)

$$V_D = \frac{1}{\phi_D}. \tag{2.19}$$

The velocity of circulation of desired money balances thus appears as the reciprocal of relative desired money balances. From relations (2.2), (2.5, (2.14), (2.17), (2.18) and (2.19), we have

$$\frac{\chi}{\chi'} = \frac{1}{V'_D} \frac{1}{\phi(z)}. \tag{2.20}$$

The instantaneous coefficient of forgetfulness χ thus appears as a function of z. We see that the coefficient of forgetfulness is inversely proportional to the level of relative desired money balances.

Postulate IV thus signifies that the velocity of circulation of desired

[16] M_D is invariant, i.e.

$$M'_D = M_D$$

where M'_D represents the value of desired money balances with reference to the psychological time scale.

money balances, when referred to a psychological time scale, is assumed to be a constant, independent of time. It follows from this and the preceding postulates that the rate of forgetfulness is inversely proportional to relative desired money balances, i.e., proportional to the velocity of circulation of desired cash balances.

It is natural to assume that for

$$z = z' = 0 \tag{2.21}$$

the instantaneous scale of psychological time is identical to the physical time scale. This implies that

$$\chi_0 = \chi(z = 0) = \chi'.\text{[17]} \tag{2.22}$$

From (2.14), (2.15) and (2.20)

$$\chi_0 = \frac{\chi'}{V'_D}\frac{1}{\phi_0} \tag{2.23}$$

i.e., from (2.22)

$$V'_D = \frac{1}{\phi_0} \tag{2.24}$$

so that from (2.20)

$$\frac{\chi}{\chi'} = \frac{\phi_0}{\phi(z)} \tag{2.25}$$

and also, from (2.16)

$$\frac{\chi}{\chi'} = \frac{1}{\varphi(z)} \tag{2.26}$$

and whence, from (2.12) and (2.22)

$$\frac{dz}{dt} = \chi_0\left[x - \frac{z}{\varphi(z)}\right]. \tag{2.27}$$

F. *The Hereditary Relativistic Formulation*

To facilitate the econometric analysis, it is advantageous to consider the index Z defined by the relation

$$Z(t) = \frac{z(t)}{\chi_0}. \tag{2.28}$$

[17] It follows from this assumption that in a stationary process in which total outlay D is constant, the psychological time scale is identical to the physical time scale. In this case, $x(t) = 0$, so that $z(t) = 0$, whence $\chi = \chi_0$ and, from (2.2), $dt = dt'$.

Z has no dimension with reference to time, and it can for this reason be denoted as the "intrinsic psychological rate of expansion." Putting

$$\varphi(z) \equiv \psi(Z) \tag{2.29}$$

from the set of Postulates I, II, III, and IV, we can write (relations 2.13, 2.14, 2.16, 2.29, 2.27, and 2.28)

$$\phi_D = \frac{M_D}{D} \tag{2.30}$$

$$\frac{\phi_D}{\phi_0} = \psi(Z) \tag{2.31}$$

$$\frac{dZ}{dt} = x - \frac{\chi_0 Z}{\psi(Z)} \tag{2.32}$$

where ψ is a determinate function of Z.

This formulation can be thought of as being hereditary and relativistic; hereditary because of relation (2.9), relativistic because of relation (2.2). For simplicity, it may be designated as the "H.R. formulation." In fact, this formulation leads to the following assumptions: (a) that the velocity of circulation of desired money balances

$$V_D(t) = \frac{D(t)}{M_D(t)} \tag{2.33}$$

is a function of time only through the intermediary agency of the psychological rate of expansion $Z(t)$, which summarizes the past history of the rate of growth $x(t)$ of nominal total outlay;[18] and (b) that if the time scale is modified in such a way as to render the instantaneous rates of forgetfulness constant—in other words if the the passage of time is so regulated that the memory of past events fades at a constant rate—the velocity of circulation of desired money balances is constant when measured against this new reference scale. This is equivalent to assuming that in physical time the velocity of circulation of desired money balances $V_D(t)$ is proportional to the instantaneous rate of forgetfulness $\chi(t)$. The H.R. formulation does not determine the function $\psi(Z)$; this can only be done by the introduction of further postulates.[19]

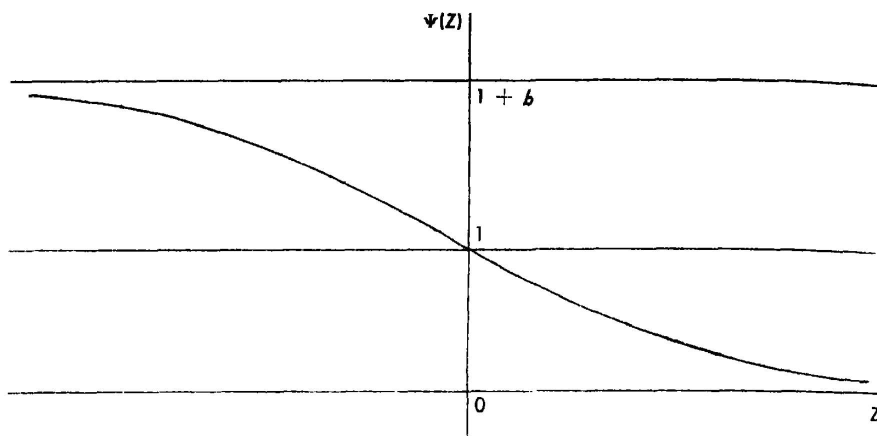
G. *The Logistic Postulate* (*Postulate V*)

The following postulate has been derived by inductive reasoning: The relative change in relative desired money balances is proportional

[18] Relations (2.9), (2.11), and (2.28).

[19] M and M_D of course are quantities without any time dimension. $D(t)$, $\chi(t)$, $x(t)$, $z(t)$, and $V_D(t)$ are quantities whose dimension is inverted time. $\phi(t)$ and $\varphi(t)$ have a time dimension.

CHART 1.



to the change in the psychological rate of expansion, the coefficient of proportionality being proportional to the relative gap between relative desired money balances and their maximum, which is assumed to be finite.

This property is translated by the relation

$$\frac{1}{\psi}\frac{d\psi}{dt} = -\alpha\frac{\psi_M - \psi}{\psi_M}\frac{dZ}{dt} \tag{2.34}$$ [20]

in which ψ_M designates the maximum value of ψ, whence, by integration,

$$\psi(Z) = \frac{1 + b}{1 + be^{\alpha Z}} \tag{2.35}$$

where α and b are two constants and

$$\psi_M = 1 + b. \tag{2.36}$$

Postulate V thus signifies that the function ψ is a logistic function of Z. The shape of the curve of the function $\psi(Z)$ is shown in Chart 1.

H. *H.R.L. Formulation*

From relations (2.29), (2.30), (2.35), (2.9), (2.11), (2.8), (2.2), (2.28), (2.26), and (2.22), the hereditary, relativistic, and logistic formulation,

[20] The assumption of proportionality of $(1/\psi)(d\psi/dt)$ and dZ/dt is quite natural, but if ψ is assumed to have ψ_M as a maximum value, the coefficient of proportionality should cancel out for $\psi=\psi_M$. The simplest assumption, then, is to assume that the coefficient of proportionality is linear in ψ.

which may for brevity be referred to as the H.R.L. formulation, and which follows from Postulates I to V, is summarized by the following relationships:

$$\phi_D = \frac{M_D}{D} \tag{2.37}$$

$$\frac{\phi_D}{\phi_0} = \psi(Z) \tag{2.38}$$

$$\psi(Z) = \frac{1 + b}{1 + be^{\alpha Z}} \tag{2.39}$$

$$Z(t) = \int_{-\infty}^{t} x(\tau) \exp\left[- \int_{\tau}^{t} \chi(u)du \right] d\tau \tag{2.40}$$

$$\frac{\chi}{\chi_0} = \frac{1}{\psi(Z)} \tag{2.41}$$

or, in differential form[21]

$$\frac{1}{\psi}\frac{d\psi}{dZ} = -\alpha\left[1 - \frac{\psi}{1 + b}\right] \tag{2.42}$$

$$\frac{dZ}{dt} = x - \frac{\chi_0}{\psi(Z)} Z \tag{2.43}$$

The parameters α and b have no dimension. They specify the form of the function $\psi(Z)$. The coefficient χ_0 specifies the rate of forgetfulness of past events when the value of Z is zero.

I. *Postulates Concerning the Constants*

The constants α, b, and χ_0 of the H.R.L. formulation can be determined from three postulates: the asymptotic postulate, the postulate of conjunctural symmetry,[22] and the postulate of temporal symmetry.

The Asymptotic Postulate (*Postulate VI*) When the relative demand for money is very small, so that changes of its square can be neglected, although the changes themselves are not necessarily negligible, the rate of psychological expansion is equivalent to the rate of increase of the circulation of money referred to the psychological time scale. Mathematically, this postulate can be written

[21] Relations (2.34) and (2.32).

[22] In French "Postulat de symétrie conjoncturelle." In French, the phrase "appreciation de la conjoncture" relates to the assessment of the economic situation and its development taking both terms in a broad sense.

$$z = \frac{1}{M_D}\frac{dM_D}{dt} \to 0 \quad \text{when} \quad \psi \to 0 \quad \text{and} \quad \frac{d\psi^2}{dt} \to 0 \tag{2.44}$$

$$\text{even if} \quad \frac{d\psi}{dt} \quad \text{does not tend to zero.}$$

In this case, it can be shown that it follows necessarily that

$$\alpha = 1. \tag{2.45}$$

It can in fact be shown that the H.R.L. formulation implies[23]

$$\frac{1}{M_D}\frac{dM_D}{dt'} - z = \frac{\left(1 - \alpha + \frac{\psi b}{1+b}\right)}{\alpha\left(1 - \frac{\psi}{1+b}\right)}\frac{d\psi}{dt}, \tag{2.46}$$

whence it follows immediately that Postulate VI implies (2.45).

The significance of this postulate becomes clear if it is assumed that (as experience confirms) the difference $(M - M_D)$ always remains relatively small,[24] i.e., that in practice

$$\frac{1}{M_D}\frac{dM_D}{dt'} \approx \frac{1}{M}\frac{dM}{dt'}. \tag{2.47}$$

The practical interpretation of Postulate VI is that at the end of a hyperinflation the psychological rate of monetary expansion z is equivalent to the rate of growth of the supply of money referred to the psychological time scale, which appears to be a quite reasonable property to assume.

It is interesting to note that from (2.46) this property is always valid whatever α if the assumption is made that $d\psi/dt$ tends to zero at the same time as ψ. But in point of fact, for none of the hyperinflations to date is it possible to consider $d\psi/dt$ as negligible by comparison with the rate $z = \chi_0 Z$.[25]

The Postulate of Psychological Conjunctural Symmetry (*Postulate* VII). Starting from stability $(Z=0)$, the intensity of the differential behavior of the economic agents is assumed to be the same, whether expansion or recession is in progress. This condition implies that the derivative $d\psi/dZ$ of relative desired money balances with respect to Z is a paired function of Z in the neighborhood of $Z=0$. It can then be deduced immediately that

[23] Relations (2.2), (2.6), (2.10), (2.11), (2.13), (2.16), (2.29), and (2.42). See [8, pp. 82–84].

[24] See below, condition (3.1).

[25] As will be seen from the fittings obtained.

(2.48) $$b = 1.$$

The Postulate of Psychological Temporal Symmetry (Postulate VIII). Economic agents are assumed to take the past into consideration as they do the future. This postulate implies that

(2.49) $$\chi = i$$

where i is the pure rate of interest.

It follows that

$$\chi_0 = i_0 \qquad \text{for} \qquad Z = 0.$$

For the period 1880–1956, the pure rate of interest i_0 can be taken as having been 5 per cent per annum in the United States, for which country satisfactory estimates are available.[26] Thus we can take

$$i_0 \approx 0.004 \text{ per month}$$

so that for all countries for which the rate i_0 can be considered as practically equal to its value in the United States

(2.50) $$\chi_0 = 0.004$$

where the time unit is the month.

J. *The Hereditary, Relativistic and Logistic Law* ($\alpha = 1$, $b = 1$, $\chi_0 = 0.004$)

In sum, the following formulation follows from the eight postulates which have been given earlier:[27]

(2.51) $$\psi(Z) = \frac{2}{1 + e^Z}$$

(2.52) $$\frac{dZ}{dt} = x - 0.002(1 + e^Z)Z.$$

This formulation follows from principles which are, from both an economic and psychological standpoint, extremely simple and quite appealing.

III. *The Confrontation of the Theory and Empirical Data*

A. *Money Balances Desired and Held*

"Desired Money Balances" M_D is a psychological concept and of course no statistical data to measure it are available. However, at any given moment, economic agents are in a position to adjust their money

[26] The exact figure is given by Allais "The Influence of the Capital-Output Ratio on Real National Income," *Econometrica*, Oct. 1962, *30*, p. 714) as 4.87 per cent.

[27] Relations (2.39), (2.43), (2.45), (2.48), and (2.50)

balances M from existing towards desired levels M_D either by spending more or buying less.[28]

Naturally, this adjustment is never perfect, but it can reasonably be suggested that the discrepancy between the actual and the desired value of money holdings is always relatively small. It then follows that it can be assumed at least as a first approximation that

$$\left| \frac{M - M_D}{M_D} \right| < \epsilon \tag{3.1}$$

where ϵ is a small quantity.

It further follows that it is possible to write as a first approximation

$$M_D \approx M \tag{3.2}$$

$$\phi_D \approx \phi \tag{3.3}$$

with

$$\phi = \frac{M}{D} \cdot \tag{3.4}$$

Likewise as a first approximation, it can be assumed that

$$\frac{D}{R} \approx \text{a constant} \tag{3.5}$$

where R is national income at current prices. Thus we can take

$$\phi_D = \frac{M}{R} \tag{3.6}$$

$$x \approx \frac{1}{R} \frac{dR}{dt} \cdot \tag{3.7}$$

During periods of hyperinflation, the index of activity Q does not in general change appreciably and the relation

$$\frac{R}{P} \approx \text{a constant} \tag{3.8}$$

where P is the price index, can be assumed. It follows that for these periods, it is possible to take

$$\phi_D = \frac{M}{P} \tag{3.9}$$

[28] This is naturally only an assumption, but as will be seen, it is one which is justified by its consequences.

$$x \approx \frac{1}{P}\frac{dP}{dt}. \tag{3.10}$$

B. *The Available Statistical Data*

Discrete weekly, monthly, quarterly, or annual series are available directly or may be calculated for the values

$$M_n, \quad R_n$$

or

$$M_n, \quad P_n \quad \text{(hyperinflations)}$$

from which series we can derive the values of

$$x_n = \frac{1}{p}\ln\frac{R_n}{R_{n-1}} \qquad \left(\text{or} \quad \frac{1}{p}\ln\frac{P_n}{P_{n-1}}\right) \tag{3.11}$$

where p is equal to 0.2308, 1, 3, or 12, according to whether the series considered is weekly, monthly, quarterly, or annual, the time unit being the month. χ_n represents the average rate of growth of total outlay between t_{n-1} and t_n.

Fifteen series have been analyzed for nine countries. The data used were those of M. Friedman and M. Allais for the United States, Great Britain, and France, while P. Cagan's figures were used for the seven hyperinflations considered.[29]

C. *Fitting of the H.R.L. Formulation*

From (3.2) and (3.6) it is possible to use

$$M^* = R\phi_D \tag{3.12}$$

as an estimate of M. Then, from (2.38)

$$M^* = \phi_0 R\psi^*(Z) \tag{3.13}$$

where $\psi^*(Z)$ represents the theoretical value of ψ, given by (2.51), with Z determined by the differential equation (2.52). Thus[30]

$$M^* = \frac{2\phi_0}{1 + e^Z} R \tag{3.14}$$

$$\frac{dZ}{dt} = x - 0.002(1 + e^Z)Z \tag{3.15}$$

with

[29] For a discussion of the data used and their characteristics, see [8, pp. 45–63] and [9].

[30] Relations (2.51), (2.52) and (2.2).

$$x(t) = \frac{1}{R}\frac{dR}{dt}. \tag{3.16}$$

If the R_n are taken as given so that the $\bar{x}_n$ are also given, Z_n can be calculated by integration of (3.12) from the values of $\bar{x}_n$ and the initial value Z_1 of Z. If a value of Φ_0 is then taken, M^* can be calculated from equation (3.14). For hyperinflations, of course, the index of national income R is replaced by the price index P.

In each case, the values of the two constants Z_1 and Φ_0 have been fixed by the condition that

$$e^2 = \frac{1}{N}\sum_{n=1}^{n=N} [\ln M_n - \ln M_n^*]^2 \tag{3.17}$$

be a minimum, i.e., by the condition that the sum of squares of the deviations of the calculated from the observed values of the logarithm of M is a minimum. This condition is considered as the criterion of the best possible fit. The minimum ϵ^2 of e^2 can be considered as a coefficient which represents the goodness of the fit.

Practically, rather than consider the initial value Z_1 of Z, it is more convenient to consider the coefficient k defined by the relations

$$\begin{cases} Z_1 = k(Z_1) \\ \dfrac{\phi_1}{\phi_0} = \dfrac{1+b}{1+be^{\alpha(Z_1)}} \end{cases} \tag{3.18}$$

where Z_1 is the value of Z at the initial instant t_1 considered, and (Z_1) represents what the value of Z_1 would be if the empirical point corresponding to instant t_1 were exactly on the fitted curve. For illustrative purposes, the coefficient of correlation ρ between $\ln M_n$ and $\ln M_n^*$ has been calculated for each series fitted.

Once the two values of k and Φ_0 are determined, the calculation of $M^*(t)$ from the values of $R(t)$ is completely independent of the values of $M(t)$. These two coefficients ϕ_0 and k appear as constants of integration.[31]

D. *Results Obtained for the Nominal Value of Desired Money Balances*

The calculations which have so far been made for the nine countries considered at different times show that the course of events is exactly as if social psychological attitudes towards the relative value of desired money balances ψ were practically invariant over time and between different countries and that this invariance is characterized by the three universal constants.

[31] For details on the computations involved in fitting the data, see [8, pp. 96–101].

(3.19) $\alpha = 1 \qquad b = 1 \qquad \chi_0 = 0.004$

Three countries were studied for the whole of the period 1900–1960: France, the United Kingdom, and the United States.[32] Six more countries were studied during periods in which their economies were dominated by hyperinflationary conditions: Germany, Austria, Greece, Hungary, Poland, and Soviet Russia. Table 2 gives the principal results corresponding to the fitting of the same H.R.L. law ($\alpha=1$, $b=1$, $\chi_0=0.004$) to the relevant series.

The parameter p corresponds to the number of months between two successive observations, and n the number of observations. ρ is the coefficient of correlation between the observed and calculated values of $\ln M_n$ and $\ln M^*$, so that $1-\rho^2$ represents the unexplained part of the variance of $\ln M_n$.

It can be seen that the coefficients ϵ^2 in general have very low values. If the coefficients of correlation are examined, it will further be observed that out of fifteen coefficients eleven exceed 0.99; eight are equal to or greater than 0.995, while there are two whose value exceeds 0.999.[33,34,35]

By way of illustration, the values of M_n and M_n^* are plotted against time in Charts 2, 3, 4, 5, 6 for France (1919–1938), United States (1918–1941), Germany (December 1919–October 1923), Hungary (July 1945–July 1946), U.S.S.R. (January 1922–February 1924). The German fitting is particularly excellent. The values of (ψ_n, ψ_n^*) are plotted in Chart 7 as a function of Z_r for France (1947–1962), the United States (1918–1941), Germany (Dec. 1919–Oct. 1923), corresponding in all to 87 pairs of values of M and R. The coefficients of correlation between the 87 pairs of values of ψ_n and ψ_n^* and $\ln \psi_n$ and $\ln \psi_n^*$ are respectively 0.9983 and 0.9959).[36] Chart 8 shows the values of ψ_n and ψ_n^* as a func-

[32] Excluding wartime periods. During such periods, the assumption that is translated by relation (3.1) cannot be considered as valid.

[33] These last two results are practically equal to those obtained in simulation runs on artificial series whose structure corresponded exactly to the H.R.L. law. For example, for the 20-term artificial series $n \cdot IA$, the coefficient of correlation between ψ and ψ^* was 0.99976, whereas for the German figures (excluding deposits) over the period March 1920–October 1923, a total of 43 terms, the coefficient of correlation between ($\ln M$ and $\ln M^*$) was 0.99987.

[34] Naturally, if shorter series only are fitted, it is generally possible to obtain better fits. Lack of space precludes their presentation here. See [6].

[35] It is interesting to note that if the differential equation (2.43) in Z is integrated using the observed values $\psi(Z) = \phi(Z)/\Phi_0$ instead of the theoretical values $\psi^*(Z)$ as calculated by (2.51), the results are less good. For example, for Germany (15th Dec. 1919-16th Oct. 1923, including deposits), the lowest value of ϵ^2 corresponding to the integration using the $\psi(Z)$ is 0.019, whereas it is 0.0032 using the $\psi^*(Z)$.

[36] The respective regression equations are

$$\psi = 0.9872\,\psi^* + 0.0037$$

$$\ln \psi = 0.9991 \ln \psi^* - 0.0030$$

(Series including deposits)

Country	Period	Chart n°	Series	p	N	Index of National Income (or Prices)		Φ_0	k	ϵ^{2} [a]	Correlation ($\ln M$, $\ln M^*$)	
						Start of Period	End of Period				ρ	$1-\rho^2$
France	1898–1913	2	1 J	12	16	0.73	1.00	1.2	1.00	0.00028	0.992	0.015
	1919–1938		2 C	12	20	2.06	7.53	1.4	1.80	0.0104	0.974	0.051
	1947–1962		4	12	16	63.23	587.8	1.8	0.95	0.0019	0.997	0.0051
Great Britain	1925–1940		1 C	12	16	0.95	1.46	1.4	1.10	0.00090	0.973	0.053
	1952–1962		1 E	12	11	3.04	5.27	0.95	0.80	0.00088	0.973	0.053
United States	1895–1915	3	2 C	12	18	0.40	1.09	1.65	1	0.0012	0.997	0.0061
	1918–1941		1	12	24	1.67	2.84	1.8	1	0.00089	0.990	0.021
	1946–1958		4 A	3	52	4.65	9.61	1.5	0.95	0.00067	0.978	0.043
Average (or total)					173				1.08	0.0021	0.984	0.031
Germany	Dec. 1919–Oct. 1923	4	3 bis	1	47	8.03	1.09×10^{9}	1.35	1.0	0.0032	0.9987	0.0026
Austria	Jan. 1921–Aug. 1922		3	1	20	65.91	1.07×10^{4}	2.1	1.1	0.017	0.994	0.013
Greece[b]	June 1941–Oct. 1944		2B bis	1	41	1.00	9.95×10^{8}	3.5	1.0	0.050	0.9982	0.0039
Hungary I[b]	July 1921–Feb. 1924		2 quarter	1	32	1.00	438.00	5.5	1.0	0.018	0.9960	0.0079
Hungary II	July 1945–July 1946	5	3 ter	1	13	105	4.00×10^{29}	1.2	1.0	0.429	0.9991	0.0019
Poland[b]	Jan. 1921–Jan. 1924		2 C bis	1	37	1.00	9.63×10^{8}	14.5	1.0	0.022	0.9992	0.0015
U.S.S.R.[b]	Jan. 1922–Feb. 1924	6	2	1	26	2.88×10^{6}	1.71×10^{10}	0.700	1.0	0.051	0.995	0.011
Average for hyperinflations (or total)					216				1.01	0.027[c]	0.997	0.0060
General average (or total)					389				1.05	0.013[c]	0.990	0.019

[a] ϵ^2 = Average of $[\ln M_n - \ln M_n^*]^2$.

[b] Excluding deposits (series including deposits are not available for the periods considered)

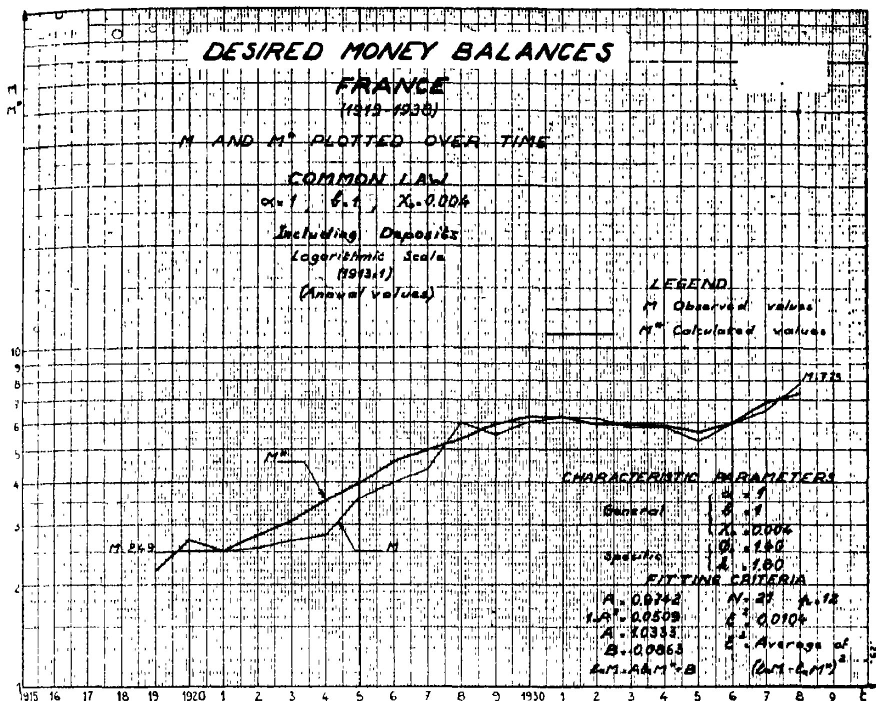


CHART 2

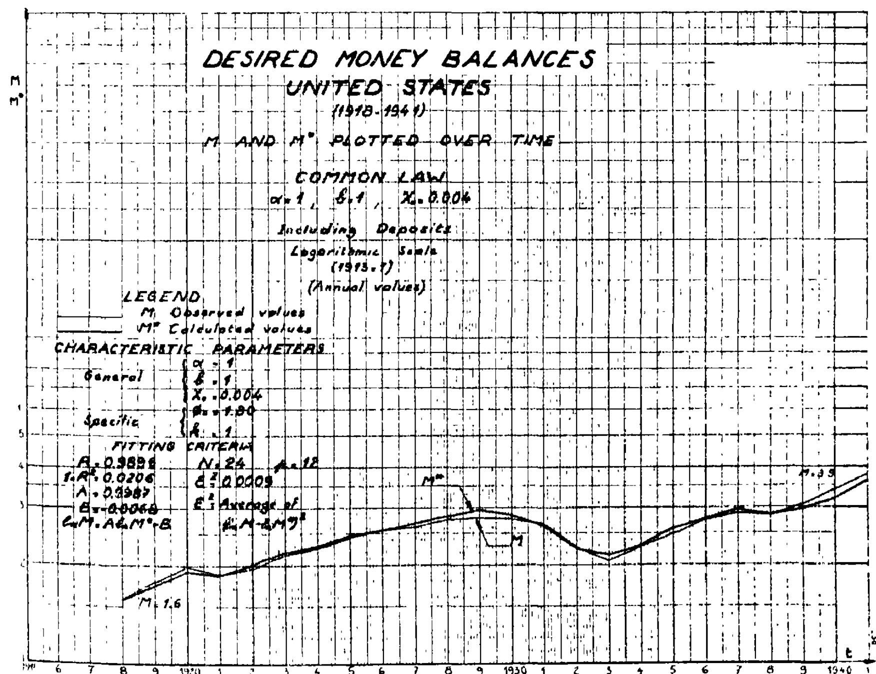


CHART 3

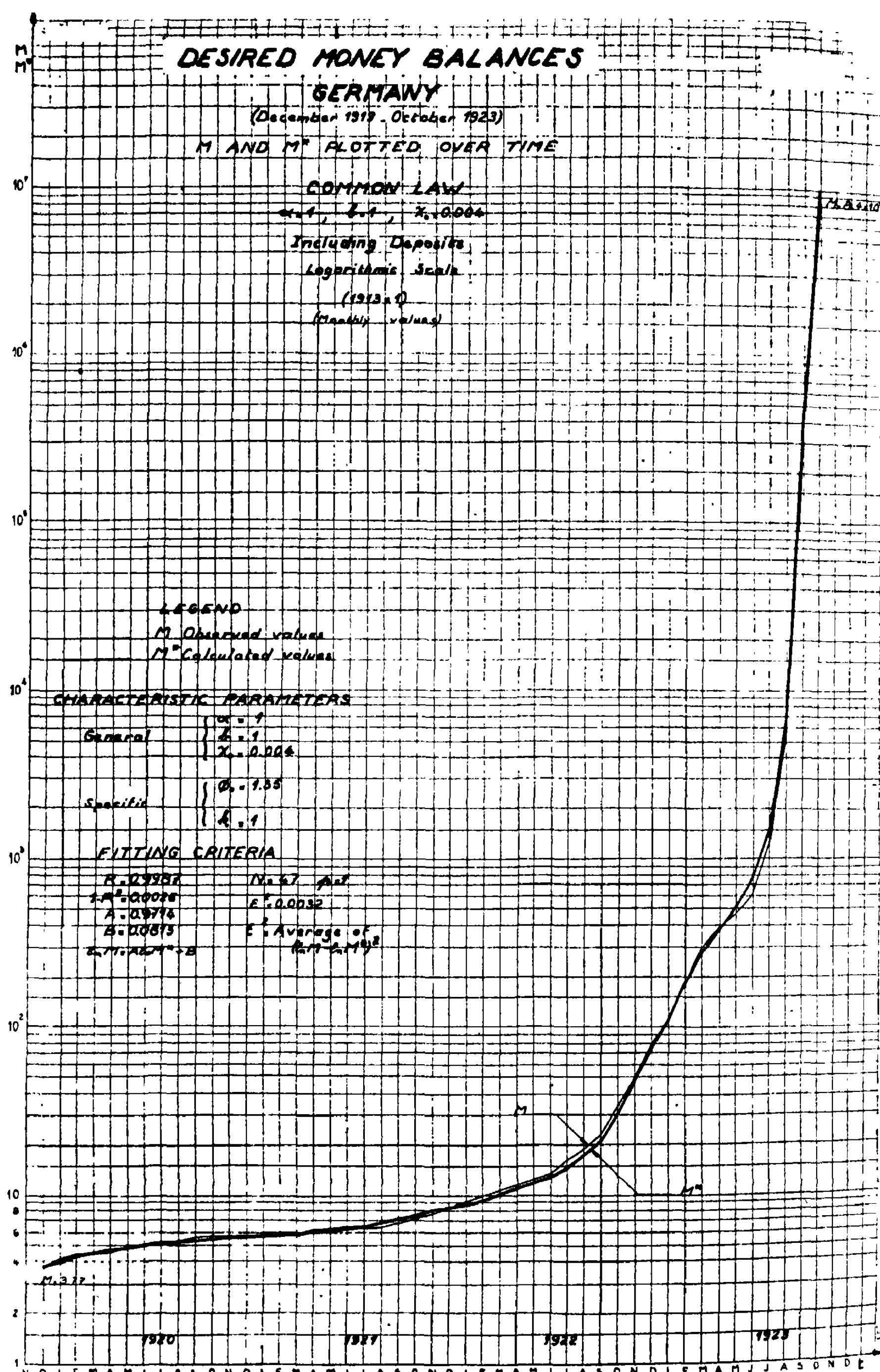

DESIRED MONEY BALANCES
GERMANY
(December 1919 - October 1923)
M AND M* PLOTTED OVER TIME
COMMON LAW
Including Deposits
Logarithmic Scale
(1913 = 1)
(Monthly values)
LEGEND
M Observed values
M* Calculated values
CHARACTERISTIC PARAMETERS
General
Specific
FITTING CRITERIA
E² = 0.0032
M
M*
1920
1921
1922
1923

CHART 4

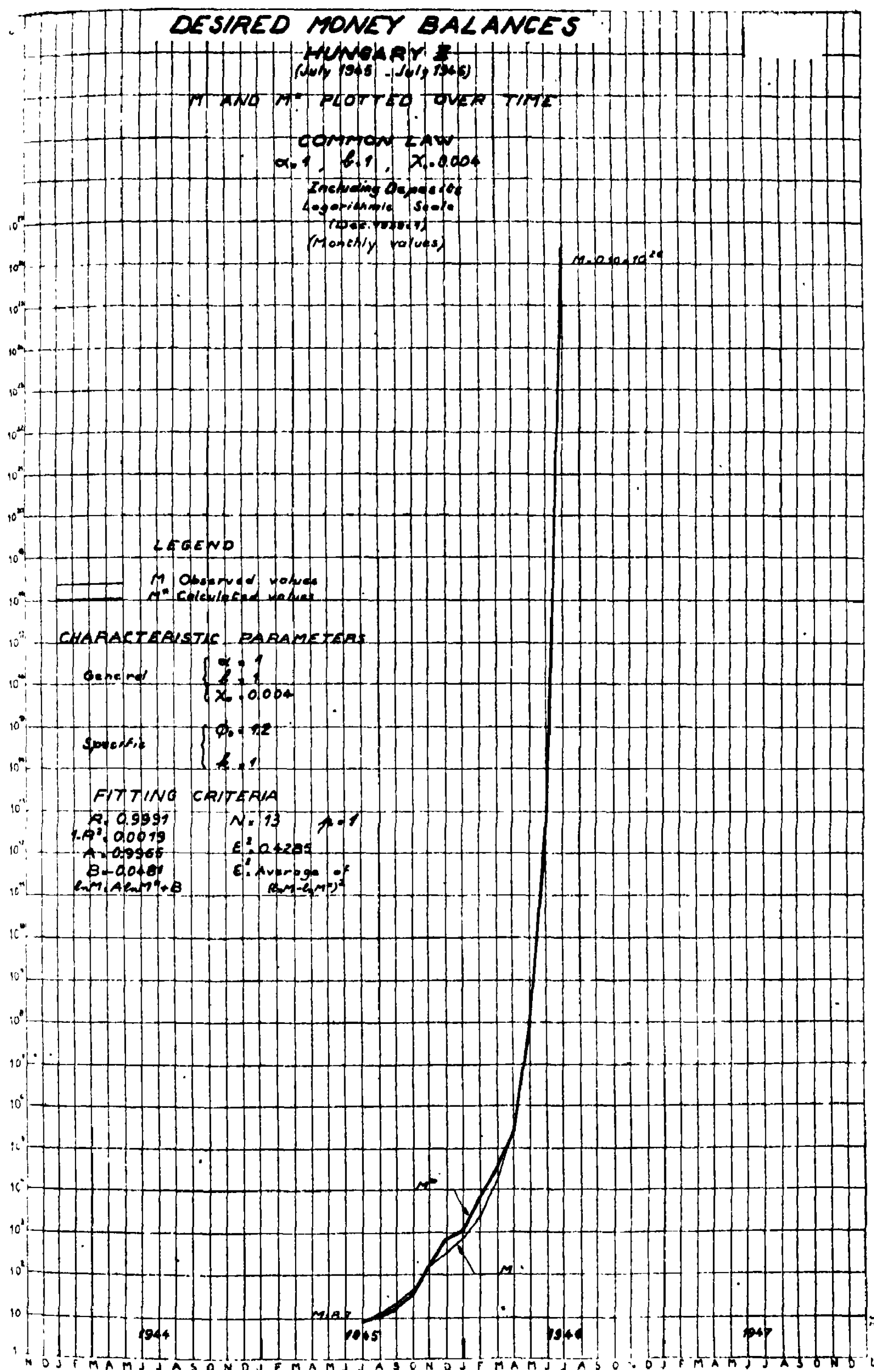

DESIRED MONEY BALANCES
HUNGARY
(July 1945 - July 1946)
M AND M* PLOTTED OVER TIME
COMMON LAW
Including Deposits
Logarithmic Scale
(Monthly values)
LEGEND
M Observed values
M* Calculated values
CHARACTERISTIC PARAMETERS
General
χ = 0.004
Specific
FITTING CRITERIA
R = 0.9991
1-R² = 0.0019
A = 0.9965
N = 13
1944
1945
1946
1947
M*
M

CHART 5

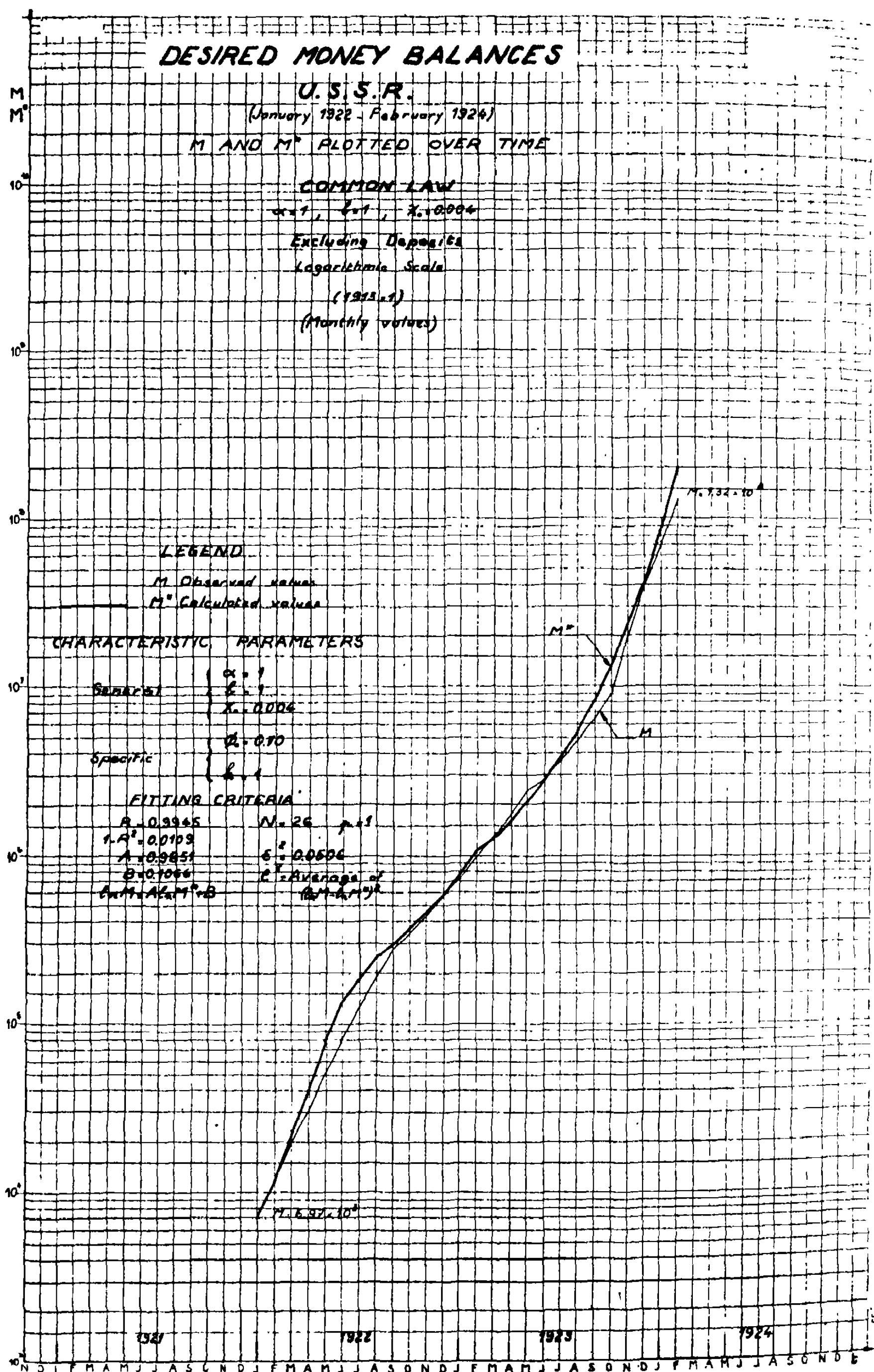

DESIRED MONEY BALANCES
U.S.S.R.
(January 1922 - February 1924)
M AND M* PLOTTED OVER TIME
COMMON LAW
Excluding Deposits
Logarithmic Scale
(Monthly values)
LEGEND
M Observed values
M* Calculated values
CHARACTERISTIC PARAMETERS
General
Specific
FITTING CRITERIA
N = 26
1921
1922
1923
1924

CHART 6

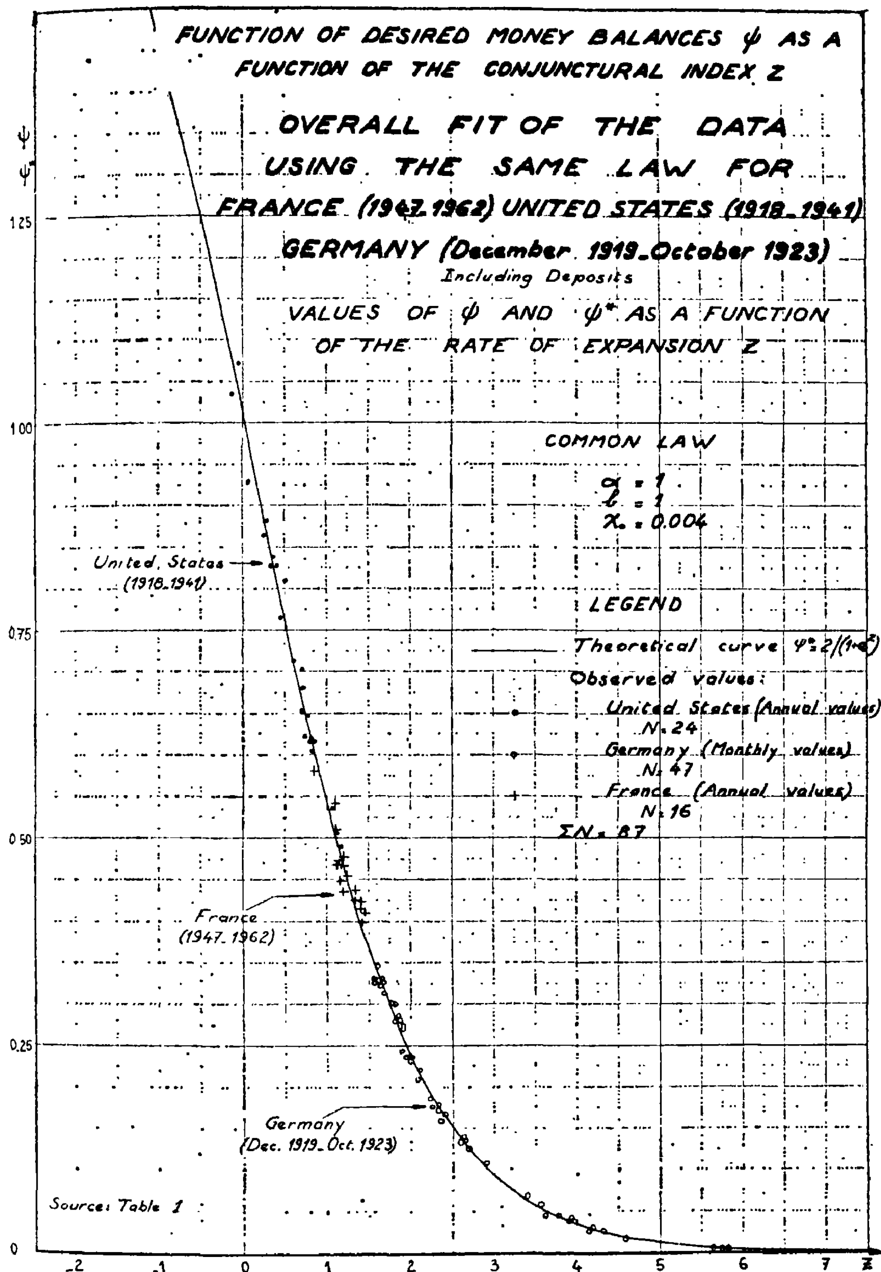

FUNCTION OF DESIRED MONEY BALANCES ψ AS A FUNCTION OF THE CONJUNCTURAL INDEX Z
OVERALL FIT OF THE DATA USING THE SAME LAW FOR FRANCE (1947-1962) UNITED STATES (1918-1941) GERMANY (December 1919-October 1923)
Including Deposits
VALUES OF ψ AND ψ* AS A FUNCTION OF THE RATE OF EXPANSION Z
COMMON LAW
α = 1
b = 1
x₀ = 0.004
LEGEND
Theoretical curve
Observed values:
United States (Annual values) N = 24
Germany (Monthly values) N = 47
France (Annual values) N = 16
ΣN = 87
United States (1918-1941)
France (1947-1962)
Germany (Dec. 1919-Oct. 1923)
Source: Table 1
ψ
ψ*
1.25
1.00
0.75
0.50
0.25
0
-2
-1
0
1
2
3
4
5
6
7
Z

CHART 7

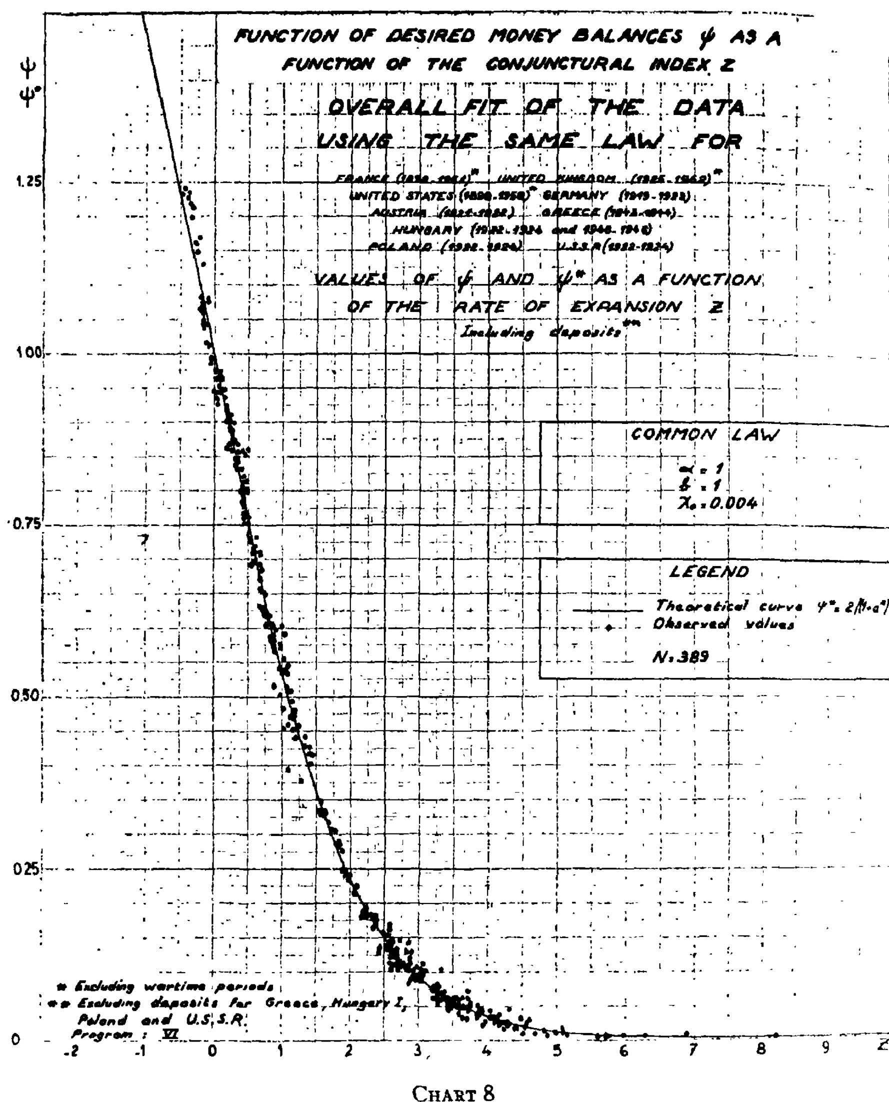


CHART 8

tion of Z_n for the fifteen numerical series in Table 2, in total 389 pairs of values of M and R. The coefficients of correlation between the 389 pairs of values of ψ_n and ψ_n^* and $\ln \psi_n$ and $\ln \psi_n^*$ are respectively 0.9984 and 0.9930.[37]

[37] The respective regression equations are

$$\psi = 0.9965\,\psi^* - 0.0016$$
$$\ln \psi = 1.0087 \ln \psi^* - 0.0076$$

E. The Significance of the Results

As has been seen, the fitting of the time series M, R for a given country over a given period using the same formulation with the same constants

$$\alpha = 1 \qquad b = 1 \qquad \chi_0 = 0.004$$

depends only on the two constants of integration ϕ_0 and Z_1, or ϕ_0 and k. Overall, the agreement between the values forecast by the theory and the observed data is surprising. It goes far beyond what could reasonably have been hoped for on a priori grounds. The values of ϵ^2 and $1-\rho^2$ are so low that the degree of explanation can be considered as complete. The fact that the *same curve* could be fitted to *all* empirical data is the more remarkable when it is considered that, for example, the highest and lowest values of the index of national income in the United States between 1918 and 1941 were 1.08 and 2.84, whereas between December 1919 and October 1923, the index of prices in Germany varied from 15 to 1.09×10^9, while for Hungary, between July 1945 and July 1946, a base value of 105 rose to the astronomical figure of 4×10^{29}.[38]

Once the integration constants k and ϕ_0 have been chosen, the numerical integration of equation (3.15)

$$\frac{dZ}{dt} = x - 0.002(1 + e^Z)Z \tag{3.20}$$

and the calculation of the M_n^* using relation (3.13)

$$M^* = \frac{2\phi_0}{1 + e^Z} R \tag{3.21}$$

with

$$x = \frac{1}{R}\frac{dR}{dt} \qquad \left(\text{or } \frac{1}{P}\frac{dP}{dt} \text{ for the hyperinflations}\right) \tag{3.22}$$

depends only on the value of R or P. The value of M does not enter into the calculation in any way. Thus, once the integration constants k and ϕ_0 are established, the calculated values, M_n^*, are wholly independent of the observed values of M.[39,40]

[38] See Charts 2, 3, and 4.

[39] But, of course, the criterion that the arbitrary coefficients ϕ_0 and k which represent the constants of integration should be so chosen as to minimize ϵ^2 means that both of these parameters depend on the observed values of M_n.

[40] Moreover, it can easily be shown that the influence of the initial value Z_1 of Z, i.e., of the coefficient k, fades progressively, and that it has an asymptotic value of 0. It follows correspondingly that the values of ψ_n^* for the end-period observations are practically independent of the selected value of k, whose influence is restricted to the beginning-of-period values of ψ_n^*.

The results that have been obtained are so extraordinarily close that they seem too good to be true; they lead one to wonder whether they are not necessarily so close, i.e., whether there may not be some implicit circularity. The answer is simple. As the relationships are defined, each curve fitted to the data depends only on two arbitrary parameters. This means that on the graph of M and M^* it is possible to arrange for the terminal points of the curves that represent M and M^* to coincide. But when this condition has been fixed there are no longer any degrees of freedom, and there is no a priori reason for the M and M^* curves to remain near to each other, or indeed to be practically identical to each other, as they are for Germany (Chart 3) over the whole range of variation between the two extreme points. The correspondence that is observed is thus a reflection of the nature of things, and it must be concluded that the observed dependence is indeed a real one.[41]

It should further be stressed that the parameters α, m, and χ_0 have exactly the same values for each set of data fitted, and that better results could still be obtained for each country if, as seems reasonable, each parameter were allowed a certain range

$$(\alpha_1, \alpha_2), \qquad (b_1, b_2), \qquad (\chi_{0,1}, \chi_{0,2})$$

of variation about the central values $\alpha=1$, $b=1$, $\chi_0=0.004$. Such a possibility can improve the results, but without greatly affecting their conceptual and economic significance. The values $\alpha=1$, $b=1$, $\chi_0=0.004$ are in any event only orders of magnitude which it seems perfectly reasonable to assume. Thus, in the same way as the pure rate of interest may vary over a certain range, it is reasonable to envisage that the rate χ_0 may fluctuate about a certain average value.[42]

From the discussion above, the value $\chi_0=0.004$ can be validly retained only in cases in which the equilibrium level of the pure rate of interest is in the region of 0.4 per cent per month, i.e., 5 per cent per annum. If this condition is not met, other values of χ_0 should be used. It should be added that the better the statistical quality of the time series used, the better the fit obtained. In fact, the curve fittings that are relatively speaking the least satisfactory concern time series whose statistical quality can be seriously questioned.[43] In any event, the slight

[41] On the testing of the results see [8, p. 143, note 120].

[42] As a matter of fact, if it is assumed that the parameters α, b, and χ_0 can take any value whatsoever for each series studied, better results still can be obtained, with reductions of the values of ϵ^2 and $1-R^2$ of the order of 2 to 1 or more in certain cases. When considering relatively long periods in which structural changes have occurred, the results can be further improved by introducing an exponential trend of the form

$$\phi = \phi_0 e^{pt} \psi(Z).$$

These results will be described in a separate paper.

[43] It may also be remarked that if the three postulates VI, VII and VIII concerning the con-

rences between M and M^* are merely the reflection of the gaps

$$M - M_D = M - M^* \quad)$$

'een actual and desired cash balances. The theory here presented es no implication that the gap is zero; merely that it is small. In the author has shown elsewhere that the difference $M - M_D$ (which be termed the inflationary or deflationary gap, as the case may be) s a decisive role in monetary developments, and that at least as a approximation we can write the relation (1.10).[44] The determina- of the inflationary gap which is rendered possible by the present ry can clearly be of great importance for the implementation of etary policy.

nally, to the extent that the formulation obtained can be considered ., it becomes possible to make meaningful comparisons between tions that are a priori incomparable, since the intensity of a hyper- tionary situation can be measured by the rate Z, which has intrinsic ficance. Thus the Polish hyperinflation of November 1923 may be pared with the German hyperinflation of July 1923, the two cor- nding values of Z being 4.60 and 4.56 respectively.

IV. *The Economic Significance of the Formulation*

'he H.R.L. Formulation

ie aim of the formulation proposed here is to give operational con- to the quantity theory of money, by showing that the velocity of lation of money at any given moment can be very closely described n invariant functional of all the earlier rates of growth x of total y defined by (2.7). It shows that this function can itself be ex- ed very simply as a function of a rate Z, designated as the psy- gical rate of expansion, and defined by the differential equation). The evidence provided by the series studied to date suggests the interdependence of V (or ϕ) and Z appears to be the same at all s and in all places. Thus the formulation proposed in this paper itutes a key to the placing of all the known facts within a single ework.

sired money balances M_D are defined as the product of relative

in §18 were not retained, but the assumption of a common law were maintained never- , the common values of the parameters α, b, and χ_0 would no longer be determinate and then be chosen in such a way as to render the overall fitting of the curves as close as e to the data. In this case, the H.R.L. law ($\alpha = 1$, $b = 1$, $\chi_0 = 0.004$) would no longer be st common law conceivable, although it would doubtless represent a good approxima- the best common law. In other words, from this point of view, excellent as the results to be, they could probably be further improved. The degree of improvement, however, of course be less than the possible improvement, under the assumption that α, b, and χ_0 t necessarily identical for all the series considered (note 42 above).

lais, 1954, A non-linear model of cyclical fluctuations [3, §8, relation (7), p. 186].

desired cash balances ϕ by total outlay. Relative desired money balances are moreover nothing other than the coefficient K of the Cambridge school. The proposed formulation is hereditary in the sense that it establishes a functional dependence between the demand for money as recorded at any given moment and the previously observed values of the rate of growth of total outlay. It is relativistic in the sense that this dependence can be given a simple formulation in terms of a psychological time-scale. Finally, it is logistic, in the sense that it leads to a logistic curve when the expression of relative desired money balances is considered as a function of the psychological conjunctural index.

The eight postulates on which the H.R.L. formulation is based are not indissolubly linked to each other. While the hereditary and relativistic postulates do seem to form a unit, the logistic postulate, despite its appeal, need not necessarily be retained. In the same way, Postulate VIII on psychological temporal symmetry is completely independent of the remaining two postulates VI and VII on the constants α and b.

B. *The Economic Interpretation of the Results*

The results obtained can be interpreted in either of two ways, according to whether the expression for relative desired money balances or the interrelationship of the price level and the quantity of money is being considered. These two interpretations, however, correspond to one, and one only, underlying reality.

Relative desired money balances which differ little from the reciprocal of the velocity of circulation may be taken first. Their level is expressed as a determinate function

$$\phi_D = \phi_0 \psi(Z) \tag{4.1}$$

of an index of psychological expansion, which in its turn is a determinate function of the historical development of total expenditure.[45]

If instead of considering relative desired money balances, attention is focussed on V, the velocity of circulation, then

$$V \approx \frac{1}{\phi_0 \psi(Z)} \quad ^{46} \tag{4.2}$$

Since

$$\begin{aligned} \phi_D &= M_D/D \\ &= M_D/PQ \end{aligned} \tag{4.3}$$

so that

[45] Or, in hyperinflations, of prices.

[46] From (2.4), (3.1), and (3.4).

$$M_D = PQ\Phi_D \tag{4.4}$$

whence it follows that, at any given moment, desired money balances are proportional to the product of the price level and the level of economic activity.

Turning to the link between prices and the quantity of money, expressed by the relationship

$$P \approx \frac{1}{\phi_0\psi(Z)} \frac{M}{Q} \tag{4.5}$$[47]

it can be seen that, at any given time, there is a proportional relationship between the price level and the quantity of money. But the coefficient of proportionality is not constant; its value at each moment depends on the past historical development of total outlay. Thus, the quantity theory of money appears to be fundamentally correct, provided that it is redefined on the basis of the hereditary and relativistic formulation which has been presented above. The velocity of circulation which is inversely proportional to relative desired money balances is not a constant, but appears as an invariant function of past developments.

The link of proportionality at any given time between the quantity of money and the product of the price level and the level of activity (relation 4.5) is a relationship of interdependence, and from the dynamic standpoint of causality can obviously be interpreted in either of two ways. It may mean that the price level is proportional to the ratio of the quantity of money to the level of activity; or it may mean that the quantity of money is proportional to the product of the price level and the level of economic activity. The examination of this double interpretation is beyond the scope of this study. In general, both viewpoints are valid, for in changing economic situations, effects influence causes, and different weights must be given to the one or the other according to circumstances. In some cases, it may be the first interpretation which applies,[48] whereas at other times the second may provide the explanation of what constitutes the motive force.[49]

The theory proposed here makes possible a new, operational reformulation of the quantity theory of money, and enables a synthesis to be made of points of view which hitherto have been considered as completely opposed. The standpoint of the quantity theorist is validated in as much as there is a proportional relation between prices and the quantity of money. But so is the anti-quantity theory view since the coefficient of proportionality is not constant. Clearly, considered in itself the formulation of the demand for money does not and cannot

[47] Which follows from (2.4), (2.13), (2.31), (3.3), and (3.4).

[48] This is so, for example, of inflations reflecting the growth of the means payment (inflation in the 16th century, hyperinflations, etc.).

[49] E.g., wage inflation (as in France in 1936 and 1945, etc.).

constitute a theory of monetary dynamics. It is but one element of a much greater whole;[50] but it is obviously an element of considerable importance. The only aim of the H.R.L. formulation is to specify how, at any given moment, the demand for money and the velocity of circulation are determined.

C. *The H.R.L. Formulation and Previous Researches*

It may be of help to show very briefly the relation of the present H.R.L. formulation to earlier formulations. Fundamentally, "desired money balances" M_D in this paper is none other than Walras' "desired money balances," but the concept of relative desired money balances ϕ_D corresponds to the coefficient K in the Cambridge formulation, and in fact is little different from the reciprocal, $1/V$, of the velocity of circulation, V, in Newcomb and Fisher's formulation.[51] Cagan's 1954 formulation has been compared with Allais' 1954 formulation in the introduction of this paper.

The present Allais formulation (1963) differs from the 1954 Allais formulation in that the two equations (1.1b) and (1.2a) are replaced by the two equations

$$\phi_D = \phi_0 \frac{1 + b}{1 + be^{\alpha Z}} \tag{4.6}$$

$$\frac{dZ}{dt} = x - x_0 \frac{1 + be^{\alpha Z}}{1 + b} Z \tag{4.7}$$

By contrast with all earlier formulations, the theory underlying both Allais' formulations (1954 and 1963) is based on the existence of a nonzero difference $M - M_D$ between the supply of and the demand for money. This difference determines changes in total outlay D by the equation (1.10). The fact that this difference remains small enables empirical study of relative desired money balances to be founded on the assumption that for practical purposes the two quantities are equivalent (condition 3.2).

The essential differences between the H.R.L. formulation of 1963 and Allais' and Cagan's 1954 formulations are: (a) the coefficient χ of forgetfulness is no longer a constant, but a function of the index $\psi(Z)$ which satisfies the condition (2.41); (b) the function ψ, instead of being exponential (Cagan) or linear (Allais 1954) in u, is a logistic function of Z. Further, in the context of analysis of the current situation, there is the essential point that Allais' formulations of 1954 and 1963 differ from Cagan's formulation, in that Allais defines x by (1.3b), whereas in Cagan's formulation it is defined by (1.3a). In fact, defining x by (a)

[50] In my papers of 1953 [2], 1954 [3], and 1955 [5] I have tried to trace the outlines of such a theory; see in particular relation (1.10) above.

[51] See Allais [8, pp. 33–34 and 153–54].

results in a less satisfactory fit of the data than when (b) is used. This is because definition (b) takes account of both changes in the price level and changes in the level of economic activity, and correspondingly provides a better image of the development of the economic situation.[52]

The superiority of the H.R.L. formulation over Cagan's formulation can be assessed by comparing the periods covered, and the values of N and $1-\rho^2$, as between Tables 1 and 2. The main advances represented by the H.R.L. ($\alpha=1$, $b=1$, $\chi_0=0.004$) formulation by comparison with Cagan formulation are a better fitting of short series,[53] the possibility of fitting long series, the applicability of the same formulation to different situations. The H.R.L. common law generally offers a better description of the facts than the earlier Cagan or Allais formulations, though the characteristic parameters in the latter varied from one series to the next[54] and though the number of arbitrary parameters is two instead of three.

According to Friedman's and Cagan's view, the coefficient u (or E)[55] appears as a "coefficient of expectation" and it is quite natural to assume that the expected rate of change du/dt of the rate of expectation u is determined in each period of time in proportion to the difference between the actual rate of change x of total outlay D and the rate of change u that had been expected (differential equation 1.2a). According to Allais' view in 1954, u is related to the past and not to the future. This rate reflects the memory of the past retained by economic agents, and the farther back in the past that events are recalled, the more it fades. When the coefficient of forgetfulness $\bar{\chi}$ is constant the two formulations (1.2) are mathematically the same, but their philosophical starting points are different. Since Cagan's and Allais' 1954 views are merely two different interpretations of the same mathematical formulation there is no point in suggesting that either is superior.

The new H.R.L. formulation which is proposed in this paper differs from Allais' earlier formulation and the Cagan and Friedman formulations in that it takes the psychological time scale into consideration. Here, from the differential equation (2.27) it is no longer possible to interpret the rate of expansion z as being proportional to the difference $x-z$. On the contrary, the new formulation appears to be a quite natural generalization of Allais' earlier conception.[56] To the extent that formulation gives better results, Allais' view appears to correspond better to the real nature of the facts.

[52] For further details on the comparison of the present formulation and those which have been put forward in the past, see [8, Table VI, p. 154].

[53] Allais [8, Tables IX and X, pp. 178–79].

[54] For further details see [8, Appendix I].

[55] See Introduction and footnote 2.

[56] Since from postulate of II.c., relation (1.2) holds when referred to a psychological time scale II.c., relation (2.10).

D. *The Proposed Formulation*

The results show that human societies placed in very different circumstances, ranging from normal situations to hyperinflations, in capitalistic or communistic regimes, today or a half-century ago, react according to one and the same law, one which moreover might validly have been deduced from a priori reasoning. This similarity of behavior should be interpreted as corresponding to the invariance of human psychology in space and over time, at least in its collective aspect.

In fact, the proposed formulation can be applied in numerous other fields. The index of psychological expansion, which can easily be calculated at any time, provides a quantitative measure of the economy's appreciation of the conjunctural situation.[57] It is therefore very probable, if not certain, that the introduction of this index into economic forecasting and analytical calculations would greatly improve both the analysis of past developments and the methods employed for making projections.

The results obtained in this study of monetary phenomena are no more than an illustration of the importance of hereditary and relativistic effects in the behavioral sciences. There is no doubt that these phenomena are merely one particular example of many cases in which the methods and principles would be valid over the whole range of social phenomena to the extent that these phenomena are linked by relationships of a hereditary type, and to the extent that time scales are subject to relativistic effects.[58]

The proposed formulation may at first sight appear to be complex. But it is verified by the available empirical data, at least in the cases studied up to now. So long as fresh data do not invalidate it, it must be considered as corresponding to the nature of things. Whether the hereditary, relativistic, and logistic formulation ($\alpha=1$, $b=1$, $X_0=0.004$) is confirmed or invalidated as a general law by the analysis of further numerical series,[59] the numerical regularities which have been brought out will stand. As with other theories, so with this one. Its logical formulation may be maintained or modified, but the real relationships between observable quantities which it has brought out will have been established once and for all, and this is certainly what must be considered as the essential contribution of this study.

[57] In French "Situation conjoncturelle." See footnote 22 above.

[58] From relations (2.2), (2.26), and (2.29) the function $\psi(Z)$ represents the value of the unit of psychological time for society as a whole as a function of the intrinsic rate of expansion Z.

[59] The author of this paper would be grateful to receive time series (M_n, R_n) or (M_n, P_n) relating to any country and covering any period for purposes of analysis. Monthly values are preferable, if they can be supplied.

REFERENCES

1. M. F. C. ALLAIS, *Economy and Interest* (Economie et intérèt). Librairie de Medicis (in deposit). Paris 1947.
2. ———, *Illustration of the Theory of Economic Cycles by Non-linear Models* (Illustration de la théorie des cycles économiques par des modèles non-linéaires). Paper submitted to the European Congress of the Econometric Society, Innsbruck, August 31, 1953.
3. ———, Explanation of Economic Cycles by a Non-linear Monetary Model with Lagged Relations (Explication des cycles économiques par un modèle non-linéaire a régulation retardée). Paper submitted to the European Congress of the Econometric Society, Uppsala, August 4 1954. *Metroeconomica*, Apr. 1956, *8*, 4–83.
4. ———, *The Accounting Bases of Macroeconomics* (Les fondements comptables de la macroéconomique). Paris 1954.
5. ———, *Explanation of Economic Cycles by a Non-linear Monetary Model with Lagged Relations* (Explication des cycles économiques par un modèle non-linéaire a régulation retardée). Paper submitted to the International Colloquium on Dynamics, Paris, May 1955. Published in *Dynamic Models in Econometrics* (Les modèles dynamiques en econométrie). National Center of Scientific Research (C.N.R.S.) Collection of Proceedings of International Colloquia Vol. 42, Paris 1956, pp. 169–308.
4. ———, *Hereditary Effects and the Relativity of Time in the Social Sciences—An Application to the Theory of Monetary Phenomena* (Des effets héréditaires et de la relativité du temps dans les sciences sociales —Application à la théorie des phénomènes monétaires). Paper submitted to the Centre National de la Recherche Scientifique, December 31, 1963.
7. ———, *The Hereditary, Relativistic and Logistic Formulation of the Demand of Money.* Lecture delivered at the University of Chicago, May 11, 1964.
8. ———, *A Restatement of the Quantity Theory of Money* (Reformulation de la théorie quantitative de la monnaie), *Bull. SEDEIS*, Sept. 1965, No. 928, Supp.
9. ———, *A Restatement of the Quantity Theory of Money* (Reformulation de la théorie quantitative de la monnaie), in preparation for publication by Hermann, Paris. (This volume will present the full framework of the study dealt with in the present paper, which has been limited to giving a summary of the results obtained.)
10. P. D. CAGAN, "The Monetary Dynamics of Hyperinflation," in M. Friedman, ed., *Studies in the Quantity of Money*, Chicago 1956, pp. 25–117. (The essential results had been given earlier in Cagan's doctoral dissertation, 1954.)
11. MILTON FRIEDMAN, "The Quantity Theory of Money—A Restatement," in *Studies in the Quantity Theory of Money*, Chicago 1956.
12. ———, *A Theory of the Consumption Function.* Princeton 1957.

THE PROPENSITY TO CONSUME PERMANENT INCOME

By THOMAS MAYER*

This paper compares the average propensity to consume at different levels of permanent income, thus testing a hypothesis of a modified permanent income theory. The modification is the treatment of consumer durable purchases as consumption—a modification defended below. The test deals only with the hypothesis that the average propensity to consume is independent of the level of permanent income; it says nothing about the other main component of the permanent income theory, the lag hypothesis. I will use the term "permanent income theory" in the strict sense, i.e., as *inter alia* an assertion that the marginal propensity to consume is independent of the level of permanent income. In addition to testing this strict theory, I am also testing the looser version of the theory, namely, that the marginal propensity to consume is greater for permanent than for measured income, though even for permanent income, the marginal propensity to consume is less than the average propensity. There is considerable evidence both for and against the strict permanent income theory, which suggests that the truth may lie somewhere in-between, i.e., that the looser version of the theory is correct. The test presented in this paper goes beyond such a vague supposition and actually measures the *extent* to which the modified permanent income theory is correct.

The main problem in testing the permanent income theory is to obtain an empirically measurable proxy for permanent income. I have used the mean income of broadly defined occupational groups. There are two ways in which the mean income of an occupational group may fail to be an adequate measure of the mean permanent incomes of its members. First, households may change their occupations and, second, the mean income of occupations may fluctuate over time. I have guard-

* The author is professor of economics, University of California, Davis. He is indebted for able research assistance to Gary Walton, Alan Cohn, Anthony Mutsear, Allen Shapiro, Richard Piper, James Brundy, and to a whole army of translators. He is indebted for programming advice to his colleague Hiromitsu Kaneda, and for helpful comments to Mrs. Margaret Gordon, Albert Fishlow, and W. P. Strassmann, as well as to members of the Continuing Seminar in Money and Banking of the University of Califoria, Davis, and Professor Brunner's seminar at UCLA where earlier versions of this paper were read. He is indebted for financial support to the University of California, Davis, for a faculty research grant, to the Institute of Industrial Relations of the University of California, and to the Davis and Berkeley computer centers of the University of California. Finally, he is indebted for providing unpublished data to the Institute of Economics and Statistics of Oxford University and the Survey Research Center of the University of Michigan.

ed against the first of these dangers by using broad occupational classes such as manual workers vs. white collar workers. Movement between such broad classes is quite infrequent,[1] particularly in pre-World War II Europe, the source of some of the data. I have guarded against the second danger by using thirty-four budget studies spread over the period 1912-1961 covering a dozen countries. Given such a large sample, differences in the receipt of transitory income among occupations should cancel out, so that differences in the mean income of occupations reflects *on the average* (though not necessarily in any single case) differences in permanent income. Using the income of occupations as proxies for permanent income is not a new idea; in fact, this has been done by Modigliani and Ando [23] on one side of the debate and by Friend and Kravis [9] on the other.[2] However, both of these tests relied on a single budget study, the 1950 Bureau of Labor Statistics Survey; but for any single year there is no justification for assuming that transitory elements average out within each occupation. It is quite possible that certain occupations had positive, and others negative, transitory income in that year. Moreover, the validity of the 1950 data is doubtful [10] [23]. In addition, neither the Modigliani-Ando study nor the Friend-Kravis study tested the looser version of the theory.[3]

I. *Method*

One advantage of an occupational test is that it does not require any elaborate and essentially untested methods—a simple comparison of average propensities to consume of different occupations is all that is required to see if the strict theory is valid. And an only slightly more complex procedure allows one to determine the extent to which the looser version is valid. This procedure consists of computing the marginal propensity to consume twice, once from households grouped by occupation, and once from the *same* households grouped by income class (i.e., by measured income). If the permanent income theory is entirely correct, then the marginal propensity to consume, when computed from the occupational grouping, should on the average equal the average propensity to consume. On the other hand, assume that the

[1] A large-scale study [19, p. 161] of occupational mobility undertaken in Oakland, California, in 1949 found considerable stability of broad occupational affiliation in spite of the preceding war and depression years. Similarly, a Swedish study for the periods 1930-36 and 1940-45 shows relatively little mobility [2].

[2] Friedman, too, uses an occupational test [8, pp. 69-79], but one that differs from the test used here. As is shown below, this test is based on inappropriate data. Another occupational test which has some similarity with the one present here was undertaken by Houthakker [12] and led to a debate with Eisner and Friedman [5, 7, 13].

[3] Both the tests reject the permanent income theory, though some of the other tests used by Modigliani and Ando do not.

permanent income theory is completely wrong, and that it makes no difference whether one uses permanent income or measured income in estimating the consumption function. If so, the marginal propensity to consume should, on the average, be the same when households are classified by measured and by permanent income.

These two alternatives, as well as the intermediate cases, can be exhibited by the statistic $c_o - a/c_v - a$ where c_o is the marginal propensity to consume computed from an occupational grouping of the households, c_v is the marginal propensity measured from an income class grouping of the households, and a is the average propensity to consume. The denominator measures the gap between the marginal and average propensities to consume when this gap is calculated conventionally, i.e., when measured income is used. The numerator, on the other hand, measures the gap between the marginal and average propensities which remains even if one uses permanent income instead of measured income. The expression, therefore, shows the remaining gap as a proportion of the conventionally computed one. Thus, it has the convenient property that if the modified permanent income theory is completely right, the numerator, and hence, the whole expression, is zero, while if the theory is completely wrong, its value is unity. And similarly, intermediate cases give a value between zero and unity. For example, if the average propensity to consume is 0.8 and the marginal propensity to consume is 0.7 for permanent income and 0.6 for measured income, the coefficient has a value of 0.5.

I have called this expression the prediction coefficient because it measures the extent to which the conventional theories (the name I will use hereafter for the absolute and relative income theories) predict correctly. One can look upon the permanent income theory as a naive theory which asserts that the percentage of income consumed is independent of the level of permanent income, while the conventional theories represent attempts to go beyond such a naive model, and to predict the percentage of income consumed from a knowledge of the level of income. Traditionally, computations of the average and marginal propensities to consume based on *measured* incomes have suggested that the conventional theories are superior to a naive model; but they involve a possible bias, which according to the permanent income theory accounts fully for their apparent superiority. The prediction coefficient is a device which measures the extent to which the superiority of the prediction survives the elimination of this bias.

In computing the marginal propensity to consume, I have used quite simple consumption functions, primarily because the data were insufficient to fit more complex ones. Most budget studies do not give information about the many variables usually included in elaborate

multivariate functions. Moreover, even when such information is available (as in the *Survey of Consumer Finances*) there are too few occupational groups (and hence, too few degrees of freedom) to fit elaborate functions. Fortunately, the use of a simple consumption function does not do violence to the theories being tested.[4]

However, I have, when feasible, included household size as an additional independent variable. The number of persons per household is clearly one of the important determinants of consumption. Moreover, this variable becomes particularly significant when comparing the marginal propensity to consume of households as computed from an occupational grouping with the same propensity computed from an income class grouping. When households are grouped by measured income, there is a positive relation between income level and family size. But if households are grouped by occupation, there exists, in the data used here, no such positive correlation. Hence, a bias may be created if family size is ignored.

I computed the marginal propensity to consume in two different ways. In those cases where I had three or more occupations I used regression analysis. But in those cases where I had only two occupations I computed the marginal propensity to consume permanent income by subtracting the mean consumption of one occupation from that of the other and dividing by the difference in mean income. Correspondingly, in these cases I calculated the marginal propensity to consume measured income by dividing the households in each occupation at the median income of that occupation and then dividing the difference in consumption of households above the below and median by the difference in mean income.[5] For reasons discussed below and in a duplicated appendix [21] available upon request, I have treated the individual budget studies as separate units.

[4] Friedman himself, though not Modigliani, uses a simple function, explicitly rejecting the more ponderous consumption functions fitted by others. And the same holds true on the other side of the debate. In the relative income theory Duesenberry uses as independent variables only income and mean income of the community (or else previous peak income), and since the last two variables are not relevant for an occupational test, a simple consumption function is a fair test. Similarly, the absolute income theory, while it may use several independent variables, is basically an assertion that the higher the level of income, the lower the percentage consumed even if other variables are left out of consideration.

[5] A number of budget studies did not cover the two occupations in proportion to their frequency in the universe. In these cases I averaged the marginal propensities to consume of the two occupations by giving the marginal propensity to consume of manual workers twice the weight of the marginal propensity to consume of clerical workers. In the countries covered by these budget studies nonagricultural manual workers were somewhat more than twice as numerous as clerical workers [35], but since their average income was lower, a two to one ratio gives an adequate approximation to the relative importance of these two groups as income receivers.

II. *Measurement of Permanent Income and Consumption*

The use of mean measured income of an occupation as a proxy for the permanent income of its members requires several safeguards. One, already mentioned, is to use many budget studies so that differences in transitory income cancel out. Another safeguard is to decide which is the higher-paid and which is the lower-paid occupation, not on the basis of mean measured income in the particular budget study, but on an a priori basis, i.e., to make this decision on the basis of one's knowledge about the relative income of occupations before even looking at measured incomes in the particular budget study.[6] To facilitate this decision I have used broad occupational groupings. While it may be difficult to decide a priori whether, say, metal workers have a higher income than wood workers, it is easy to decide that skilled workers have a higher income than unskilled ones. Still another safeguard is to exclude certain occupational classifications such as "unemployed" or "retired," which are clearly dominated by transient households.

Turning to consumption, the main definitional problem is the treatment of consumer durables. I have treated the purchase of consumer durables as consumption, a choice largely dictated by the data, and hence I am testing a *modified* version of the permanent income theory. This modification can be defended on two grounds. First, most of the data cited by Friedman and Modigliani, as well as others, in support of the permanent income theory include the purchase of consumer durables in consumption. If such data are allowed to confirm the theory, they must be allowed to reject it too. Second, in a more positive vein, for the great preponderence of macro-economic problems, the relevant definition of consumption is one which *includes* consumer durable purchases. Thus, in Friedman's discussion of the policy implications of the permanent income theory, the problems he lists are better served by a definition of consumption which includes durable purchases [8 pp. 235-38]. Hence, whether or not my data constitute a fair test of the permanent income theory, they do deal with an important policy parameter, the marginal propensity to consume as conventionally defined.

Another problem is the treatment of insurance. When insurance is stated separately, I have included life insurance (and burial insurance) premiums in saving for reasons discussed in [21]. In some studies all insurance premiums were given as a single item. In these

[6] In only a few cases did I look at the relative incomes of the occupations before making the decision, and even then I did not attach much importance to the ranking in the particular study. Only those budget studies in which the actual income ranking did correspond to a priori expectations were used.

cases I have computed the coefficients twice, once including insurance premiums in saving and once excluding them. Some other measurement problems are discussed in [21].

III. *The Sample*

I have attempted to include with exceptions described in [21] all budget studies since 1900 that provide useable data.[7] Since only a relatively small proportion of the numerous budget studies undertaken in this period give data on occupations, this turned out to be a feasible task. The use of early budget studies may need justification since we are primarily concerned with the present. Such a justification is given in [21].

I have not used all occupations given within each study, but limited the analysis to those occupations for which one would expect a clear-cut income difference. Thus, the most common distinction used is that between manual workers, on the one hand, and white collar employees or officials on the other. One important exclusion is self-employed households. In addition to the fact that the data for such households are particularly unreliable, they must be excluded because both the permanent income theory and the more conventional theories assert—though for different reasons— that entrepreneurial households save more than nonentrepreneurial households. Hence, a comparison of the propensities to consume of entrepreneurial and other households can neither support nor reject the permanent income theory. Finally, I have, in all but one case, excluded farm worker households and armed forces households because of the problems created by their receipt of nonmoney income.

IV. *The Results*

Table 1 shows the budget studies used, the coefficient of ranked correlation between the mean income of the occupation and the average propensity to consume, and for those cases where it could be com-

[7] The main exclusions are budget studies (1) for underdeveloped countries and iron curtain countries, (2) for the immediate post-World War II years, (3) covering less than a full year with one exception, (4) showing a rising marginal propensity to consume measured income, (5) based on an inadequate number of cases or available only in inadequate secondary sources. In addition, to keep the sample balanced, I did not—with one exception discussed in [21]—use budget studies for more than three consecutive years for the same country.

One obvious omission is the large scale 1935-36 *Survey of Consumer Purchases* study used by Friedman in his occupational test [8]. The reason for its omission is that it excluded from its tabulation of income and consumption by occupation all households with incomes outside the normal range for their occupation. Since the proportion of transitory income excluded in this way is likely to vary for each occupation, any test of the permanent income theory based on these data is inadequate.

In locating the earlier studies I was aided by a magnificent annotated bibliography [25].

TABLE 1—RANKED CORRELATION COEFFICIENTS AND PREDICTION COEFFICIENTS

Locality	Date of Study[a]	RHO[b]	Prediction Coefficient	Occupations Covered and Number of Households[c]	Method of Computing Prediction Coefficient[d]	Source
Switzerland	1912	−1.0[e] −0.8[f]	0.8[e] 0.1[f]	$U=26$, $SK=515$, $E=201$[g], $=49$	R	[52]
Zurich	1919	+1.0	—	[h]	—	[63]
Finland	1920–1	−1.0	—	$W=437$, $E=117$	—	[34]
Sweden	1923	−1.0	0.4	$W=747$, $E=445$[i], $MC=208$	R	[48]
Amsterdam	1923–4	−1.0	1.0[ej] 0.9[fj]	$WC=81$, $MC=64$	M	[28]

[a] In some cases no date was given for the study, and it had to be surmised from the date of publication.

[b] Coefficient of ranked correlation between mean income of occupation and its average propensity to consume. Coefficient is *not* weighted by number of households in occupation.

[c] Occupational code used is:

- *C*—clerical workers
- *E*—white collar employees
- *M*—managerial
- *MC*—middle class
- *O*—officials
- *P*—professionals
- *S*—sales
- *SK*—skilled manual workers
- *SE*—semiskilled manual workers
- *U*—unskilled manual workers
- *W*—manual workers
- *WC*—working class

[d] R denotes multiple regression, using income and household size as independent variables. Household size is measured sometimes by number of persons and sometimes by number of consumption units. M denotes that income classes were split at median and that no adjustment for household size was made.

[e] Insurance premiums treated as saving.

[f] Insurance premiums treated as consumption.

[g] Middle rank officials and teachers only.

[h] I regrouped data into two classes: (a) teachers, middle rank government officials and government white collar employees, and officials of private enterprises (44 households); (b) lower ranking white collar employees of private enterprise and manual workers (41 households).

[i] Lower ranking white collar employees only.

[j] The data understate saving and income, and since this understatement is greater for the lower income group than for the upper income group, there is a bias in favor of the permanent income theory.

TABLE 1—(*Continued*)

Locality	Date of Study[a]	RHO[b]	Prediction Coefficient	Occupations Covered and Number of Households[c]	Method of Computing Prediction Coefficient[d]	Source
Czechoslovakia	1925–6	+1.0	−3.1[k] −0.7[l]	$W=51$, $E=115$	M	[30]
Minnesota	1926–7	−0.7	—	$U=41$, $SE=46$, $SK=41$, $C+M=78$, $P=44$	—	[27]
Denmark	1931	−1.0	6.8	$W=140$, $O=31$	M	[31]
Sweden	1933	−1.0	0.9	$W=526$ $MC=195$, $E^{i}=524$,	R	[49]
Netherlands	1935–6	−1.0	1.0	$W=278$, $E=206$	M	[38]
Switzerland	1936–7	−1.0	0.6	$W=741$, $E+O=713$	M	[50]
Switzerland	1937–8	−1.0	0.8	$W=321$, $E+O=269$	M	[50]
United States	1948	−0.4	0.6	$P=216$, $C=275$, $SK+SE=758$, $U=303$[m]	—	[60]
United States	1949	−0.4	—	—[n]	—	[56, 57]
Switzerland	1949	−1.0	1.6	$W=274$, $E=244$	M	[53]
Urban United States	1950	−0.7	0.5	—[o]	R	[62]
Switzerland	1951	−1.0	0.6	$W=160$, $E=102$	M	[54]
Netherlands	1951	−1.0[e] +1.0[f]	−0.6[e] −0.1[f]	$W=970$, $E=1167$	M	[39]
Great Britain	1951–2	−1.0	—	$C+S=330$, $SK=807$, $U=388$, $M^{p}=259$	—	[20]
Switzerland	1952	−1.0	1.2	$W=151$, $E=100$	M	[55]

[k] Health insurance as well as life insurance premiums included in saving.

[l] Health insurance and life insurance premiums included in consumption.

[m] Clerical occupation includes other (i.e., nonprofessional) white collar employees, and unskilled occupation includes service workers.

[n] The actual number of spending units in each occupation is not given, and due to oversampling of the high income groups, it differs somewhat from the proportion of the total population in each occupational group. The proportion of the total population in each of the groups covered is: professional and semiprofessional, 7 per cent; clerical and sales, 13 per cent; skilled and semiskilled, 27 per cent; unskilled and service, 12 per cent. The total number of households covered is slightly over 3,000.

[o] The exact number of cases in each occupation is not given. The total sample covered *more than* 12,000 households, distributed as follows: salaried professionals and officials, 13.6 per cent; clerical and sales workers, 13.1 per cent; skilled wage earners, 17.8 per cent; semiskilled wage earners, 17.1 per cent; unskilled wage earners, 14.9 per cent. (The rest of the sample consists of self-employed and not gainfully employed households.)

[p] Includes technical employees.

TABLE 1—(Continued)

Locality	Date of Study[a]	RHO[b]		Prediction Coefficient		Occupations Covered and Number of Households[c]	Method of Computing Prediction Coefficient[d]	Source
Great Britain	1952-3	−0.4		—		$C+S=292$, $SK=730$, $U=358$, $M^p=154$[q]	—	[16]
Coimbra (Portugal)	1953-4	−1.0		0.4		$W=1183$, $O=857$	M	[42]
Great Britain	1953-4	−0.8		0.6		$C+S=336$, $SK=809$, $U=248$, $M=417$	R	[41]
Denmark	1955	−0.8[j]		—		—[r]	—	[32, 33]
Sweden	1955	−1.0		—		$W=526$, $E=236$	—	[45]
Great Britain	1955-6	+0.5[s]	−1.0[s]	—		—[t]	—	[17]
Evora & Viseau (Portugal)	1955-6	−1.0		2.2[e]	1.7[f]	$W=87$, $O+E=88$	M	[43, 44]
Sweden	1957	−0.9[u]	−0.7[u]	—		—[v]	—	[46]
Israel	1957-8	−0.6		—		$M+P=270$, $C=599$, $SK=707$, $U=534$	—	[36]
Norway	1958	−1.0[w]		—		$W=2401$, $O=1035$	—	[40]
United States	1958-9	−1.0		—		$W=1068$, $E=2127$	—	[3]
Japan	1960	−1.0		0.2		$W=1095$, $E=1501$	M	[26, 37]
Urban United States	1960	−0.9		0.6		—[x]	—	[59]
Urban United States	1961	−1.0				—[x]		[59]

[q] Number of cases in each group is approximate only.

[r] Five hundred forty-eight higher officials and employees, 1,132 lower officials and employees, 515 skilled workers, 745 unskilled workers, and 160 agricultural workers.

[s] Rho is positive if company directors are included in the sample, and negative if they are excluded. The study states that the saving of company directors is understated [17]. Moreover, even if one includes company directors, Rho is −1.0 if receipts from inheritances are included in income.

[t] Thirty-two company directors, 77 doctors, and 45 dentists.

[u] Since the relative income position of two of the occupations covered, officials and lower business managers, is unclear, I computed Rho twice—once excluding lower officials and once excluding lower business managers. Rho is −0.9 in the former case and −0.7 in the latter case.

[v] Twenty-three higher business managers, 34 lower business managers, 70 academically trained employees exclusive of farmers and business managers, 47 higher officials, 185 lower officials, 22 foremen, and 499 workers.

[w] The marginal propensities to consume of the two occupations are very similar so that it is possible, though perhaps not very likely, that the difference between them is merely the result of differences in household size.

[x] Occupations used are the same as for 1950. The number of cases in each occupation in the sample is not given. The total sample (including the self-employed and unemployed) amounted to 4,463 households in 1960 and 4,879 households in 1961.

puted, the prediction coefficients. A brief description of the data, of the problems encountered, and the approximations used, is given in [21].

The results shown in Table 1 strongly reject the modified permanent income theory.[8] The signs of the ranked correlation coefficients show that in only 2 of the 31 unequivocal cases (Zurich 1919 and Czechoslovakia 1925-26) does the higher-paid occupation consume a larger percentage of its income than the lower-paid occupation. And in the former case the result can be explained by the fact that the upper occupations consisted largely of government employees. Since there had been a substantial inflation in previous years [29] the measured real income of this group was presumably below its real permanent income. In 3 additional cases (Netherlands 1951, Great Britain 1955-56, and Norway 1958) there is considerable doubt about the results. But even if one gives these 3 doubtful cases to the permanent income theory, there are only 5 cases out of 34 in which the conventional theories predict incorrectly. The probability of getting that many "successes" merely by "chance" is less than 1 per cent.[9]

It is worthwhile to look not only at all cases jointly, but at their components. First, one may look at prewar and postwar studies separately. There are twelve prewar studies, and in all but two of them the

[8] The reason why the ranked correlation coefficient is so frequently (minus) one, is that in many studies there were only two usable occupational groups, so that the coefficient has to be (plus, or else, minus) unity.

[9] The significance test used throughout this paper is a simple binomial test. I have treated each individual study as a unit and not applied significance tests to the difference of the two marginal propensities to consume with each study. Significance tests applied to any single budget study taken in isolation would understate the import of the results. Assume, for example, that in each of, say, six budget studies, the difference between the marginal propensities to consume computed from occupational and from income class groupings is significant only at the 20 per cent level. If the differences are all in the same direction, then the differences for all studies taken together are clearly significant, in spite of the fact that none of the individual studies has significant results at a respectable significance level. Moreover, within a single study, a significance test would have little meaning. One of the disturbances, perhaps the main one, is the existence of positive (or negative) transitory income. There is no reason at all for assuming that *within* any one study transitory income is randomly distributed.

These problems disappear if one takes as the unit of observation, not the individual household, but the whole budget study, treating the propensity to consume of an occupation or income class as though it were the propensity to consume of a single representative household in this group. If one assumes that budget studies occurred in years which were chosen randomly with respect to the distribution of transitory income, one can then apply significance tests to the results. By calling each case where the average propensity to consume of the lower-paid occupation exceeds the propensity of the higher-paid occupation a "success" for the conventional theories, one can see if the resulting number of successes could be due to chance. And similarly, by seeing if the marginal propensity to consume calculated from data grouped by occupations exceeds this propensity as computed from income groupings, one can find the number of successes for a theory which asserts that the marginal propensity to consume is greater for permanent than for measured income. Sucesses on both tests then support a position intermediate between the permanent income and conventional theories.

conventional theories predict correctly. This is significant at the 3 per cent level. For the postwar period there are 22 studies and, even if one gives the 3 doubtful cases to the permanent income theory, the conventional theories predict correctly in 19 of these 22 cases, a result significant at the 1 per cent level. The fact that the results are significant for each period taken separately is useful because it shows that the outcome is really based on a long period of time and cannot be explained away by the higher-paid occupation having more positive transitory income for a brief span of years.

Another way of separating the data is to select the most reliable ones. There are 19 studies which have a sample of more than 500 households and seem reliable and relevant in every way.[10] These 19 cases do not include the 2 in which the average propensity to consume is clearly less for the lower income group than for the upper income group, though they do include the 3 doubtful cases. But, even if one gives these 3 doubtful cases to the permanent income theory, the conventional theories predict correctly at the 1 per cent significance level.

But the "success" of the conventional theories is a severely qualified one, since it appears that the marginal propensity to consume *permanent* income is greater than the marginal propensity to consume *measured* income. As previously pointed out, in this case the prediction coefficient is less than unity. And this is true for the great majority of the coefficients shown in Table 1. If the doubtful insurance premium in the Amsterdam 1923-24 case is treated as saving, then, of the 20 prediction coefficients, 14 are below unity, 2 are equal to unity, and only 4 are above unity. Treating the doubtful insurance premiums as consumption yields 15 coefficients below unity, 1 equal to unity, and 4 above unity.

To apply a significance test to these outcomes, something must be done about the coefficients of unity—where the coin, so to speak, falls neither heads nor tails, but stands on its edge. If, instead of rounding the prediction coefficients to one place of decimals, one expresses them to two places, one coefficient (Netherlands 1935-36) drops below unity. The other coefficient remains at unity, but fortunately it relates only to the case where the doubtful insurance premium is treated as saving. If one averages the two coefficients, one treating the insurance premium as saving and one treating it as consumption, the resulting coefficient is again below unity.[11]

Making these adjustments, of the 20 coefficients, 16 are below unity.

[10] I excluded the Japanese study from this set because it covers less than a full year.

[11] The coefficient will be less than unity even if one gives most of the weight to the coefficient of unity. It is unity to two places of decimals whereas the other coefficient to two places of decimals is 0.86.

TABLE 2—MEDIANS AND MEANS OF PREDICTION COEFFICIENTS

	Median	Positional Mean Mid 50 Per Cent	Mean	Number of Studies
All studies	0.6	0.7[a] 0.6[b]	0.9	20
Prewar studies	0.8	0.8[a] 0.7[b]	1.0[a] 1.2[b]	9
Postwar studies	0.6	0.6	0.7	11
Coefficients computed by regression	0.6	0.6	0.6[a] 0.5[b]	7
Coefficients not computed by regression	0.8	0.8	1.0[a] 1.2[b]	13
Selected studies[c]	0.6	0.6	0.8	8

[a] Insurance premiums which could not be broken down by type allocated wholly to saving.
[b] Insurance premiums which could not be broken down by type allocated wholly to consumption.
[c] Sweden 1923 and 1933, Switzerland 1936–37, 1937–38 and 1949, United States 1948, Great Britain 1953–54, U.S. 1960–61.
Source: Table 1.

and the hypothesis that the marginal propensities to consume are the same for permanent and measured income can be rejected at the 1 per cent significance level. Alternatively, if one simply excludes the studies with a prediction coefficient of unity, the results are significant at the 3 per cent level.

Thus the data reject both the modified permanent income theory and the conventional theories and show that the truth lies in between. The prediction coefficients allow one to see where it lies between the two theories. Table 2 shows various averages for the coefficients. The medians and positional means shown in this Table are more meaningful than are the arithmetic means, because the arithmetic means are too heavily influenced by a few extremely large cases. These extreme cases are much less meaningful than the other cases. The prediction coefficient is defined as the percentage of the gap between the average propensity to consume and the marginal propensity to consume measured income which disappears if one uses permanent income in place of measured income. But no such meaning is attached to the prediction coefficient in those cases where the marginal propensity to consume measured income appears to exceed the marginal propensity to consume permanent income, so that the prediction coefficient exceeds unity.[12] In these cases the prediction coefficient is merely a statistical artifact without a useful economic interpretation.

[12] These cases probably result either from errors in the basic data, sampling fluctuations, or, perhaps more likely, from differences in transitory income receipts by various occupations. While these transitory elements should cancel out for all studies jointly, one would expect them to dominate the result in a few cases. Such random factors which raise the prediction

The medians and positional means of Table 2 show that, in predicting the proportion of income consumed, the conventional theories are closer to the truth than is the modified permanent income theory. The medians and positional means suggest that, if one uses permanent income in place of measured income, only about one-third of the difference between the average propensity and marginal propensity to consume disappears.

Table 2 also shows various breakdowns of the overall results. There is no great difference between the results for postwar and prewar studies, and for coefficients estimated by regression and those estimated by comparing only two occupations. This close agreement between the results obtained by these two methods is useful since it suggests that the results cannot be attributed to a bias in the method. Table 2 also shows that the results for selected studies (i.e., the most applicable and reliable ones) are not very different from those for all studies together, thus suggesting that the results are not merely the consequence of errors in the data.

V. *Other Variables*

The negative relation between permanent income and the per cent of income consumed cannot be attributed to chance elements. But this does not necessarily imply that the lower average propensity to consume of the better-paid occupations is due to their higher income. Let us, therefore, look at the variables other than income which are thought to be important for the consumption function and see if they can account for the above results.

In his well-known text, Gardner Ackley [1, Ch. 12] mentions the following variables: the interest rate, consumer credit terms, sales effort, relative prices, deferred demand, the stocks of consumer durables, price and income expectations, income distribution, demographic factors, wealth, liquid assets, the money stock, capital gains, and subjective and cultural factors. To this list the following three variables should be added: frequency of home-ownership, contractual saving commitments, and pension arrangements. Friedman suggests that the main variables determining the average propensity to consume permanent income are: "the rate of interest, the relative dispersion of transitory components of income and of consumption, the ratio of wealth to

coefficient would tend *not* to be fully offset by random factors in the other direction, since outside the zero to unity range the prediction coefficients are not symmetrical. The prediction coefficient will be negative only if the marginal propensity to consume as calculated from occupational income is negative. To be symmetrical, it should be greater than unity only if the marginal propensity to consume *measured* income is negative—but as pointed out above, I have discarded all such cases. Instead, the prediction coefficient exceeds unity if the marginal propensity to consume is greater for measured than for permanent income.

income, and the age and composition of consumer units" [8, p. 232]. In [21] I have examined each of these variables to see if they could account for the above results. Here I will just summarize the results.

The first nine of these variables can be dismissed quickly since they are either not relevant for the cross-section test used here, or else because they would tend, if anything, to increase the average propensity to consume of the higher occupations relative to the average propensity to consume of the lower occupations.[13] Hence, although some of these factors may perhaps be invoked to explain why the data reject the conventional theories (or why the prediction coefficients have a downward bias), they cannot be used to explain why the data reject the modified permanent income theory.

One demographic variable, household size, has been taken into account in some of the studies. Another one, age, shows little variation among occupations, at least in the United States and Great Britain [18, p. 81] [21, Tables 3 and 4]. Unfortunately, very little information on wealth holdings is available. But some data for Denmark, Britain, and Sweden [32, pp. 24, 63] [45, pp. 71, 179] [21, Table 4] show that the ratio of net worth to income is positively correlated with the mean income of occupations.[14]

Incidentally, it is worth noting that the fact that the higher occupations have a greater ratio of wealth to income than the lower occupations is consistent with the hypothesis that they save a larger proportion of their income. And while this fact is not *necessarily* inconsistent with the modified permanent income theory, it can be made consistent with it only by some rather tenuous reasoning.[15]

[13] For simplicity I will use the terms "higher" and "lower" occupations instead of "higher-paid" and "lower-paid". The three variables which may increase the propensity to consume of the upper occupations relative to that of the lower occupations are: the rate of interest paid on borrowing, consumer credit terms, and sales effort. (This makes the conventional assumption that the interest elasticity of saving is positive.)

[14] Unfortunately, the direction of the wealth effect on consumption is not quite certain. The predominant view is that the higher the level of wealth, the lower the saving-income ratio, because the higher the stock of wealth, the lower is the marginal rate of substitution of current consumption for wealth, i.e., for future consumption [1, pp. 269-73] and [24]. But there is a minority view which asserts that an increase in wealth *may* stimulate saving by raising the household's level of aspirations. (See G. Katona [14, pp. 131-37] and L. Klein [15, p. 99].) And there is one fact which appears to support this view: households covered by private pension funds save (apart from the pension system) a higher proportion of their income than households without pensions [4]. If the predominant view is correct, then the fact that I have omitted the wealth variable creates a bias in favor of the modified permanent income theory; while if the other view is correct, there is a bias against the theory.

If the correlation between wealth and mean occupational income is the same as the correlation between wealth and measured income, then the prediction coefficient is unaffected by the omission of wealth, but in other cases, the prediction coefficient may be biased.

[15] The only explanation that seems consistent with the permanent income theory is that there is a positive correlation between windfall receipts by one generation and high occupa-

The next variable, liquid asset holdings, is, at least in the postwar United States and Great Britain, positively correlated with occupational income [20, pp. 54 and 81] [21, Table 4]. Hence if this variable has an effect on the propensity to consume (a proposition frequently denied it) it biases the results in favor of the modified permanent income theory.[16]

Next there is homeownership. Unfortunately, the data on the relative frequency of homeownership show an unclear picture. For the postwar period there is no pronounced relationship between occupational status and the frequency of homeownership in the United States in one set of data, but there is such a positive relationship in another set of data [21, Tables 3 and 7]. In Great Britain and Switzerland these two variables are positively correlated, but in Sweden the correlation is negative [20, p. 99] [51, Vol I, p. 269, Vol. II, p. 635] [47, p. 193].

But, in any case, a positive relationship between occupational income and homeownership cannot be invoked to discount the positive correlation between occupational income and the saving ratio. If high income households are more prone to be homeowners than low income households, and if homeownership raises the saving-income ratio, the resulting correlation of income and the saving ratio is not spurious, but measures the *indirect* effect of income differences on the saving ratio.

Turning to the relative dispersion of transitory income and consumption, a greater dispersion leads to greater saving since a household may try to guard against risk by accumulating wealth. The relative dispersion of income is probably greater for the lower occupations than for the higher ones, since unemployment is greater for these occupations, so that this factor, too, produces a bias in favor of the modified permanent income theory.[17]

The next variable is capital gains. Fortunately, the fact that I am

---

tional status of the next generation, so that households with high occupational status tend to have relatively large inheritances. Such a situation is not completely implausible; it could result, for example, from men with high occupational status marrying women whose parents had large positive windfall income, or from parents with large positive windfall incomes providing their children with a better education. But while these explanations are not *completely* implausible, they seem to be rather forced. Hence, the wealth figures provide an additional reason for abandoning the hypothesis that the average propensity to consume is independent of permanent income.

[16] Almost no information is available on the next variable, money holdings, but if we assume that a dollar of money holdings has a roughly similar effect on consumption to a dollar of nonmoney liquid assets, then the problem is less severe because demand deposits are included in liquid assets.

[17] For data on unemployment rates of various occupations in the postwar United States, see [11, p. 62]. These data show substantially greater unemployment rates for manual workers than for white collar employees. The relative dispersion of transitory consumption can be ignored—there is no reason to assume that it varies among occupations.

excluding the self-employed, who presumably account for a large proportion of capital gains, alleviates this problem.[18]

The type of pension system is a variable that is frequently omitted from the consumption function; yet it could be important when comparing the propensities to consume of different occupations since they may have different pension arrangements. There are two aspects to this problem. First, since budget studies generally do not count the employers' or government's pension contribution as saving, total saving is understated. Second, it is highly probable that many of the budget studies did not even count the employee's contribution as saving. The understatement of pension savings is a general problem with cross-section data, and applies also to other cross-section data cited both for, and against, the permanent income theory. But at least in the United States, coverage by private pension systems does not seem to reduce the rate of saving apart from pensions substantially, and may even increase it slightly [3, pp. 51-52] [4]. Hence, if one is interested in saving other than saving via pension funds, one does not have to attach much importance to variations in pension fund coverage of different groups. Moreover, for one of the above-cited studies [3], separate data for covered and noncovered households are available and they reject the modified permanent income theory. No information is available on contractual savings other than pensions.

The final variable, subjective and cultural factors, is vague, and hence hard to analyze. Yet it should, at least, be mentioned since it *may* account for some of the differences in the propensities to consume of various occupations. Thus, it is possible that households with high permanent incomes save a relatively large percentage of their income, in part, because they are better educated and hence have more foresight. Similarly, households with a low permanent income may have a low saving-income ratio because they are less influenced by the "puritan ethic" than are wealthier households. But there is some evidence that the negative correlation of permanent income and the average propensity to consume cannot be due to differences in the psychology of social classes. If it were, one would not find it if one compares occupations comprised in the same broad social class. And a comparison of the average propensities to consume of skilled and unskilled workers

[18] In particular, for those studies which compare white collar employees and manual workers, capital gains are presumably quite unimportant. Insofar as capital gains do occur among the groups covered in the above studies, capital gains (as a proportion of income) are probably positively correlated with permanent income. Hence, by excluding capital gains from income, but including consumption out of capital gains in consumption, the data probably raise the propensity to consume more for households with high permanent income than for households with a low permanent income. The direction of this bias is in favor of the modified permanent income theory [6, p. 693], and hence this factor too cannot be held responsible for the poor showing of the theory. For the prediction coefficient the direction of the bias is uncertain.

shows a significant negative correlation between permanent income and the average propensity to consume.[19] Moreover, if the psychology of different income groups is really important, it is surprising that it has not been used in other consumption function studies, and I will therefore assume that I can ignore it here too. Admittedly, this assumption may be wrong, but if these cultural factors *are* important, much of the evidence favorable to the permanent income theory would have to be re-evaluated too.

There is some additional evidence that the results shown in Tables 1 and 2 do not represent merely the correlation of some other variable with permanent income and with the average propensity to consume. First, I have used budget studies for many countries over many years covering a wide variety of occupations. If the higher propensity to consume of the low permanent income groups were due to a factor other than income, it is not likely that this factor would be operative in nearly all of the studies. Second, I undertook a similar analysis covering only manual workers in the United States 1878-1900; it, too, rejects the permanent income theory[20] [22]. It is unlikely that any special factors which account for the negative correlation of permanent income and consumption when different broad occupational groups are considered would also account for the same result for relatively narrow occupational groups, all of them manual workers.

Hence, one may conclude that using the conventional definition of consumption, on the one hand, the permanent income theory is wrong in asserting that the marginal propensity to consume permanent income is equal to the average propensity. On the other hand, the theory does contain an important truth since the marginal propensity to consume *is* greater for permanent than for measured income. But the substitution of permanent income for measured income reduces the gap between the average and marginal propensities to consume only by about a third.[21]

[19] There are twelve studies which provide data for skilled (or skilled and semiskilled) workers and for unskilled (or unskilled and service) workers. In eleven of these cases the unskilled workers have a higher average propensity to consume. This is significant at the one per cent level. (In one of these studies the difference between the two propensities to consume shows up only in the second decimal place. If one therefore leaves this study out the results are still significant at the one per cent level.)

[20] For these data the prediction coefficients (which are not given in the article) show a great variability and the median is, by a narrow margin, greater than zero, thus supporting the conventional theories. The difference between these two studies in this regard could be due to sampling fluctuations. In any case, the 19th century data are not as reliable as the ones used in this paper.

[21] But note that this result is derived from a very simple consumption function. A more complex function which includes variables such as liquid assets which are likely to be partially proxies for permanent income would presumably show a smaller difference between the propensities to consume measured and permanent income.

REFERENCES

1. G. ACKLEY, *Macroeconomic Theory*. New York 1961.
2. C. ANDERSON, "Lifetime Inter-Occupational Mobility Patterns in Sweden," *Acta Sociologica*, *1*, 168-202.
3. P. CAGAN, *The Effect of Pension Plans on Aggregate Saving*, Nat. Bur. Econ. Research, Occas. Paper 95, New York 1965.
4. ———, "The Use of Wealth to Compare Households Average Saving," *Jour. Am. Stat. Assoc.*, Sept. 1964, *59*, 737-45.
5. R. EISNER, "The Permanent Income Hypothesis: Comment," *Am. Econ. Rev.*, Dec. 1958, *48*, 972-90.
6. M. J. FARRELL, "The New Theories of the Consumption Function," *Econ. Jour.*, Dec. 1959, *69*, 678-704.
7. M. FRIEDMAN, "The Permanent Income Hypothesis: Comment," *Am. Econ. Rev.*, Dec. 1958, *48*, 990-91.
8. ———, *A Theory of the Consumption Function*. Princeton 1957.
9. I. FRIEND AND I. KRAVIS, "Consumption Patterns and Permanent Income," *Am. Econ. Rev.*, *Proceedings*, May 1957, *47*, 536-55.
10. I. FRIEND AND S. SCHOR, "Who Saves?" *Rev. Econ. Stat.*, May 1959, *41*, Pt. 2, 213-48.
11. R. GORDON, "Has Structural Unemployment Worsened?," *Inds. Rel.*, May 1964, *3*, 53-77.
12. H. S. HOUTHAKKER, "The Permanent Income Hypothesis," *Am. Econ. Rev.*, June 1958, *48*, 396-404.
13. ———, "The Permanent Income Hypothesis: Reply," *Am. Econ. Rev.* Dec. 1958, *48*, 991-93.
14. G. KATONA, *The Powerful Consumer*. New York 1960.
15. L. KLEIN, "Applications of Survey Methods and Data to the Analysis of Economic Fluctuations," in George Katona, L. Klein, J. Lansing, and J. Morgan, *Contributions of Survey Methods to Economics*, New York 1954, pp. 241-60.
16. ———, "Patterns of Savings: The Surveys of 1953 and 1954," *Bull. Oxford Univ. Inst. Stat.*, May 1955, *17*, 173-214.
17. ———, K. H. STRAW, AND PETER VANDOME, "Savings and Finances of the Upper Income Classes," *Bull. Oxford Univ. Inst. Stat.*, Nov. 1956, *18*, 293-319.
18. I. KRAVIS, *The Structure of Income*. Philadelphia 1962.
19. S. LIPSET AND R. BENDIX, *Social Mobility in an Industrial Society*. Berkeley 1959.
20. H. F. LYDALL, *British Incomes and Savings*. Oxford 1955.
21. T. MAYER, *Duplicated Appendix* (available upon request).
22. ———, "The Permanent Income Theory and Occupational groups," *Rev. Econ. Stat.*, Feb. 1963, *45*, 16-22.
23. F. MODIGLIANI AND A. ANDO, "The 'Permanent Income' and 'Life Cycle' Hypothesis of Savings Behavior: Comparison and Tests," in I. Friend and R. C. Jones (ed.), *Consumption and Saving*, Vol. 2, Philadelphia 1960, pp. 49-176.

24. F. Modigliani and R. Brumberg, "Utility Analysis and the Consumption Function: An Interpretation of Cross-Section Data," in K. Kurihara, *Post-Keynesian Economics*, London 1955, 388-436.
25. F. M. Williams, and C. Zimmerman, *Studies of Family Living in the United States and Other Countries: An Analysis of Material and Method*, U. S. Dept. Agriculture, Misc. Pub. No. 223, Washngton, D.C. 1935.
26. K. Yoshihara (Chief General Affairs Section, Bureau of Statistics), letter, Tokyo, February 7, 1964.
27. C. Zimmerman, *Incomes and Expenditures of Village and Town Families in Minnesota*, Univ. Minnesota Agric. Experiment Station, Bulletin No. 253, St. Paul 1929.
28. Amsterdam, Bureau van Statistiek der Gemeente Amsterdam, Statistische Mededeelingen No. 80, *Huishoudrekenigen van 212 Gezinnen uit Verschillen de Kringen der Bevolking*, Amsterdam 1927.
29. Basel, Statistisches Amt des Kantons Basel-Stadt, Mitteilungen No. 45, *Die Veränderung der Lebenshaltungskosten 1911-1922 und die Basler Indixziffern*, Basel 1923.
30. Czechoslovakia, Zprávy, *Stàtniho Úradu Statistického Republiky Československè, 7*, No. 112-117, 1926.
31. Denmark, Denmarks Statistik, "Hushholdningsregnskaber 1931," in *Statistiske Meddelelser* ser. 4, 100 No. 1, Copenhagen 1937.
32. ———, *Opsparing i Lønmodtagerhusstandene, 1955*, Statistiske Undersøgelser No. 3, Copenhagen 1960.
33. ———, "Udgifter og Opsparing i Lønmodtagerhusstande," *Statistike Efterretninger*, Dec. 1957, *49*, 1129-47.
34. Finland, Sosialiministeriet "Perhetalouksien elinkustannukset vousina 1920-1921," *Sosiaaline Aikakauskirja, 18*, 1924, pp. 800-816.
35. International Labor Office, *Yearbook of Labor Statistics, 1938*. Geneva 1938.
36. Israel, "Survey of Family Savings 1957-58," *Bank of Israel Bull.*, No. 10, Oct. 1959, 17-41.
37. Japan, Office of the Prime Minister, *Annual Report on the Family Income and Expenditure Survey, 1960*, Tokyo, n.d.
38. Netherlands, Centraal Bureau voor de Statistiek, *Huishoudrekeningen van 598 Gezinnen*. Gravenhage 1937.
39. ———, *National Budgetonderzoek, 1951*. Utrecht 1953 and Zeist 1957.
40. Norway, Statistisk Sentralbyra, Norges Offisielle Statistikk, *Forbruksundersøkelsen* 1958, *1* + *2*, Oslo 1961 (mimeo.).
41. Oxford University Institute of Statistics, Card deck and related information from *1953-54 Incomes and Savings Survey*.
42. Portugal, Instituto Nacional de Estatística, Estudos No. 30, *Inquérito ds condicões de viva da populãcao da cidade de Coimbra, 1953-54*, Lisbon 1958.
43. ———, Estudos No. 35, *Inquérito ds condicões de viva da populãcuo da cidade de Evora, 1955-56*. Lisbon 1960.
44. ———, Estudos No. 37, *Inquérito ds condicões de viva da populãcao da cidade de Viseau, 1955-56*. Lisbon 1960.

45. Sweden, Konjunkturinstitutet, Meddelanden Ser. B: 25, *Hushållens Sparande år 1955*, 1, Stockholm, 1959.
46. ———, Meddelanden Ser. B: 31, *Hushållens Sparande år 1957*. Stockholm 1963.
47. Sweden, Statistiska Centralbyrån, *Statistisk Årsbok för Sverige, 1962*, Vol. 49. Stockholm 1962.
48. Sweden, Sveriges Officiella Statistik, Socialstyrelsen, *Levnadskostnaderna i Städer och Industriorter omkring år, 1923*, Stockholm 1929.
49. Sweden, Sveriges Officiella Statistik, K. Socialstyrelsen, *Levnadsvillkor och Hushållsvanor i Städer och Industriorter, omkring år, 1933*. Stockholm 1938.
50. Switzerland, Departement Federal de l'Economie Publique, *Budgets Familiaux de la Population Salariée, 1936-37 et 1937-38*. Suppl. No. 42 *Vie Economique*. Berne 1942.
51. Switzerland, Schweizerische Gesellschaft für Statistik und Volkswirtschaft, *Handbuch der Schweizerischen Volkswirtschaft*, Vols. 1 and 2, Berne 1955.
52. Switzerland, Schweizerisches Arbeitersekretariat, *Die Lebenshaltung schweizerischer Arbeiter und Angestellter vor dem Krieg*. Zurich 1922.
53. Switzerland, Volkswirtschafts Department, "Budgets Familiaux de la Population Salariée année 1949," *La Vie Economique*, Feb. 1951, *24*, 49-62.
54. ———,"Budgets Familiaux de la Population Salariée année 1951," *La Vie Economique*, Sept. 1952, *25*, 388-402.
55. ———, "Budgets Familiaux de la Population Salariée année 1952," *La Vie Economique*, Oct. 1953, *26*, 380-94.
56. U. S. Board of Governors of the Federal Reserve System, "1951 Survey of Consumer Finances, Pt. IV," *Fed. Res. Bull.*, Sept. 1951, *37*, 1061-78.
57. U. S. Board of Governors of the Fed. Res. System, "1952 Survey of Consumer Finances," *Fed. Res. Bull.* Sept. 1952, *38*, 974-1001.
58. U. S. Bureau of Labor Statistics, *Family Expenditures in Selected Cities, 1935-36*, Bull. 648, 8, Washington, D.C., 1941.
59. U. S. Bureau of Labor Statistics, *Consumer Expenditures and Income, Urban United States 1960-61*, BLS Rept. No. 237-38, Washington, D.C., 1964 + 1965, and Suppl. 2-Part A to BLS Report 237-38.
60. University of Michigan, Survey Research Center, Card Deck from 1949 Survey of Consumer Finances.
61. ———, *1960 Survey of Consumer Finances*. Ann Arbor 1961.
62. University of Pennsylvania, Wharton School of Finances and Commerce, *Study of Consumer Expenditures, Incomes and Savings*, Vols. 1, 18. Philadelphia, 1956 and 1957.
63. Zürich, Statistisches Amt der Stadt Zürich, *Statistik der Stadt Zürich* No. 28, Haushaltsrechnungen aus dem Jahre 1919, Zürich, 1921.

FULL EMPLOYMENT POLICY AND ECONOMIC GROWTH

By RICHARD R. NELSON*

Within the context of a simple aggregative model, this paper explores how the rate of labor force growth and the rate of technical advance influence the fiscal and monetary policies needed to maintain equality of aggregate demand and potential output. The model attempts to tie together two strands of analysis that have developed in the literature—a neoclassical strand stemming from Solow's work on the determinants of growth of potential output and a neo-Keynesian strand stemming from the Harrod-Domar studies of the conditions under which full employment can be maintained in a growing economy [8] [9] [4] [1].[1]

I. *The Basic Model*

The framework of the model is traditional. The supply of output at full employment (or potential output) is assumed to be a Cobb-Douglas function of capital and labor, given the level of technology. To simplify notation in subsequent calculations, and to facilitate generalization in the appendix, the production function will be written in a slightly unconventional form:

$$Q = K^{\alpha}(L \cdot A)^{1-\alpha}, \tag{1}$$

where Q is potential output, K is the stock of capital, L is the labor supply and A an index of labor effectiveness. Technical progress is assumed to be purely labor augmenting. (In the Cobb-Douglas production function, labor augmenting technical progress and Hicks neutral technical progress are equivalent.) Thus the rate of growth of potential output is:[2]

*Any views expressed in this paper are those of the author. They should not be interpreted as reflecting the view of The RAND Corporation or the official opinion or policy of any of its governmental or private research sponsors.

The author is indebted to Benton Massell, Edmund Phelps, Hugh Rose, and Robert Solow for a number of useful comments and suggestions.

[1] There are a number of other treatments of this same general topic, the relationship between growth of potential output and the conditions required for full employment. Tobin [10] [11], Smith [7], Eisner [2], and Hall [3], in particular, have dealt with this question, but within the context of models that differ from the present one in significant respects.

[2] The more conventional formulation is:

$$Q = BK^{\alpha}L^{1-\alpha}, \quad \text{and} \quad \frac{\dot{Q}}{Q} = \frac{\dot{B}}{B} + \alpha\frac{\dot{K}}{K} + (1-\alpha)\frac{\dot{L}}{L}.$$

If $A = B^{1/1-\alpha}$, the two formulations, of course, are exactly equivalent.

$$\frac{\dot{Q}}{Q} = \alpha \frac{\dot{K}}{K} + (1 - \alpha) \left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A} \right). \tag{2}$$

In the appendix to this paper a more general production function is used. While there are a number of important added complications, it is demonstrated there that many of the implications of the model employing a Cobb-Douglas production function carry over, with qualifications, when a more general neoclassical production function is employed, so long as it is assumed that technical progress is purely labor augmenting.

On the demand side, I ignore exports and imports and assume total effective demand is given by:

$$Y = C + G + I, \tag{3}$$

where Y is total demand and C, G, and I are consumption, government spending, and investment, respectively. Consumption is assumed to be a constant fraction of disposable income which in turn is equal to GNP minus taxes at a constant proportional tax rate, t. There are no business retained earnings or depreciation allowances. Government spending is a constant fraction, g, of GNP. Thus:

$$Y = c(Y - tY) + gY + I. \tag{4}$$

Defining $s = 1 - c$, this can be written in the familiar form:

$$Y = \frac{I}{s + t(1 - s) - g}. \tag{5}$$

The concept of "full employment" in equation (1) is a normative one relating to the socially optimal degree of labor utilization. In general the measured rate of unemployment will not be zero at "full employment" output, nor is this the maximum output that can be attained; however, if output is pushed (or pulled by demand) beyond this level there will be stronger inflationary pressures. Full employment output thus represents output at the degree of labor utilization the society deems optimal, balancing the benefits in terms of decreased unemployment and greater output that result as demand is increased, against the greater inflationary tendencies and, perhaps, higher unit costs, that also are a concomitant of greater pressure of demand on a given supply of labor and capital.[2a]

[2a] The relationship between output and increased labor force utilization need not, and in general will not, follow equation (1). One would expect average labor quality to decline as labor utilization rates increased. While in a situation where the economy was operating with capital being underutilized the decreased labor quality effect might be for a while be offset by more efficient operation of existing capital, we are concerned with sustained degrees of labor utilization, and it can be assumed that capital adjusts to that rate. Thus in the long-run production function with the (sustained) degree of labor utilization as a variable, output should be positively related to amount of employment but negative related to the degree of labor force utilization.

A principal objective of fiscal and monetary policy, then, is to adjust actual output so that it is equal to full employment output, neither lower with higher unemployment rates, nor higher with more rapid price increase. For this objective to be achieved, the demand for goods and supply of goods at full employment, Y and Q, must be equal over time.[3] If we focus on a moment of time, say time t_0, then the level of effective demand generated by investment through the multiplier will equal full employment output when:

$$I_0 = [s + t(1 - s) - g]Q_0. \tag{6}$$

For full employment to persist into the next moment of time:

$$\left(\frac{\dot{I}}{I}\right)_0 = \left(\frac{\dot{Q}}{Q}\right)_0. \tag{7}$$

These conditions of course do not imply that full employment will persist further. But under certain conditions $\dot{I}/I$ and $\dot{Q}/Q$ will be constants. If they are constants and if equations (6) and (7) hold initially, Y and Q will continue to be equal.

The key relationship in the model is the investment equation. Investment is both an important contributor to growth of potential output, and the principal dynamic determinant of the level of aggregate demand. I assume that the level of investment is determined by the volume of profitable opportunities to expand the capital stock. In this paper we are examining conditions that must hold for full employment (and full utilization of the capital stock) to be realized; if there is no slack, there will be profitable opportunities for new investment if the marginal productivity of capital (at full employment of labor) exceeds the interest rate.[4] Indeed, at interest rate r there will be profitable investment opportunities so long as the actual capital stock falls short of K^*, the profit maximizing stock, where:[5]

[3] I assume that, if the goods market is in balance at full employment, the labor market is also—that is, that the real wage rate is such that the demand for labor equals the full employment supply of labor when aggregate demand and supply of output are in balance.

[4] There are, of course, two major qualifications to this. First, the correct statement of the conditions for expected profitability is that the expected discounted flow of future net returns exceeds the cost of new equipment. This involves a look into the future. The condition of excess of short-run marginal productivity over the existing interest rate is a proxy for the correct profitability conditions only under rather special circumstances. Second, under conditions of risk aversion or imperfect capital markets, the cutoff on investment decisions might occur at expected rates of return significantly higher than the interest rate.

[5] K^* is derived by solving for K in the equation:

$$r = \frac{\delta O}{\delta K} = \alpha K^{(\alpha-1)} L^{(1-\alpha)} A^{(1-\alpha)}.$$

Again, it should be noted that I am assuming the wage rate is compatible with full employment

(8) $$K^* = \left(\frac{\alpha}{r}\right)^{1/1-\alpha} L \cdot A.$$

The investment equation is:

(9) $$I = \lambda(K^* - K),$$

or

(9a) $$I = \lambda \left[\left(\frac{\alpha}{r}\right)^{1/1-\alpha} L \cdot A - K\right]$$

Thus the greater the opportunities for profitable expansion of the capital stock, the greater the rate of investment.

The dynamics of the model are provided by labor force growth and technical advance. Notice that these factors have two effects. First, both increase potential output. In the popular discussion of the problem of maintaining full employment it seems to be generally believed that the more rapid are technical progress and labor force growth, the greater is the difficulty in achieving an equal growth of demand. But, of course, a second effect of technical progress and labor force growth is to increase the size of the capital stock that is profitable at any given interest rate—and thus to spur investment and increase effective demand. (A third effect, then, is to further spur potential output through the increase in investment they stimulate.) If we take $\dot{A}/A$ and $\dot{L}/L$ as autonomous, and are free to vary r and t, under what values of r and t will the capacity-creating and demand-increasing effects of technical progress and labor force growth be equal? Is it true that if labor force growth or technical progress is more rapid, a more expansionary government policy is needed? These are the questions explored in the following sections.

Although these are Harrod-Domar type questions, the model presented here enables explicit recognition that the growth of the labor force and the advance of technology affect both the natural rate of growth (the growth of potential output) and the growth of investment; in the Harrod-Domar analysis the effect of these variables on investment demand is suppressed. The underlying model of growth of potential output follows Solow's neoclassical model. However, Solow does not work with explicit demand equations and hence cannot examine the conditions of sustained full employment; obviously this hinges on the

---

if the other conditions are met—that

$$\left.\frac{\delta 0}{\delta L}\right|_{\substack{L = L\text{ full} \\ K = K^*}} = W$$

assumed response of investors and savers to various conditions. Thus this model is a marriage of the two strands.

Before proceeding with the analysis it is important to make clear one important thing that the model does not try to do. The model says nothing about what happens if the dynamic equilibrium conditions are not met—if there is either excess demand or involuntary unemployment. There are two themes in the Harrod-type analysis—a statement of conditions of dynamic equilibrium, and a statement about what happens if there is excess capacity. The simple investment function used in this model could be enriched to take account of slack and of unfilled orders for new equipment, but I have not done this in the formal model. Also, the model contains no mechanism, explicit or implicit, to explain why the interest rate is what it is; it only attempts to say what the interest rate must be to be compatible with full employment, given the other variables of the system.[6]

II. *Full Employment Without Fiscal Policy*

Let us begin by assuming no government spending or taxes. Assume also that technical progress and labor force growth are proceeding at constant rates. The variable that is free is the interest rate. What is required of the interest rate if full employment is to be achieved and maintained?

Recalling the necessary conditions [equations (6) and (7)], the investment demand equation (9a), and the growth of potential output equation (2), the requirements for full employment to exist and persist into the next moment of time are:

$$I(A, L, r, K) = sQ \tag{6a}$$

$$\frac{I_A A + I_L L + I_r \dot{r} + I_K \dot{K}}{I} = \alpha \frac{\dot{K}}{K} + (1 - \alpha) \left(\frac{L}{L} + \frac{A}{A}\right). \tag{7a}$$

If we ignore depreciation (which is treated in the appendix), the two equations are, of course, linked by the identity: $I = \dot{K}$. The relevant calculations are made in a footnote.[7]

[6] Notice that the behavior equations of the model do not involve any wealth or financial variables explicitly. The model could be enriched to include these, as in the Tobin and Hall models. However, the benefits of the simple treatment used here are that certain important relationships stand out more strongly. Implicitly, something like the following is assumed with respect to financial and wealth variables. The allocation of accumulated consumer saving between money and claims on real assets is a function of the interest rate; however, the saving rate is not a function of either the interest rate or of wealth. The managers of business firms invest in new plant and equipment and pay for this by selling claims. The government finances its deficits by selling bonds whose rate of return is pegged to the rate of return on claims on real assets. The money price level is rigid.

[7] The more explicit form of (6a) is:

One implication is that, under our assumptions, full employment will be compatible with a constant interest rate only if conditions are such that the capital stock and potential output are growing at the same rate. Or:

$$\frac{\dot{K}}{K} = \frac{\dot{Q}}{Q} = \frac{\dot{L}}{L} + \frac{\dot{A}}{A}. \tag{10}$$

Since $\dot{K}/K = sQ/K$ (assuming full employment), the saving rate, the output-capital ratio, the growth of labor force, and the rate of advance of technology must be related as follows:

$$\frac{Q}{K} = \frac{1}{s}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right). \tag{11}$$

This "golden age" result was to be expected. Under the production function assumptions the rate of return on capital will rise or fall depending on whether the output-capital ratio is rising or falling. If Q/K initially falls short of (11), at investment equal to full employment saving, $\dot{K}/K$ will fall short of $\dot{Q}/Q$ and hence both the output-capital ratio and the rate of return on capital will rise. If Q/K exceeds (11), investment equal to full employment will lead to a fall in the output-capital ratio and the rate of return on capital. Under our investment assumption, such a rise or fall will lead to an increase or decrease in investment as a fraction of potential GNP, unless offset by a compensating change in the interest rate. Since saving is assumed to be a constant percentage of GNP, these compensating changes in r are then required for maintenance of equality between Y and Q.

The system will asymptotically approach the golden age solution if $\dot{L}/L$, $\dot{A}/A$, and s remain constant. Let us now examine what this asymp-

---

$$\lambda\left[\left(\frac{\alpha}{r}\right)^{1/1-\alpha} L\cdot A - K\right] = sQ. \tag{6b}$$

Noting that $I = sQ$, the explicit form of (7a) is:

$$\frac{\lambda\left[\left(\frac{\alpha}{r}\right)^{1/1-\alpha} L\cdot A\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A} - \frac{1}{1-\alpha}\frac{\dot{r}}{r}\right) - \dot{K}\right]}{sQ} = \alpha\frac{\dot{K}}{K}(1-\alpha)\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right). \tag{7b}$$

From (6b) we know that $\lambda[(\alpha/r)^{1/1-\alpha} L\cdot A] = \lambda K + sQ$. Substituting in (7b) and solving for $\dot{r}/r$:

$$\frac{\dot{r}}{r} = -\left[\frac{sQ}{K} - \frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right](1-\alpha)\left[\frac{\lambda K + sQ\alpha}{\lambda K + sQ}\right].$$

$$\frac{\dot{r}}{r} = -\left[\frac{1}{1-\alpha}\frac{\dot{K}}{K} - \frac{\dot{Q}}{Q}\right](1-\alpha)\left[\frac{\lambda K + sQ\alpha}{\lambda K + sQ}\right].$$

Equation (10) follows by solving for the conditions under which $\dot{r}/r = 0$.

totically required interest rate is, and the variables on which it depends. Setting $\dot{r}=0$ and $Q/K=1/s(\dot{L}/L+\dot{A}/A)$ in equations (6a) and (7a) yields the following expression for the full employment interest rate in golden age equilibrium:[8]

$$r = \frac{\alpha}{s}\left(\frac{\dot{L}}{L}+\frac{\dot{A}}{A}\right)\left[\frac{\lambda}{\frac{\dot{L}}{L}+\frac{\dot{A}}{A}+\lambda}\right]^{1-\alpha}. \tag{12}$$

If λ is very large the condition stated in equation (12) approaches:

$$r = \frac{\alpha}{s}\left(\frac{\dot{L}}{L}+\frac{\dot{A}}{A}\right). \tag{12a}$$

For some purposes (12a) will be easier and more illuminating to work with.[9]

If the r of equation (12) prevails, the growth of potential output generated by $\dot{L}/L$ and $\dot{A}/A$, and the growth of effective demand spurred by the impact of $\dot{L}/L$ and $\dot{A}/A$ on investment opportunities, will be in balance. Equation (12) can be interpreted as defining Wicksell's natural rate of interest—the interest rate at which investment demand and full employment saving are equal. Equation (12) also can be interpreted as defining the conditions under which the natural rate of growth will equal the warranted rate of growth in a Harrod-like formulation of the problem; if we work with (12a), $\dot{L}/L+\dot{A}/A$ clearly is the natural rate of

[8] Manipulation of the Cobb-Douglas equation allows (6b) to be written:

$$\lambda\left[\left(\frac{\alpha}{r}\frac{Q}{K}\right)^{1/1-\alpha}K-K\right]=sQ. \tag{6c}$$

Dividing by K and noting equation (11) and that $\dot{K}/K=sQ/K$ we have the following triplet of equalities which define the conditions of sustained full employment in golden age equilibrium:

$$\frac{\dot{L}}{L}+\frac{\dot{A}}{A}=\lambda\left[\left(\frac{\alpha}{r}\frac{Q}{K}\right)^{1/1-\alpha}-1\right]=\frac{sQ}{K}.$$

Solving the first two for Q/K:

$$\frac{Q}{K}=\left|\frac{\frac{\dot{L}}{L}+\frac{\dot{A}}{A}+\lambda}{\lambda\left(\frac{\alpha}{r}\right)^{1/1-\alpha}}\right|^{1-\alpha}.$$

Noting that Q/K also must equal $1/s(\dot{L}/L+\dot{A}/A)$ and solving for r yields equation (12).

[9] Of course (12a) could have been derived directly by assuming that the actual capital stock always equalled the profit-maximizing capital stock—there are never any unexploited investment opportunities—and working with an investment equation, $I=\dot{K}^*$. Tobin, in his 1964 *AER* paper [11], derives this equation by examining what will be the marginal productivity of capital in the golden age. Of course, this will be the ***full employment compatible interest rate*** only if it is assumed that $K=K^*$.

growth, equation (8) shows that the desired capital-output ratio, (K^*/Q), equals α/r, and thus $s(r/\alpha)$ is the warranted rate of growth. If the actual and natural rates of interest are equal, then there will be equality of the natural and warranted rates of growth. An interest rate enters this formulation but not Harrod's because of the difference in investment functions. In this formulation, unlike Harrod's, there are a number of capital-output ratios that will satisfy businessmen, not just one, because the desired capital-output ratio is a function of the interest rate.[10] If the interest rate is fixed and not a variable, the requirements for sustained full employment of this model are identical to Harrod's, despite a somewhat different investment demand equation and production function.[11]

Equation (12) shows that given $\dot{L}/L$ and $\dot{A}/A$, the interest rate compatible with full employment is negatively related to the saving rate. To balance a higher saving rate, investment must tap opportunities with a lower rate of return. Given the saving rate, the interest rate will be higher the faster the growth of the labor force, and the more rapid technological progress. While this last result may be surprising, the reason is obvious. Technological advance and labor force growth are the key factors determining the rate at which profitable investment opportunities grow and a given investment rate will yield a higher sustained rate of return when they proceed rapidly than when they are slow. If we treat r as fixed and s as a variable to be adjusted to match the investment-GNP ratio, then the result of faster technological progress or labor force growth would be an increase in the investment-GNP ratio (and a required increase in the saving rate), as equation (13) shows:

$$\frac{I}{Y} = \frac{\alpha}{r}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right)\left[\frac{\lambda}{\frac{\dot{L}}{L} + \frac{\dot{A}}{A} + \lambda}\right]^{1-\alpha}. \tag{13}$$

This is a result clearly in the spirit of Schumpeter. But if the saving rate is fixed, with faster technical progress or labor force growth the interest rate must be increased to keep investment from outrunning full employment saving.

Thus, in this model at least, given any existing interest rate, an acceleration of technical progress or labor force growth provides even more of a spur to demand than to potential output. In a world where the

[10] This, of course, has been the main bone of contention between Solow [10] and Tobin [8], on the one hand, and Harrod [4] on the other.

[11] Eisner makes the point that the Harrod rigidity of (or a ceiling on) the capital-output ratio can stem from either rigidity of the desired capital-output ratio, or from interest rate rigidity or an interest rate floor. If the elasticity of substitution were less than one, then a given change in the desired capital-output ratio would require a far larger change in the interest rate, and the floor interest rate might be hit at a quite low capital-output ratio.

interest rate adjusts up and down to equilibrate investment and full employment saving, this result would be of no particular significance. But if the interest rate is sticky, an acceleration of these forces will be reflected in decreased unemployment or inflationary pressures.

III. *The Requirements for Stabilization Policy*

Active stabilization policy may be required for two different reasons. First, in the absence of active policy it may be impossible to achieve the parameter values consistent with full employment; this might be the case, for example, if there is an interest rate floor. Second, even if the parameters can be achieved without active policy, tendencies of the system to move and stay at full employment may be weak. The concern about full employment generated by the early Harrod-Domar models involved both aspects—the required conditions appeared infeasible (in the absence of government deficit spending programs to reduce total saving as a fraction of GNP) and, in addition, full employment seemed a highly unstable equilibrium.

As pointed out earlier, the model above contains no analysis of the stability of the full employment path, nor will any formal analysis be presented here. However, the following brief discussion may help to put the problem in context.

Assume that when aggregate demand and potential output are not equal, the difference first shows up in unfilled orders or unintended inventory accumulation. Let us focus on the latter case of deficiency in aggregate demand. Assume that there are two kinds of reactions taken by business firms. First, they reduce investment expenditures; thus the basic investment equation must be modified by adding a term reflecting underutilization of existing capital. Second, they reduce production and income payments. They may or may not lay off workers or cut back on new hires—if they do not, they reduce hours and, in any case, incomes are reduced in line with cutbacks in output. Consumption then will fall.

Thus in this model, there may be present the type of instability discussed by Harrod despite strictly neoclassical investment and production assumptions. The sensitivity of investment and output decisions to the accumulation of inventories is a strong destabilizing force. To the extent that the interest rate rises or falls depending on the difference between the existing level of investment and full employment saving, this is a stabilizing force. However, one would expect that, even if the interest rate adjusts relatively quickly, the short-term sensitivity of the desired capital-output ratio to the interest rate might be much smaller than the long-term sensitivity as given by equation (7).[12]

[12] In the limit with no short-run sensitivity of the desired capital-output ratio to the interest

The extent to which active stabilization policy is required depends on the balance between the destabilizing effects of accumulating inventories and growing underutilization of capacity, and the stabilizing effects of interest rate adjustments. In the following discussion it will be assumed that conscious policy actions are required at least from time to time to keep aggregate demand and potential output in balance.

Let us now introduce the government sector and government stabilization policy into the model. If the income tax rate is t, and government expenditures as a fraction of GNP are g, then the conditions of full employment in the steady state are:[13]

$$r = \left(\frac{\alpha}{s + t(1-s) - g}\right)\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right)\left[\frac{\lambda}{\frac{\dot{L}}{L} + \frac{\dot{A}}{A} + \lambda}\right]^{1-\alpha}, \tag{12b}$$

(12a) becomes

$$r = \left(\frac{\alpha}{s + t(1-s) - g}\right)\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right). \tag{12c}$$

For given values of s, g, $\dot{L}/L$, $\dot{A}/A$, and λ, this equation enables us to trace out the alternative pairs of r and t that are consistent with full employment in a golden age.[14] The curve is a rectangular hyperbola, as shown in Figure 1, which will shift to the right or left depending on the strength of the other factors that determine demand.

Note that, with fiscal policy now admissible, the previous results regarding the interest rate compatible with full employment are generalized as follows: the faster are technical progress or labor force growth, the tighter must be stabilization policy. This statement applies to the steady state in which $\dot{K}/K = \dot{Q}/Q$ and a constant r and t are com-

---

rate, and strong sensitivity of investment to the degree of slack, we have an accelerator investment demand equation rather than the long-run neoclassical one of this paper. The investment demand equation of this paper would appear to make behavioral sense only a world in which government policy, or natural stability, is strong enough so that business firms assume slack to be temporary.

[13]

$$\lambda\left[\left(\frac{\alpha}{r}\right)^{1/1-\alpha} L\cdot A - K\right] = [s + t(1-s) - g]Q, \tag{6c}$$

and

$$\frac{\left[\lambda\left(\frac{\alpha}{r}\right)^{1/1-\alpha} L\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right) - \dot{K}\right]}{[s + t(1-s) - g]Q} = \alpha\frac{\dot{K}}{K} + (1-\alpha)\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right). \tag{7c}$$

Only the constant before Q in equations (6) and (7) has been changed. Equation (12b) follows.

[14] The results here are very similar to those of Smith [7] and of Musgrave [5]. However, the investment equation in this model is explicitly dynamic.

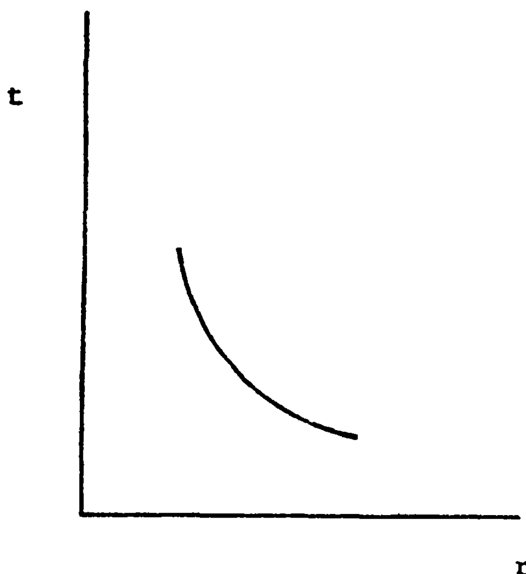


FIGURE 1.
Alternative t, r pairs consistent with equality of Q and Y.

patible with sustained full employment. Assume that we are on one long-run equilibrium time path and that, suddenly, the rates of technical progress or the rate of growth of the labor force rises. What is required of policy?

One possibility is to shift immediately to the new steady state path through fiscal policy. In the new steady state $\dot{K}/K$ must rise to equal the now increased $\dot{L}/L+\dot{A}/A$; thus room must be made for a higher ratio of investment to GNP through increased taxes so that $[s+t(1-s)-g]Q/K=\dot{L}/L+\dot{A}/A$ at the new higher level. If λ is infinite, investment will rise to the needed higher fraction of potential GNP at the old interest rate. If λ is not infinite, equation (12b) shows that some reduction in the interest rate will be required. Once these changes in the tax and interest rates are made, the system is on a golden age path again, with a higher investment-GNP ratio.

Another possibility is to stress monetary policy and to respond more gradually. Taxes can be maintained and the interest rate tightened to keep investment demand from rising as a fraction of potential GNP. With technical advance now more rapid, and the interest rate increased to keep investment at the same fraction of GNP, the rate of growth of the capital stock will be faster than it was because GNP will be growing faster. But it will not immediately rise to equal the faster rate of growth of potential output. Therefore the capital-output ratio will gradually fall toward its new (lower) steady state equilibrium. And as it falls, the

rising marginal productivity of capital will require further increases in the interest rate. Eventually the new equilibrium will be reached.

The fact that these choices are open to the government means that full employment policy and growth policy cannot be separated; the target degree of labor utilization can be achieved through different alternative policies which have different effects on present consumption and future output. The golden rule for golden age advocates would keep the investment—GNP ratio constant and equal to capital's share of GNP, and vary the interest rate up or down following variations in $\dot{L}/L+\dot{A}/A$. Those who believe in a constant social rate of time preference would keep r constant and at that rate, and permit I/GNP to rise or fall with $\dot{L}/L+\dot{A}/A$, varying the tax rate to make room.

Returning to the steady state conditions, an interesting question is: under what conditions will full employment be consistent with a government surplus? For simplicity we will use equation (12c). Rearranging:

$$t - g = \frac{\alpha}{r}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right) - s + st.$$

Letting $S^* = t - g$ be the government surplus as a fraction of potential GNP:

$$S^* = \frac{\frac{\alpha}{r}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right) - s + sg}{1 - s}. \tag{14}$$

Equation (14) shows some familiar results. The balanced budget multiplier requires the surplus at full employment without inflation to be larger (or the deficit smaller) as government spending increases as a function of potential GNP. And obviously the surplus must be larger, or the deficit smaller, the looser is monetary policy. The new result is that, given g and r, the faster technical progress and labor force growth, the greater will be the surplus or the smaller the deficit needed to keep Q and Y in line.[15] In a very real sense the higher tax rates required when technological change is rapid should be viewed as the price we have to pay to achieve rapid growth without inflation. But if there are pressures against increasing government spending as a percentage of GNP, if there exist strong political pressures against running sustained budget deficits, and if there is a floor to the interest rate, rapid technical progress, far from being a threat to full employment, may be a prerequisite for it.

[15] Here the ignoring of the effect of financial wealth and liquidity is important. Obviously the size and sign of the deficit has an effect on the economy through these variables as well as through disposable income.

Appendix

If we assume that the capital stock depreciates at rate δ, this affects the basic equations in two ways. First, at the profit maximizing capital stock, the marginal productivity of capital is equated to $r+\delta$, not r. Second, the rate of growth of the capital stock is equal to $sQ/K-\delta$, not sQ/K. This affects the conclusions in the following ways. First, the golden age output-capital ratio is

$$\frac{Q}{K} = \frac{1}{s}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A} + \delta\right).$$

Second, the golden age interest rate is

$$r = \frac{\alpha}{s}\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A} + \delta\right)\left[\frac{\lambda}{\frac{\dot{L}}{L} + \frac{\dot{A}}{A} + \lambda + \delta}\right]^{1-\alpha} - \delta.$$

Thus the qualitative conclusions change in no essential way.

Relaxing the Cobb-Douglas assumption raises a number of complications. In particular Hicks neutral technical progress no longer is necessarily equivalent to labor augmenting technical advance, and further there may be upper or lower bounds on the output-capital ratio which preclude equality of the rate of growth of the capital stock and the "augmented" labor force for certain values of s, $\dot{L}/L$, and $\dot{A}/A$. However, assuming technical progress is labor augmenting and that a golden age (with a positive elasticity of output with respect to labor) is possible within the domain of interest, the qualitative results apply to a model with a more general neoclassical production function.

The production function can be written in the more general form

$$Q = F(K, L\cdot A).$$

Assuming technical progress is labor augmenting:

$$\frac{\dot{Q}}{Q} = \left[\frac{F_k K}{Q}\right]\frac{\dot{K}}{K} + \left[\frac{F_{LA} L\cdot A}{Q}\right]\left(\frac{\dot{L}}{L} + \frac{\dot{A}}{A}\right).$$

If $F(\cdot)$ has the standard neoclassical properties and there is perfect competition, the terms in brackets will be the shares of capital and labor, respectively. Investment will be profitable up to the point where $F_k(K, L\cdot A)=r$. Under our assumptions this is equivalent to $F_k(K/L\cdot A, 1)=r$. The profit-maximizing capital stock is $K^* = F_k^{-1}(r)\cdot LA$. Notice that the forms of these more general equations are very similar to the Cobb-Douglas special cases.

The conclusions of the model would appear to depend strongly on the existence of a stable golden age equilibrium where $sQ/K=\dot{L}/L+\dot{A}/A$. With a Cobb-Douglas function Q/K increases without finite upper bound as $L\cdot A/K$ increases, and declines toward zero as $K/L\cdot A$ increases; thus there is a Q/K which can satisfy the golden age equation for any positive

values of s, $\dot{L}/L$, and $\dot{A}/A$. This does not apply generally; for certain neoclassical production functions Q/K may be bounded above as $L \cdot A/K$ increases, or approach a positive lower bound as $K/L \cdot A$ increases.

So long as we are considering values of $1/s(\dot{L}/L+\dot{A}/A)$ which do not fall outside the bounds of Q/K, the qualitative conclusions regarding the relationship of $\dot{L}+\dot{A}/A$ to the full employment interest rate carry over to this more general formulation. Within this range at least, the marginal productivity of capital is an increasing function of $Q/K=h(L \cdot A/K)$, and the higher the marginal productivity of capital the higher must be the interest rate required to keep investment demand equal to full employment saving (given the investment and saving assumptions of the model). This result does not depend on the sign or size of the elasticity of substitution, although the quantitative sensitivity of r to $\dot{L}/L$ and $\dot{A}/A$ will depend on the exact shape of the isoquant. Further, it is interesting to note that, for the case where the elasticity of substitution is less than one, labor's share will tend to be smaller in the full employment equilibrium when technical change and labor force growth are rapid than when they are slow; the opposite holds if the elasticity of substitution exceeds one.

However, if the elasticity of substitution is less than one, it is possible that at very rapid rates of growth of labor or technical advance, it will be impossible to achieve a golden age equilibrium with capital and "augmented" labor growing at the same rate. For as Q/K and $L \cdot A/K$ increase, the elasticity of output with respect to labor will fall, and it may fall to zero before Q/K can equal $\dot{L}/L+\dot{A}/A/s$. In this case asymptotically $\dot{Q}/Q$ and $\dot{K}/K$ will grow at the same pace but slower than $\dot{L}/L+\dot{A}/A$. A related problem happens when the elasticity of substitution is greater than one, and $\dot{L}/L+\dot{A}/A$ is small. (For an excellent discussion, see Pitchford [6].)

If s and $\dot{L}/L+\dot{A}/A$ take on values such that either of these conditions occurs, the analysis becomes quite complicated, depending on the exact form of the production function. Historically, however, we have not come close to experiencing situations where labor's share of national income was driven close to zero. If the rest of the assumptions of the model do not strain reality too much, the limitation that the model only applies to a particular range of s, $\dot{L}/L$, and $\dot{A}/A$ [the range depending on $F(\cdot)$] does not appear to detract from any heuristic value it might have.

References

1. E. D. Domar, "Expansion and Employment," *Am. Econ. Rev.*, March 1947, *37*, 34–55.
2. Robert Eisner, "On Growth Models and the Neo-Classical Resurgence," *Econ. Jour.*, Dec. 1958, *68*, 707–21.
3. C. A. Hall, *Fiscal Policy for Stable Growth*. New York 1960.
4. R. F. Harrod, *Toward a Dynamic Economics*. London 1958.
5. R. A. Musgrave, *The Theory of Public Finance*. New York 1959.
6. J. D. Pitchford, "Growth and the Elasticity of Substitution," *Econ. Record*, Dec. 1960, *36*, 491–504.

7. W. L. SMITH, "Monetary-Fiscal Policy and Economic Growth," *Quart. Jour. Econ.*, Feb. 1957, *71*, 36–55.
8. R. M. SOLOW, "A Contribution to the Theory of Economic Growth," *Quart. Jour. Econ.*, Feb. 1956, *70*, 65–94.
9. ———, "Technical Change and the Aggregate Production Function," *Rev. Econ. Stat.*, Aug. 1957, *39*, 312–20.
10. JAMES TOBIN, "A Dynamic Aggregative Model," *Jour. Pol. Econ.*, Apr. 1955, *63*, 103–15.
11. ———, "Economic Growth as an Objective of Government Policy," *Am. Econ. Rev.*, May 1964, *54*, 1–20.

THE HECKSCHER-OHLIN THEOREM, THE LEONTIEF PARADOX, AND PATTERNS OF ECONOMIC GROWTH

By NOBUO MINABE*

The core of the Heckscher-Ohlin theorem of the nature of trade patterns [6] [15] is the explanation of the relation between factor endowments and the doctrine of comparative costs (or commodity price ratios). The theorem asserts that a country exports those commodities produced with relatively large amounts of the country's relatively abundant factor. Thus a capital rich country exports capital intensive products. Leontief [13] applied the theorem to upset the popular notion that the United States exports capital intensive commodities. Leontief's input-output data show that America imports capital intensive commodities compared with American export goods. Heckscher defined relative factor abundance by the pre-trade ratio of factor prices in the two countries. By definition capital is relatively cheaper in the capital rich country before trade. On the other hand, Ohlin and Jones [10] introduced an alternative definition of relative factor abundance, which is related to the physical endowments of the factors of production in each country. According to the definition a country is relatively capital abundant if, and only if, it is endowed with a higher proportion of capital to labor than the other country. By using the revised definition, they have demonstrated that a bias on the supply side, due to differences in (physical) factor endowments between the two countries, may be offset by differences in the community taste patterns between countries. The other reason for the Leontief paradox, which has been given by Jones and later Johnson [8], is the case of factor intensity reversals. A commodity, F, is capital intensive for a factor price ratio when it employs a higher capital/labor ratio than the other commodity, M. However, assuming constant-return production functions it is quite possible that F is capital intensive for some set of factor price ratios and labor intensive for some other set, so the capital/labor ratios employed in the two commodities must be equal for some factor price ratio. When the factor endowment ratios of the two countries lie on opposite sides of the common capital/labor ratio, there is no unique relation between factor endowment and commodity price ra-

* The author is associate professor in economics in Osaka City University. In preparing this article I have benefited by conversations with Professor Ronald W. Jones. I am also grateful to the referee of this paper for thoughtful criticisms and useful comments.

tios. If the Leontief paradox occurs because of factor intensity reversals between the two industries, factor prices can never be equalized between the two countries. On the other hand, I mentioned [14] that if the Leontief paradox occurs without assuming the existence of factor intensity reversals under the condition of the same production functions in the two countries, then factor price equalization must hold. This condition is equivalent to a sufficient condition for incomplete specialization. The law of factor price equalization proved by Samuelson [17] asserts that there is a continuous one-to-one correspondence between goods and factor prices when two different countries have the same production functions and produce all the commodities. Incomplete specialization is necessary for the law, and the incomplete specialization of both countries is possible (but not inevitable, for much depends on demand) if endowment ratios of different countries are very close. The Stolper-Samuelson theorem [19] asserts that if a price of a commodity rises, the real rental to the corresponding "intensive" factor will increase. Assuming no intensity reversals and identical production functions without specialization, the Stolper-Samuelson theorem implies factor price equalization because the former demonstrates the existence of a unique magnification effect of a rise in the commodity price on the corresponding intensive factor price.[1] The Rybczynski theorem [16] bears a dual relation to the Stolper-Samuelson theorem, as has been pointed out by Jones [11]. Rybczynski analyzes the effect on absolute levels of outputs of changes in the endowment of one factor at unchanged commodity prices; namely, that a rise in the endowment of one factor will bring about a more than proportionate rise in the output of the commodity which is intensive in the factor in question, while it will decrease the output of the other commodity at a constant commodity price ratio. On the other hand, the Heckscher-Ohlin theory of the nature of trade patterns revised by Jones [10] examines the ratio of outputs to be determined by ratios of factor endowments at a constant commodity price ratio. In other words, the Heckscher-Ohlin theory asserts the existence of a one-to-one correspondence between output ratios and the factor endowment ratios, while the Rybczynski effect asserts the existence of a special kind of relation between the factor endowments and outputs. Leontief [13] himself explains the "Leontief paradox" by differences in labor qualities in the two countries. Qualities of (physical) capital may differ in the two countries, too. Differences in labor and capital qualities may be theoretically translated into differences in production func-

[1] This is true even when the number of commodities and that of factors increases if we define the Stolper-Samuelson theorem in the strong form, i.e., all the nonintensive factor's real income to be decreased. See Chipman [3].

tions or in technology with the same qualities of labor and capital in the two countries. The traditional Heckscher-Ohlin theory assumes identical production functions in the two countries. But technological conditions may differ in the two countries. In these cases, commodity prices differ in the two countries at common factor prices and factor endowment ratios. In other words, a capital rich country may export labor intensive goods. The law of factor price equalization never holds in these cases because the real factor rewards must not be the same in the two countries. Only in a special case of technical progress in the two industries, the relative (but not real) factor prices are equalized after trade.

The paper falls into three parts, and the model used is the Heckscher-Ohlin type of two factors and two commodities based on the analysis of comparative statics. First, in order to isolate the core of the Heckscher-Ohlin theory of the nature of trade, I will assume that the two countries have the same production function in each industry and also have the same community taste patterns. Assuming no factor intensity reversals, the two countries differ only in their factor endowments. We will consider the effect on the transformation curve of changes in factor endowments. Second, I abandon the assumption of identical production functions in order to analyze the relation between technological changes and the transformation curve in the two-sector model. Finally, I will consider new conditions under which the Leontief paradox occurs and/or conditions for the Heckscher-Ohlin theory. In the process of economic growth, the trade pattern may change alternately between the two countries.

Thus our discussion will be done in the following order. The core of the Heckscher-Ohlin theory demonstrates the existence of a unique relation between the factor endowment ratio and the shape of the transformation curve assuming identical production functions in the two countries. The output ratio of the capital intensive commodity would be higher in the capital abundant country than in the labor abundant country at a common factor (and commodity) price ratio. When we relax the assumption of identical production functions in the two countries, the Leontief paradox is possible (not inevitable, for much depends on demand) if the price of the capital intensive commodity is cheaper in the labor abundant country than in the capital abundant country at a common factor price ratio. To close the production model I assume that community taste patterns are the same in the two countries. The relation of the closed economy commodity price ratios between the two countries depends upon demand conditions as well as factor endowment ratios and technological conditions. If the two countries have identical taste patterns, the Leontief paradox may occur by

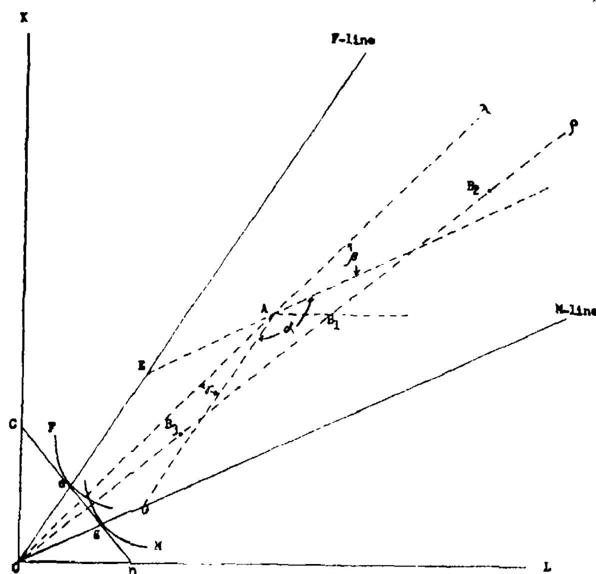


FIGURE 1

technological progress. Whether the Heckscher-Ohlin theory holds or not depends upon not only physical factor endowment ratios but also patterns of economic growth when the two countries have different production functions. We can easily relax the assumption of identical taste patterns in the two countries with a minor modification.

I. *Factor Endowments and Transformation Curves*

There are two commodities, manufactured goods, M, and food, F. The production function of each industry is identical and homogeneous of the first degree in the two countries, America, A, and Britain, B. Manufactured goods are assumed to be labor (L) intensive and food to be capital (K) intensive for any wage/rental ratio. Countries A and B have identical community taste patterns, but are endowed with different ratios of the two factors of production, L and K. We assume country A is endowed with a higher proportion of capital to labor than country B. That is,

$$(K/L)_A > (K/L)_B$$

Let us consider Figure 1. The two axes show respectively labor and capital, and unit isoquants, M and F, are drawn, where unit isoquants are loci of all possible combinations of two factors to produce one unit of each commodity. For a given factor price ratio of the slope of CD, the M-line and the F-line represent the factor intensities of the two

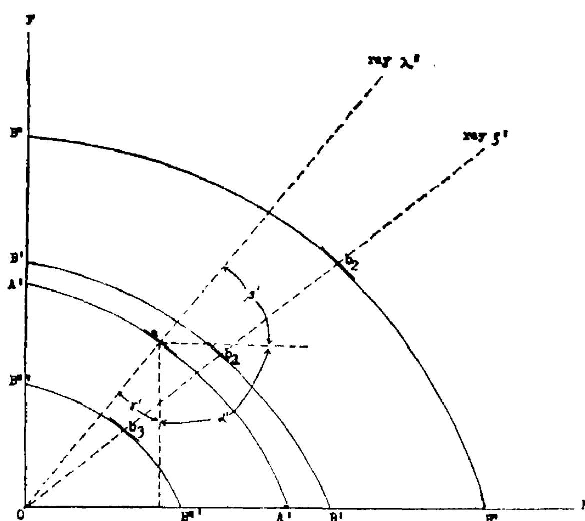


FIGURE 2

commodities at the competitive equilibrium situation. The point A represents country A's factor endowments, and the ray λ is country A's capital/labor endowment ratio. Assume country B's endowments lie at some point along the ray ρ. The outputs of manufactured goods and food are represented by the length of OG/Og and OE/Oe by drawing the lines from A parallel to the M-line and the F-line. By extending those parallel lines, the whole area below the ray λ can be divided into the three parts, which are represented by angles α, β and γ.

The Rybczynski theorem [16] asserts that, if country B's factor endowments lie at some point on the horizontal line from point A (for instance, B_1), country B produces more manufactured goods and less food than country A does at a constant factor price ratio. This theorem can be generalized as follows: if country B's factor endowments belong to any point inside the angle α, country B produces more manufactured goods and less food than country A does at a constant factor price ratio. But when country B's factor endowments lie in the angle β or γ, country B produces respectively more or less of both goods than country A does at a constant factor price ratio. Those relations can be seen by comparing the outputs corresponding to three points B_1, B_2, and B_3, all of which lie along the ray ρ. Those outputs are read off by completing the parallelograms at B_1, B_2, and B_3.

Figure 1 can be translated into Figure 2. Curve $A'A'$ represents country A's transformation curve, and the point a is the corresponding outputs of the point A in Figure 1. Ray λ' shows country A's output ratio (O_F/O_M) at the factor price ratio of slope CD of Figure 1. As

country A is endowed with a higher proportion of capital to labor than country B, country B's outputs will never lie at any point above the ray λ' at a constant factor price ratio. Furthermore, we can say that when country B's endowments lie at any point along the ray ρ of Figure 1, the corresponding output must be located at some point along the same ray, say, the ray ρ' of Figure 2. The slope of the ray ρ' must be smaller than that of the ray λ'. Notice that, assuming the three points b_1, b_2, and b_3 to be the corresponding outputs to points B_1, B_2, and B_3 of Figure 1 at a constant factor price ratio, country B's transformation curve would have respectively the same slope at those points, which is also the same as that of country A's transformation curve at the point a. They differ only in scale, and the ratio of $Ob_3 : Ob_1 : Ob_2$ of Figure 2 must be equal to that of $OB_3 : OB_1 : OB_2$ of Figure 1.

The shape of the transformation curve depends upon the technology and factor endowment ratios, while the scale of the transformation curve is determined by quantities of the two factors. We have three cases: (1) when country B's factor endowments lie at any point inside the angle α of Figure 1, the corresponding outputs of country B must lie at some point inside the angle α' of Figure 2; (2) when the former lie at any point inside the angle β of Figure 1, the latter must lie at some point inside the angle β' of Figure 2; and (3) when country B's factor endowments lie at any point inside the angle γ of Figure 1, the corresponding outputs must lie at some point inside the angle γ' of Figure 2 at a constant factor price ratio.

We have analyzed the relation between the factor endowments and the outputs at a constant factor price ratio. Notice that, whenever the factor endowment ratio is given, the shape of the transformation curve is uniquely determined independently of the scale of production, which depends upon the quantities of the two factors. There is a unique relation between the factor endowment ratio and the shape of the transformation curve. This is the core of the Heckscher-Ohlin theory.

II. *Technological Conditions and Transformation Curves*

We have mentioned that the Heckscher-Ohlin theory guaranteed a unique relation between the output ratio associated with the corresponding commodity prices and the factor endowment ratio at a given factor price ratio. It implies that the Heckscher-Ohlin theory is a dual of the theory of factor price equalization, which proves a unique relation between the factor and commodity price ratios at a given factor endowment ratio assuming incomplete specialization with identical production functions in the two countries. In this section, I will aban-

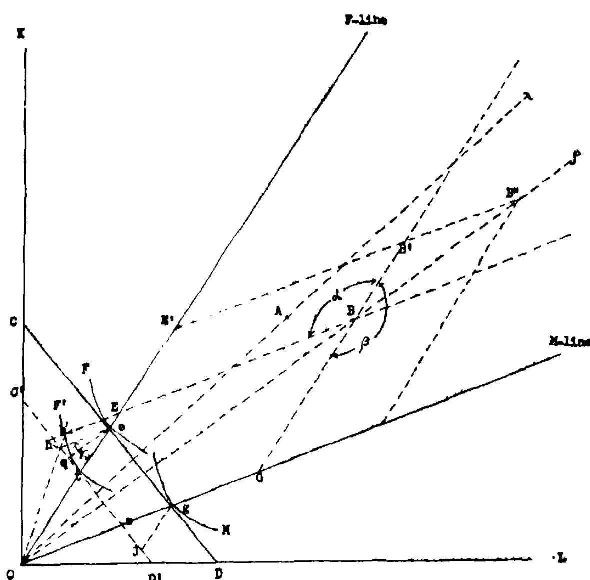


FIGURE 3

don the assumption of identical production functions in the two countries. That is, we will consider the effect on the transformation curve of technical progress in the two industries. Analysis of the effect of the economic expansion caused by technological progress may be pursued in two alternative ways. One is by analyzing new outputs that will result from changes of commodity price ratios caused by technical progress in the two industries at the initial factor price ratio. The other is by considering the effect on the outputs of economic expansion caused by technical progress in the two industries at a constant commodity price ratio. The latter approach may be applicable when the economy is small compared with the rest of the world, and the economic expansion does not affect the terms of trade. The second approach has been traditionally used (for instance [9]) since R. Findlay and H. Grubert [4] neatly developed the technique for dealing with biased technical progress, while in this section I will use the first approach.

Let us consider Figure 3, which is essentially the same as Figure 1. We assume as before that country A is endowed with a higher proportion of capital to labor than country B. Suppose technical progress occurs in country B's F industry. Technical progress can be expressed by an inward shift of the unit F-isoquant curve. There are several types

of technical progress: Hicksian neutral, capital saving, and labor saving. On the other hand, it has been said [2] that technical progress has two effects, "cost-reducing" and "factor-saving," though our definition slightly differs from them (see Jones [12, p. 569, n.21]).

1. When Hicksian neutral technical progress [7] occurs in the F industry, F isoquants are scaled down so that, for instance, the unit F isoquant shifts to F' which is tangent to line $C'D'$, and parallel to line CD, at the point t along the F-line. The relative price of F must fall at a constant factor price ratio (which is the "cost-reducing" effect). On the other hand, the output of F rises from OE/Oe to OE/Ot, and that of M does not change at a constant factor price ratio. That is, Hicksian neutral technical progress in the F industry has the same effect on outputs as would an increase of the factor endowments from B to a certain point B' along the ray GB at a constant factor price ratio. The point B' is obtained by taking a point E' along the F-line such that $OE/Ot = OE'/Oe$ and constructing a parallelogram $E'EBB'$ (which is the factor-saving effect).

2. The given extent[2] of Hicksian capital-saving technical progress in the F industry would be represented by an inward shift of the unit F-isoquant such that it is tangent to line $C'D'$, at some point below t. Hicksian capital-saving technical progress in the F industry has the same effect on outputs as would an increase of the factor endowments from B into cone α at a constant factor price ratio.

3. The given extent of Hicksian labor-saving technical progress in the F industry would be represented by an inward shift of the unit F isoquant so that it is tangent to the line $C'D'$ at some point above t. In this case, the output of M always rises but that of F may decrease at a constant factor price ratio in spite of technical progress in the F industry. That is, the "factor-saving" effect has the same effect on outputs as would a change of factor endowments into the cone β at a constant factor price ratio. The critical case is such that the labor-saving technical progress in the F industry makes the unit F isoquant shift so as to be tangent to the line $C'D'$ at the point h, which is obtained by drawing a line through the point e parallel to the M-line so the output of F does not change (note that $Oh'/Oh = OE/Oe$). When the new unit F-isoquant is tangent to line $C'D'$ above the point h, the output of F must decrease at a constant factor price ratio. If outputs of both commodities are to increase at a constant factor price ratio, technical progress must occur such that the unit F isoquant is tangent to $C'D'$ on the segment ht in the cone γ. As a special case of this, if the unit F isoquant is

[2] The extent of technical progress in an industry is defined by a percentage fall in the relative price of the commodity at a constant factor price ratio, assuming no technical change in the other industry.

tangent to line $C'D'$ at the point q, which is derived by drawing a line through the point e parallel to the vector of factor endowments OB, outputs of the two commodities increase proportionally at a constant factor price ratio. Thus Hicksian neutral technical progress in one industry does not imply the neutral expansion of factor endowments. Other interesting cases may be an inward shift of the unit F isoquant along the horizontal or the vertical lines through the point e. The "factor-saving" effects are the same as the initial case of the Rybczynski effect in those cases: namely, one of the factor endowments increases without any change of the other.

If there are identical taste patterns in the two countries, the Leontief paradox is possible when the price of the capital intensive commodity is cheaper in the labor abundant country than in the capital abundant country at a common factor price ratio. In this sense the relatively labor abundant country has a production bias in favor of the capital intensive commodity in the case of the Leontief paradox. On the other hand, if we relax the assumption of identical production functions in the two counstries, the Heckscher-Ohlin theory is possible when the price of the capital intensive commodity is cheaper in the capital abundant country than in the labor abundant country at a common factor price ratio. In this sense the relatively capital abundant country has a production bias in favor of the capital intensive commodity as in the traditional Heckscher-Ohlin theory.[3] However, whenever the technology differs in the two countries, the law of factor price equalization never holds, because real factor prices must be different in the two countries after trade.

We have analyzed the effect on outputs and commodity price ratios of technical progress in one industry at a constant factor price ratio. Suppose technical progress takes place also in the M industry. The new unit M isoquant may (i) still lie outside line $C'D'$, (ii) be tangent to line $C'D'$, or (iii) cut line $C'D'$. In case (i) the relative price of F must decrease at a constant factor price ratio. In case (ii) it does not change, and in case (iii) it must rise at a constant factor price ratio. Clearly it is only in case (iii) that the Heckscher-Ohlin theory of trade patterns holds at a common factor price ratio in the two countries. Furthermore for each case there are Hicksian neutral, capital-saving, and labor-saving technical progress in the M industry too. There are several combinations of technical progress in the two industries.

[3] In the above examples, the price of the capital intensive commodity, F, is cheaper in the labor abundant country, B, than in the capital abundant country, A, at a common factor price ratio. On the other other hand, if country B were endowed with a higher proportion of capital to labor than country A, the Heckscher-Ohlin theory would still hold.

Let us consider case (ii). It is only in case (ii) that both goods can continue to be produced at a constant factor price ratio without any change in the commodity price ratio. That is, the "cost-reducing" effects have cancelled each other out in the two industries so that the relative commodity price does not change. This case may be called the "neutral industry effect."[4] Similarly cases (i) and (iii) may be called respectively F- and M-industry biased technical progress. The "differential industry effect"[5] depends only upon the extent of relative technical progress in the two industries. When it is neutral, the relation between commodity and factor price ratios is the same as it would be in the case of no technical progress.

But changes in outputs depend not only upon the extent but also upon the nature of technical progress. Assume a given extent of Hicksian neutral technical progress in the F industry. (1) One critical case is such that the new unit M isoquant shifts to the point j derived by drawing a line gj parallel to the F-line.[6] Outputs of M must decrease when the unit M isoquant shifts to any point below the point j at a constant factor price ratio in spite of technical progress. The "factor-saving" effect is represented by a change in factor endowments into the cone α. If outputs of both commodities are to increase at a constant factor price ratio, technical progress must occur such that the new unit M isoquant shifts to a point above j. (2) When the new unit M isoquant is tangent to line $C'D'$ at the point s along the M-line, the technical progress is Hicksian neutral. In this case, outputs of both commodities increase proportionally at a constant factor price ratio without any change of commodity prices. The "factor-saving" effect can be represented by a shift of factor endowments from B to B'' which is located along the ray OB. The transformation curve expands its scale with the same degree in the two industries, as in Figure 2. Notice that Hicksian neutral technological progress in every industry does not necessarily increase the outputs of all the commodities by the same proportion at a constant commodity price ratio unless the two industries have the same degree of technical progress. That is, the economic expansion is neutral only when the new unit M isoquant is scaled down to the point s on the line $C'D'$. On the other hand, when it shifts down to some point above (below) the point s on the line $C'D'$, the "scale-line" which is the loci of the economic expansion at a constant commodity price ratio is M-biased (F-biased).

[4] The "neutral industry effect" seems to be essentially the same as Leontief's "comparative technological parity" [13, p. 344], at least marginally.

[5] The name has been given first by Jones [12, p. 569]. See also Amano [1, p. 393].

[6] The point j corresponds to the point h in the F industry.

Notice that, when the "differential industry effect" is neutral, the effect on outputs of technical progress is the same as in the case of changes in factor endowments, which has been discussed in section I. When Hicksian neutral technical progress occurs in the F industry and the differential industry effect is neutral, the economic expansion (or the scale of the economy) is neutral, M-biased, or F-biased depending on respectively Hicksian neutral, labor-saving, and capital-saving technical progress in the M industry. However, whenever Hicksian neutral technical progress occurs to the same degree in the two industries, the modified factor price equalization theory holds in the sense that the relative (but not real) factor prices must be the same in the two countries after trade (provided that specialization is incomplete). This is because the relation of product price ratios and ratios of factor reward is completely the same in these cases as it would be in the case of identical production functions in the two countries.

So far, we assumed that the degree of technical progress was the same in the two industries [case (ii)]. We can analyze similarly cases of biased industrial technological progress in the two industries [case (i) and (ii)], which in turn will be combined respectively with Hicksian neutral and biased technical progress in each industry. We still assume Hicksian neutral technical progress in the F industry.

1. When the differential industry effect is M-biased, the scale of the economy will expand in favor of M (that is, M-biased economic expansion) even in the case of Hicksian neutral technical progress in the M industry too. This is true also in the case of Hicksian labor-saving technical progress in the M industry. But when technical progress is capital saving in the M industry, the economic expansion might be F-biased.

2. When the differential industry effect is F-biased, the economic expansion is F-biased even in the case of Hicksian neutral technical progress in the two industries. This is true also in the case of Hicksian capital-saving technical progress in the M industry. When technical progress is Hicksian labor-saving in the M industry, the economic expansion might be F-biased. Whenever the differential industry effect is not neutral, the "cost-reducing effect" appears explicitly so that factor price equalization never holds even in the modified form.

III. *The Heckscher-Ohlin Theorem and the Leontief Paradox*

To close the production model, I assume that community indifference curves are the same in the two countries in order to clarify our argument.[7] On the other hand, Gorman [5] has proved that there is a unique set of community indifference curves if, and only if,

[7] I will relax the assumption later.

the Engel curves for different individuals at the same prices are parallel straight lines. Along the Engel curves the direction normal to the corresponding personal indifference locus will be parallel to the fixed direction.[8] Clearly tastes of different individuals may be different, but an extra unit of purchasing power should be spent in the same way no matter to whom it is given under the necessary and sufficient condition quoted above.

The shape of the transformation curve is determined by the technology as well as the factor endowment ratio. When technological conditions are given in the two industries, the shape of the transformation curve depends on the factor endowment ratio. On the other hand, when the factor endowment ratio is given, the shape of the transformation curve is determined by the nature of technical change Hicksian neutral, capital-saving, and labor-saving technical progress under the condition of the given differential industry effect of technical progress. The differential industry effect which is determined by the relative extent of technical progress in the two industries will change the shape of the transformation curve so that it pulls out the transformation curve in a certain direction. For instance, when Hicksian neutral technical progress occurs only in the M industry (F industry), the transformation curve expands to the horizontal (vertical) direction in Figure 2. If Hicksian neutral technical progress occurs to the same degree in the two industries, the transformation curve is scaled out to the same degree in all directions.

Furthermore, notice that if the shape of the transformation curve is given, the closed economy commodity price ratios depend on the scale of the economy as well as on demand conditions whenever community taste patterns are not homothetic. In Figure 4, the straight line, OaT, which is an Engel curve, is derived from homothetic community indifference curves. In this case, the income elasticity of demand is unity. If the Engel curve is RaT', the income elasticity of demand for manufactured goods is less than unity; the demand expansion is biased against manufactured goods. If it is aQT'', the income elasticity of demand for manufactured goods is greater than unity. Suppose the shapes of the two countries' transformation curves are the same, but country B's scale of the economy is greater than country A's by neutral industrial technical progress with Hicksian neutral technological changes in the two industries. The community indifference curves are assumed to be the same in the two industries, so it is easy to demonstrate that (1) if the income elasticity of demand for manufactured goods is less than unity, country B's relative price of manufactured

[8] One must be certain that commodities are consumed in positive amounts, otherwise Gorman's criterion would be wrong. See Samuelson [18, p. 5, footnote 2].

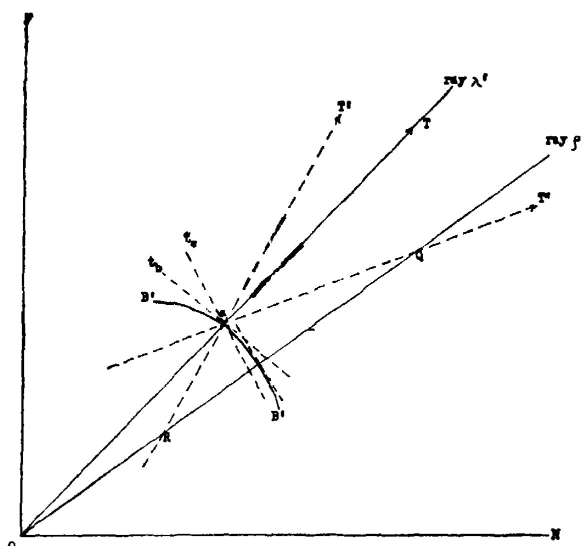


FIGURE 4

goods is less than country A's, (2) if the income elasticity of demand for manufactured goods is greater than unity, country A's relative price of manufactured goods is less than country B's, and (3) if the identical community taste patterns are homothetic, both countries are at equilibrium without any trade.

Let us return to the Heckscher-Ohlin case, where the factor endowment ratios differ between the two countries. In Figure 4, the point a is assumed to be country A's initial closed economy equilibrium point. The curve $B'B'$ is the initial transformation curve of country B. The slopes of lines t_a and t_b are the two countries' closed economy commodity price ratios, with the output ratio of the slope of the ray λ' assuming the same production at the point a in the two countries. The rays λ' and ρ' are respectively country A's and country B's economic expansion paths with the commodity price ratio of the slope of line t_a when Hicksian neutral technical progress occurs to the same degree in the two industries. From Figure 4 some simple but important propositions become clear.

First, if the Engel curve rises at a slope less than the ray ρ', such as a QT'', it must intersect country B's production expansion path ρ' at some point Q. Country B's new transformation curve may (i) cut the ray ρ' below the point Q, (ii) cut it at the point Q, or (iii) cut it above the point Q, depending upon the scale of country B's economy caused by technical progress. In case (i), the closed economy product price ratio, P_F/P_M, is lower in country A than in country B so that the Heck-

scher-Ohlin theory must hold. In case (ii) they are equivalent so that there is no trade between the two countries even though the factor endowment ratios and the technology differ between them. In case (iii), the capital abundant country, A, will export the labor intensive commodity, M, in exchange for the import of the labor intensive commodity, F. Clearly, the Leontief paradox occurs under these assumptions if, and only if, the scale of country B's economy caused by technical progress is large enough compared with country A's, so that the corresponding transformation curve cuts the ray ρ' above the point Q. When the Engel curve rises at a slope greater than the ρ' but less than the ray λ' so that there is no intersection of the Engel curve and country B's production expansion path, then the Heckscher-Ohlin theory must always hold.

Secondly, if the Engel curve rises at a slope greater than the ray λ' such as RaT', it must intersect country B's production expansion ray, ρ' at some point R. Country B's transformation curve may (i) cut the ray ρ' below the point R, (ii) cut it at the point R, or (iii) cut it above the point R. In case (i), the closed economy price ratio, P_F/P_M, is higher in country A than in country B, so that the Leontief paradox must hold. That is, under these assumptions the Leontief paradox holds if and only if the scale of country B's economy is small enough compared with country A's, so that the corresponding transformation curve cuts the ray ρ' below the point R. In case (ii) there is no trade between the two countries. Clearly, the Heckscher-Ohlin theory must hold in case (iii).

Thirdly, if the demand expansion curve is a straight line, such as OaT (that is, the demand pattern is homothetic), it never intersects the ray ρ', so that the Heckscher-Ohlin theory must hold.[9]

The closed economy factor price ratios in the two countries are obtained from the Stolper-Samuelson theorem [19] when the same degree of Hicksian technical progress occurs in the two industries in one country.[10] In all cases in which the Heckscher-Ohlin theory of the trade pattern holds, the relationships between commodity and factor price ratios in the two countries are represented by:

$$\left(\frac{P_F}{P_M}\right)_A < \left(\frac{P_F}{P_M}\right)_B \quad \text{implies} \quad \left(\frac{r}{w}\right)_A < \left(\frac{r}{w}\right)_B.$$

[9] The third case was pointed out by Jones [10, p. 4, footnote 1].

[10] Notice that I modify here the meaning of the Stolper-Samuelson theorem. The original Stolper-Samuelson theorem asserts that, by the imposition of an import duty, the real reward of whichever factor is employed intensively in the import-competing industry rises, and the real reward of the other factor contracts. But I use the theorem in terms of the relative reward to each factor.

On the other hand, in all the cases in which the Leontief paradox holds, these relations will be given by:

$$\left(\frac{P_F}{P_M}\right)_A > \left(\frac{P_F}{P_M}\right)_B \quad \text{implies} \quad \left(\frac{r}{w}\right)_A > \left(\frac{r}{w}\right)_B .$$

The reverse relation is also true in both cases. Each country exports always a commodity which is produced by using intensively a cheaper factor compared with the other country. In other words, if factor abundance is defined by the pre-trade ratios of factor prices between

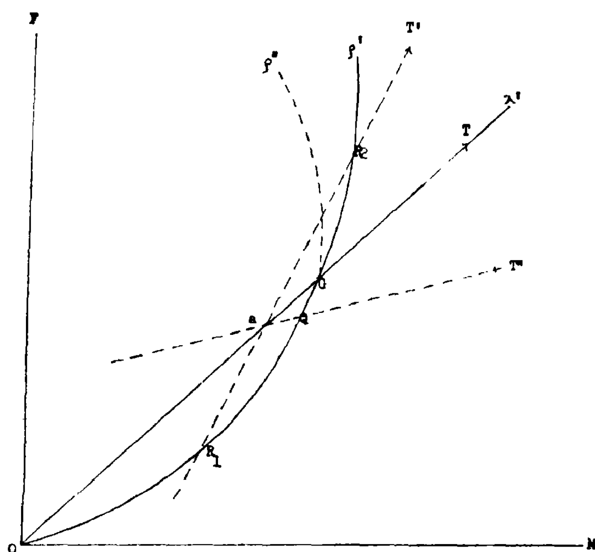


FIGURE 5

the two countries as Heckscher did, the Heckscher-Ohlin theory always holds.

Now I abandon the assumption of the same extent of Hicksian neutral technical progress in both industries in country B, assuming still no technical change in country A. The crucial modification of this model is that country B's production expansion path, ρ', becomes a curve instead of a straight line. In Figure 5 the curve OGR_2 (or the curve ρ') shows country B's production expansion path which rises at an increasing rate. The shape of country B's production expansion curve may be determined as follows: for instance, when the same degree of Hicksian neutral and capital-saving technical progress occurs respectively in the F- and the M-industry, the transformation curve will expand such that the output ratio, O_F/O_M, rises at a constant commodity price ratio. If technical progress is continuous and Hicksian

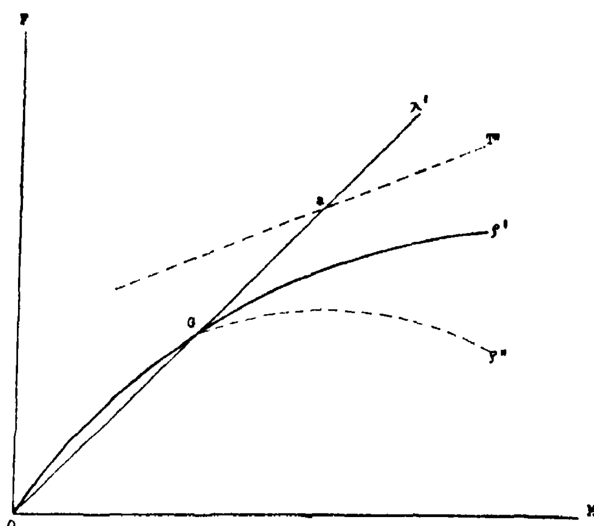


FIGURE 6

capital-saving technical progress in the M industry is such that the unit M isoquant curve shifts to a point above the point j of Figure 3, the production expansion curve ρ' is rising at an increasing rate and does not bend back. If the unit M isoquant curve shifts to a point below j of Figure 3, it bends back like line ρ''. On the other hand, the dotted lines R_1aT' and aQT'' show respectively inelastic and elastic income demand expansion curves for manufactured goods. Of course, the line OaT shows the homothetic demand taste pattern.

When country B's production expansion curve rises at an increasing rate, it may cut country A's production expansion line, the ray λ', which would be caused by neutral increment of the two factors, say, at the point G. In this case even if the community indifference curves are homothetic (and the demand expansion curve is represented by the ray OaT), the trade pattern changes after country B's transformation curve cuts the ray at the point G, and the Leontief paradox occurs. But when the income elasticity of demand for manufactured goods is less than unity, such as shown by the line R_1aT', it may cut the production expansion curve more than once, say, at R_1 and R_2. In this case the meaning of the point G differs from before. The pattern of trade does not change on both sides of the point G, and the Heckscher-Ohlin theory of the trade pattern holds if, and only if, country B's transformation curve cuts the curve ρ' at some point inside of the range R_1 and R_2. On the other hand, the Leontief paradox occurs if, and only if, country B's transformation curve cuts curve ρ' at some point below R_1, or above R_2. In this example, the pattern of trade between the two

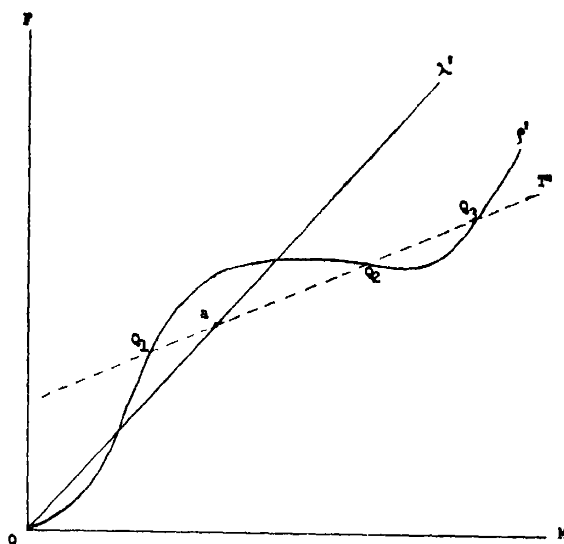


FIGURE 7

countries changes three times, and when country B's transformation curve cuts the curve ρ' at R_1 or R_2, there is no trade. This is true even if the production expansion curve bends back, such as curve ρ''. If the income elasticity of demand for manufactured goods is greater than unity, such as shown by the line aQT'', it must intersect the production expansion curve ρ' (or ρ''), say, at the point Q. The point Q is an equilibrium point with no trade between the two countries. The Heckscher-Ohlin theory holds if, and only if, country B's transformation curve cuts the curve ρ' (or ρ'') at some point below Q, while the Leontief paradox holds if, and only if, country B's transformation curve cuts the curve ρ' (or ρ'') at some point above Q.

Of course, these are not all the possible cases. Many other configurations are a priori possible. For example, Figure 6 shows a case in which the demand elasticity for manufactured goods is greater than unity, which is shown by curve aT'', and country B's production expansion curve is represented by ρ' or ρ''. There is no intersection between the demand and supply expansion curves so that the trade pattern does not change in the whole area. The Heckscher-Ohlin theory always holds in this situation in spite of different production functions in the two countries. On the other hand, in Figure 7 country B's production expansion curve, ρ', fluctuates so that it intersects with the demand expansion line, aT'', several times, that is, at Q_1, Q_2, and Q_3. In this case, the trade pattern changes alternately at those points between the two countries. The Heckscher-Ohlin theory holds at the ranges OQ_1 and Q_2Q_3,

while the Leontief paradox occurs between Q_1 and Q_2, and also above the point Q_3.

We have demonstrated that the Leontief paradox may occur if we assume identical taste patterns in the two countries. We have obtained also sufficient conditions for the Heckscher-Ohlin theory of the nature of trade pattern when we abandon the assumption of the same production functions in the two countries. Finally, we can easily relax the assumption of identical taste patterns in the two countries. When country B's taste pattern differs from country A's, country B's Engel curve which represents the same commodity price ratio as country A's closed economy commodity price ratio does not pass through the point a which is assumed to be located on the ray λ' (in Figures 4 to 7). However, notice that the intersection points between country B's production expansion curve and its Engel curve mentioned above are crucial in our discussion. Whether the Engel curve of country B passes through country A's closed economy equilibrium point a or not is not necessarily important to consider changes in the trade pattern between the two countries if no technological change occurs in country A. Therefore, all the conclusions do not essentially change if the two countries have different taste patterns.

REFERENCES

1. A. AMANO, "Determinants of Comparative Costs: A Theoretical Approach," *Oxford Econ. Papers*, n.s. Nov. 1964, *16*, 389-400.
2. J. BHAGWATI AND H. G. JOHNSON, "Notes on Some Controversies in the Theory of International Trade," *Econ. Jour.*, March 1960, *70*, 74-93.
3. J. S. CHIPMAN, "Factor Price Equalization and the Stolper-Samuelson Theorem," (abstract), *Econometrica*, Oct. 1964, *32*, 682-83.
4. R. FINDLAY AND H. GRUBERT, "Factor Intensities, Technological Progress and the Terms of Trade," *Oxford Econ. Papers*, n.s. Feb. 1959, *11*, 111-121.
5. W. M. GORMAN, "Community Preference Fields," *Econometrica*, Jan. 1953, *21*, 63-80.
6. E. HECKSCHER, "The Effect of Foreign Trade on the Distribution of Income," *Ekonomisk Tidskrift*, 1919, *21*, 1-32, reprinted in *Readings in the Theory of International Trade*, Philadelphia 1949, pp. 272-300.
7. J. R. HICKS, *The Theory of Wages*. London 1935.
8. H. G. JOHNSON, *International Trade and Economic Growth*, London, 1958, Ch. 1, pp. 17-30.
9. ———, *Money, Trade and Economic Growth—Survey Lectures in Economic Theory*, London 1962, Ch. 4.
10. R. W. JONES, "Factor Proportions and the Heckscher-Ohlin Theorem," *Rev. Econ. Stud.* 1956, *24*, pp. 1-10.

11. ———, "Duality in International Trade: A Geometrical Note," *Can. Jour. Econ.*, Aug. 1965, *31*, 390-93.
12. ———, "The Structure of Simple General Equilibrium Models," 1965, *Jour. Pol. Econ.*, Dec. 1965, *73*, 557-72.
13. W. LEONTIEF, "Domestic Production and Foreign Trade: the American Capital Position Re-examined," *The Proceedings of the American Philosophical Society*, Sept. 1953, *97*, 331-49.
14. N. MINABE, *Essays in the Theory of International Trade*, Ph.D. Thesis, The University of Rochester, June 1965, Ch. 1, 1-12.
15. B. OHLIN, *Interregional and International Trade*. Cambridge, Mass. 1933.
16. T. M. RYBCZYNSKI, "Factor Endowments and Relative Commodity Prices," *Economica*, n.s. Nov. 1955, *22*, 336-41.
17. P. A. SAMUELSON, "International Factor-Price Equalization Once Again," *Econ. Jour.*, June 1949, *59*, 163-84.
18. ———, "Social Indifference Curves," *Quart. Jour. Econ.*, Feb. 1956, *70*, 1-22.
19. W. F. STOLPER AND P. A. SAMUELSON, "Protection and Real Wages," *Rev. Econ. Stud.*, Nov. 1941, *9*, 58-73.

COMMUNICATIONS

A New Approach to International Liquidity*

Almost a quarter century ago, Professor Abba Lerner advocated a revolutionary new doctrine he named functional finance. The gist of it was that public spending and taxation should be governed exclusively by the goals of full employment and price stability, without regard to whether the budget was balanced. At the time, this doctrine looked like Lerner at his wildest—today it is fiscal orthodoxy not only in the classroom but in many treasuries and ministries of finance as well.

The idea can be extended into the international sphere. There is a lively discussion going on today of whether or not the supply of international liquidity is adequate; and if not, what would constitute an adequate level. There is much disagreement and little hope of its ever being resolved. All parties to the argument, however, seem to agree on one thing: if the supply of liquidity were to be brought to what is considered an adequate level, all would wish to keep it stable at that level, or, at the most, increase it at an annual rate of three per cent—which is the limit to which respectable economists are willing to stretch the meaning of the word stable, as a concession to the realities of our expanding universe. The reason why the warring factions can agree on this one point is that they all believe in the desirability of balancing, in the long run, the balance of payments. Disagreement on how large a supply of liquidity is adequate is disagreement on how long the long run should be. All economists seem to agree that in the long run, however defined, each country should maintain balance-of-payments equilibrium.

Let me point out first of all that this is a moral doctrine. Our moral conscience tells us that it is not *right* for anyone or any country to get something for nothing—although that, precisely, is what an indefinite balance of payments deficit implies. If the deficit is on the current account of the balance of payments, there is the further connotation that the country lives beyond its means and spends more than it earns, with all the moral disapprobation that this calls forth. It is all right to run a deficit temporarily, in an emergency, and as long as one's reserves allow; but people consider it morally imperative that these reserves be finite and either of the kind that have been earned beforehand or of the kind that must be repaid afterwards. This is the puritan ethic of international trade. What is wrong with it?

What is wrong with it, I think, is that it is ethics rather than economics. Not only is it perfectly good economics occasionally to get something for nothing; it is or should be one of the economist's main preoccupations. It is, indeed, the subject matter of welfare economics. To move from a non-optimal to a Pareto-optimum situation is the art of getting something for nothing. A

* I am indebted to Professors Alvin H. Hansen and Abba P. Lerner for reading the manuscript and making many valuable suggestions.

more pertinent example of this art is public works to relieve unemployment, since these create something, a bridge, or a post office, or tidily raked leaves, whose social cost is nothing or less than nothing, zero or negative.

If we must have a moral doctrine to govern our economic affairs, a much better one would be that no country be required to give up anything without receiving something, an adequate *quid pro quo* in exchange. This is a much less restrictive rule, more acceptable also to the economist; and we shall explore its implications in the following.

The economist has no business to accept unquestioningly and to regard as self-evident dogma the desirability of long-run equilibrium in the balance of payments. There are times and situations when payments balance is, indeed, desirable; but its desirability must be established by economic argument and not treated as a premise. The functional approach to international finance is to forget about payments equilibrium in the long as well as in the short run and to ask instead whether it might not be possible to let each country have the kind of balance of payments that best serves its domestic goals whatever these may be: price stability, fast growth, full employment, or some combination or compromise of these.

The reason, I suspect, why no one so far has bothered to ask this question is that the answer obviously is no. It is obviously not possible to let every country have the balance of payments it needs or wants. The developing countries for example would probably show an insatiable appetite for payments deficits; and even according to the conservative estimate of a banker, Mr. Woods, president of the International Bank for Reconstruction and Development, they could absorb another $4.5 billion of unrequited imports per annum. But if it is impossible to satisfy every country, might there not be a compromise? There are deficit countries, such as the United States today, that make insufficient use even of their own resources and certainly do not need, and do not want, unrequited resources from abroad. And there are surplus countries that are happy with their surpluses, or at least not unhappy enough to try to eliminate them. Could not a system of international payments be devised to accommodate at least these countries? And if, in the process, some of the developing countries' desire for unrequited imports can also be satisfied, so much the better.

A plan to achieve this will be presented in the following. It will call for the creation by a reformed International Monetary Fund of international paper money, to be made available to developed countries with unemployed resources against their national currencies to be held as security by the IMF, *and against* real resources to be given as grants to developing countries. But before discussing the detail of this plan, we shall first state its principles and show how they follow from the requirements of functional finance and the new moral doctrine enunciated on the preceding page. It is also worth noting here that the need for some such plan was first stated by a very practical and policy-oriented expert in the field.

Richard Cooper has argued, in contrast to most international-trade theorists, against the devaluation of the dollar (cf. [1]). He was afraid that this

might restore balance-of-payments equilibrium, which, he said, is not what we want—certainly not what we need and ought to want. His argument was that, over the past 15 years, the Western European countries (not to mention Japan) had a very fast rate of growth, unprecedented in Europe; and that this had much to do with their balance-of-payments surpluses, since it was based on export-oriented expansion, which in these countries is more feasible than the expansion of domestic markets, which inevitably increases competition at home. Their payments surpluses in turn were made possible by the persistent payments deficit of the United States, which she did not seem to mind as long as the outside world did not object, and which did not cramp her style of growth either during most of this time. Dollar devaluation might bring to an end this perfectly satisfactory state of affairs by worsening (eliminating the surplus in) the European and Japanese balances of payments. He hoped that we would be able to do better than that—devise some means whereby the Europeans and Japanese could continue their surpluses, we our deficits, and all of us our fast rates of growth.

Let us now try to devise such a system. Deficit countries should certainly not be put under pressure to improve their balances of payments by means of restrictive policies that would lower either the level or the efficiency of employment of their resource capacity, and so inflict a net loss on the world as a whole. Instead, additional international reserves should be created to accommodate these countries, but made available to them only in exchange for goods and services. To exact payment in products for the newly created reserves is to guarantee that they will be claimed, created and used only when, and only to the extent that unemployment or inefficiency exists in the recipient countries or would be created if such reserves were not available. For the social cost of the products paid for the new reserves is zero or less than zero at times of underemployment (or when the alternative to producing them is to create underemployment); whereas it is quite high and likely to be considered prohibitive at times when their production would create or aggravate inflationary pressures. At such times therefore the deficit countries would fear the inflationary pressures of the payment exacted for the new reserves; and, rather than obtain these, they would choose to eliminate the deficit through restrictive fiscal and monetary policies, more appropriate also to the domestic needs of the economy.

The products surrendered in exchange for the newly created reserves would be available to fill part of the development needs of the developing countries; and their composition would have to be suitable for this purpose—a restriction that will be discussed further.

The creation of additional liquidity, and its injection into the system through deficit countries short of reserves, would enable surplus countries to continue their surpluses if they wished to. At the same time, they could also eliminate their surpluses—and simultaneously slow or prevent the creation of additional liquidity—if they found either their surpluses or their reserves excessive. A surplus country can always eliminate its surplus if it wants to (and much more easily than a deficit country can eliminate its deficit), by exchange

revaluation, by temporarily liberalizing imports, by special taxes on foreign tourists or foreign capital, to mention just the more agreeable weapons in the surplus country's armory. By eliminating their surpluses, the surplus countries eliminate also the deficit countries' deficits and thereby prevent their needing, and causing the creation of additional reserves.

Under this plan therefore new reserves could be created only when there are countries with surpluses and deficit countries with unemployed resources. (See, however, p. 1217 below for an exception.) Since the new reserves would be made available primarily to deficit countries and only against products, these countries' domestic and external troubles would be relieved at the same time. The products, obtained against and representing the real counterpart of the newly created reserves, become the unrequited imports of the developing countries, which can always use the right kind of imports from industrial countries with benefit to their economies.

The one seemingly arbitrary element in this scheme is the transfer of products from deficit to developing countries; and the equality of their value to that of the new reserves created (they could just as well be half or twice as great in value).

This latter, however, also has its justification. To provide a country with new reserves and thereby allow it to run a deficit without drawing down either previously earned reserves or subsequently repayable credits amounts to giving that country something for nothing. This can be done without a corresponding loss to other countries only when its acquisition of these new reserves goes hand in hand with the production of output that would not be produced otherwise. The best way to assure this is to require the country that wants additional reserves to surrender in exchange products at least equal in value; and it is a matter of equity to make these available to those most in need.

The deficit country, however, which obtains the new reserves, is enabled to raise its output (or avoid restricting it) by an amount much larger than its new reserves. For its new reserves enable it to make foreign payments in equal amount; and these would equal only the additional imports resulting from the much larger increase in aggregate income and output.

The difference between the additional output and that part of it which is surrendered in exchange for the reserves obtained resembles a Marshallian surplus and is a net gain to the deficit country. Hence the question whether this gain should be allowed to accrue to the deficit country or whether instead it should be transferred, wholly or partly, to the developing countries? In other words, should not $1 million of newly created reserves be sold at a price higher than $1 million worth of products? To answer this question it is best to ask first what would happen if the country in need of reserves would not be able to obtain them. It would then have to curtail its foreign payments by a like amount, which it could do by restricting aggregate output by a larger amount: larger in the ratio of the reciprocal of its marginal propensity to import. If this were the only way to curtail foreign payments, additional reserves that made the curtailment unnecessary would be worth more in terms of out-

put than their nominal value. Nevertheless, the recipient country should not be made to pay more than their nominal value for the new reserves, because there are also other and usually cheaper ways of curtailing foreign payments: trade restriction and administrative controls.

When, for lack of reserves, a country curtails its imports through duties, quotas, or administrative controls, the consequent loss is the value of the imports foregone, plus the nuisance value and inefficiency such measures entail. These latter costs, though real, are intangible and difficult to measure; and they may well be offset by the beneficial side effects of import restriction: the stimulus to and protection of domestic production. As a first approximation therefore, the loss created by import restriction may be said to be no more or not much more than the value of the imports foregone. Here, then, is the justification for selling newly created reserves to the country in need neither for more nor for less than the nominal value of the reserves.

Another argument for exacting payment in products for newly created reserves is that it maintains the deficit countries' balance-of-payments discipline for the occasions when this is necessary. It may seem paradoxical to argue for balance-of-payments discipline and adjustment and for reserve creation at the same time. After all, the latter's purpose is to avoid the need for adjustment. Payments adjustment, however, must not and cannot always be avoided; and an ideal system of international payments will both minimize the need for adjustment and encourage and facilitate it when adjustment is required.

This combination of trying both to avoid adjustment when unnecessary and to encourage it when called for is the Scylla and Charybdis on which past reforms have foundered. It seems very hard to provide reserves or extend credit to a deficit country without weakening its will to make payments adjustment in the future. Conditional credits seemed at one stage the solution to the dilemma; but they too proved a broken reed. Yet, this will must be preserved in a world where payments adjustment is often necessary, always painful, and where the pain can only be mitigated by dividing the burden of adjustment between deficit and surplus countries.

It is true that the deficit countries have for many years carried the main burden of adjustment, which makes one wonder if concern to preserve their will to adjust is not misplaced or exaggerated. But the reason why they did most of the adjusting in the past was the world's deflationary bias, with more unemployment in the world than inflationary pressure. If new reserves are created as here proposed, they will not only relieve many deficits but eliminate much of the deflationary bias as well. In fact, my proposal may be regarded as an extension of Keynesian policy into the international sphere. While Keynes wanted to increase the supply of national moneys in order to lower interest rates and so stimulate investment and employment, I am proposing to increase the supply of international reserves in order to eliminate restrictive national policies when these lead, or threaten to lead to unemployment.

If there were no deflationary bias in the world, the surplus countries would be under much more pressure to eliminate their surpluses; and for them to bear the main burden of adjustment in an inflationary world would be just as bad as its opposite. This is why the deficit countries must be made to earn

their reserves and pay for them in products. The social cost of these, and hence of the reserves, depends on the state of the economy; economists as well as the general public are likely to consider this cost prohibitive in inflationary times but worth paying and even beneficial in all others.

Accordingly, the scheme as here outlined would enable deficit countries to obtain newly created reserves when there is unemployment but force them to make payments adjustment when inflation threatens. It would also put indirect pressure on countries with payments surpluses and inflationary pressures to take the initiative in eliminating both.

This raises two questions. First, should not also surplus countries with unemployed resources be allowed to obtain additional reserves on the terms here outlined and so enabled to raise employment levels? Second, is it right to make newly created reserves available to countries with deficits and unemployment at times when these are due to an overvalued currency that has priced the country out of world markets?

The answer to the first question is yes. If a surplus country prefers this way of raising employment or considers its reserves so small that it wants to add to them faster than its surplus allows, there can be no objection to its buying more reserves against products if it can do so without harming other countries and even while benefiting some. This case may be quite important, because developed countries will find it easier to maintain employment by extending foreign aid than by stimulating domestic investment whenever they lack the amount and type of labor needed to man the new equipment that additional domestic investment would create (cf. [2]).

The second question is tricky. One might argue that additional reserves should be made available only to countries in temporary payments difficulties; whereas a permanent payments disequilibrium, and especially one due to misalignment of price levels, should be remedied by exchange-rate readjustment. The principle of functional finance, however, may well give a different answer.

An overvalued currency often leads to both underemployment and payments difficulties; but the country may nevertheless be reluctant to devalue its currency, for fear not only of losing face but also of worsening its terms of trade. Here is a real cost of exchange-rate readjustment, which must be weighed against the cost of producing the goods that would buy the new reserves needed if exchange rates were kept unchanged. This, we argued earlier, involves no social cost at a time of underemployment—at least not in the short run. In the long run, other ways of raising the level of employment must also be considered; and, weighed against such alternatives, the opportunity cost of buying reserves with development aid is quite likely to be positive. Whether this cost will be greater or smaller than the cost of worsened terms of trade cannot be determined a priori; there might well be situations in which the country would prefer to keep its currency overvalued indefinitely and finance the resulting payments deficit out of newly created reserves paid for by products. There remains the question how the outside world would view such preference and behavior on the part of the deficit country.

Other developed countries would probably accept and even prefer it. For

they usually regard a fellow developed country not so much as a market in which to buy as one in which to sell and as a competitor in third markets. They are quite likely therefore to live happily with its currency overvalued. As to the developing countries, as a group they would gain either way, perhaps more from development aid than from a slight improvement in their terms of trade.

Having presented the bare bones of the plan and its economic justification, there remains to be added its monetary mechanism, the money veil so to speak. There are many different ways, of course, in which such a plan can be implemented; the following is just one example among many, chosen primarily for simplicity of exposition.

Credit instruments (owned reserves) are preferable to credit, which is why the newly created reserves should be instruments of indebtedness, international paper money, issued by the IMF against the security of member countries' currencies. These Fund Obligations could be created only on the initiative of the countries desirous of obtaining them, though preferably subject to approval by the Fund. If a deficit country, such as the United States, wished to obtain, say, $1 billion additional reserves, her government would have to have the corresponding appropriation in its budget, so as to be able to hand over to the Fund $1 billion in U.S. currency. This would also have to carry a gold guaranty. Against the security of the U. S. currency, the Fund would issue $1 billion worth of Fund Obligations, which would ultimately become part of the U.S. external reserve, acceptable and usable as equivalent to gold by all member countries of the Fund. (To assure this, central banks would have to be required to hold Fund Obligations in prescribed minimum proportions to other reserves.) At first, however, the Fund Obligations would be restricted in use and handed over, not to the United States, but to the International Development Association. The IDA would use them to finance investment projects of developing countries approved by the IBRD's regular screening machinery as technically feasible and economically sound; but giving priority to projects least likely to generate hard-currency earnings and therefore least suitable for financing by loans.

For the $1 billion would be used by IDA as grants-in-aid, not as loans. They would not be repayable but could be spent only in the originating country (i.e., the country against whose currency they were issued). In other words, they would be "tied gifts," although without restriction as to the commodities on which they could be spent within the country specified.

Once the developing countries spent the $1 billion Fund Obligations in the originating country (the United States), they would become unrestricted reserves, as good as gold, spendable and acceptable in all member countries. The delay with which the originating country would, under this arrangement, obtain the addition to her external reserves could be eliminated by the Fund's extending to her a line of credit repayable out of the new Fund Obligations as these are spent in, and so reach, the originating country.

This, in a nutshell, is the monetary side of functional international finance. The link it establishes between the creation of international reserves and the

granting of development aid, though often criticized, is a very natural one, with plenty of precedents on the national level. Money in the modern world, whether central-bank money or commercial-bank deposits, has always been *earned* by the people holding it, and paid for in terms of resources that were then made available to business and government to help national economic development. In an expanding world, the demand for international reserves is also expanding; and it makes good sense to fill this demand only against real resources that can contribute to development. At the same time, the newly created Fund Obligations would be backed in this plan by the gold-guaranteed currencies of the developed countries—an essential condition for establishing confidence in an international paper money.

This advantage, however, comes at a price. For one thing, only developed countries would be able to buy newly created reserves from the Fund; for another, they could only buy these reserves through the cumbersome machinery of budgetary appropriation subject to parliamentary or congressional approval. Both restrictions have their justification. The only excuse for creating new reserves is the economic possibility of creating something out of nothing; and this is only possible when unemployed and re-employable resources are present. There is no point, however, in creating additional products that nobody wants, and no justification for creating new reserves against payment in such products. This is why I propose to let the Fund create new reserves only against the currencies of countries that can pay for these reserves in products that developing countries want and need.

As to reliance on the budgetary process when a deficit country wants to augment its reserves, this is a way to remind the public that there is no easy way out of payments difficulties. Imports and foreign investments alike must be paid for by exports; although there are times when these are easily spared and do not reduce domestic availabilities. Both these truths need to be stressed; and the budgetary process is the most likely to accomplish that. This is why it seems the best machinery for an orderly expansion of the world supply of international reserves and the best safeguard against inflation.

This brings us to another apparent shortcoming of the plan: the tied nature of the aid, which is the counterpart of reserve creation. A tied gift, however, is very different from a tied loan and free from the latter's shortcomings. The objection to a tied loan is that it can be expensive. Interest and repayment that seem reasonable in money terms may prove exorbitant and usurious when the debtor is forced to buy at the creditor's high prices. By contrast, a gift, even a tied gift is unrequited generosity; and there is truth in the saying that one must not look a gift-horse in the mouth.

The purpose of tying grants-in-aid is to confine their expansionary influence to the deficit country that has decided that its economy can afford the additional exports and absorb the expansionary effects they entail. The tying of aid therefore is a safeguard against the inflationary influence of reserve creation.

It is true that money is fungible and tying seldom 100 per cent effective. In this case, however, there are two guaranties of reasonable effectiveness. One is that such grants-in-aid would finance specific development projects and could

be spent only on goods and services needed for these. The other is the developing countries' desperate need for development aid, which assures that such grants-in-aid would become additional expenditure and not the alternative financing of goods imported in any case.

It may be noted in closing that reserve creation as here outlined is designed and suitable for the secular expansion of international reserves in keeping with the needs of an expanding world economy. It would be too slow and cumbersome to deal with payments problems created by crises of confidence, which are best countered by bilateral (or multilateral) credit or swap arrangements.

TIBOR SCITOVSKY*

* The author is professor of economics at the University of California, Berkeley.

REFERENCES

1. R. N. COOPER, "Dollar Deficits and Post-War Economic Growth," *Rev. Econ. Stat.*, May 1964, *46*, 155-59.
2. J. H. POWER, "Laborsaving in Economic Growth," *Am. Econ. Rev.*, Proc. May 1962, *52*, 39-45.

Moral Suasion as an Instrument of Economic Policy

Christopher Robin goes
Hoppity, hoppity
Hoppity, hoppity, hop
Whenever I tell him politely to stop it, he
Says he can't possibly stop
—A. A. MILNE

In the fourth century, Emperor Julian the Apostate exhorted the merchants of Antioch to hold down wheat prices—with a spectacular lack of success [2, p. 801]. Long lurking in the box of little-used policy tools, moral suasion appears to be undergoing a modern-day resurrection. In recent years one can observe both a marked increase in governmental (and particularly, presidential) suasion as a policy instrument as well as (at least) a superficial increase in its effectiveness. Recent examples of moral suasion are legion: the Kennedy-U.S. Steel and Johnson-aluminum, -copper and -steel industry confrontations; implementation of wage-price guideposts; top level mediation in labor-management disputes; "voluntary" curbs on foreign investment and travel; "jawbone" exhortations to business to pass the "savings" arising from excise tax cuts on to consumers; and many, many more.

There is little evidence that moral suasion is being used wholly as a substitute for other instruments of economic policy. The increasing number and complexity of government policy objectives has led to increased use of all policy instruments, monetary, fiscal, etc. As Tinbergen has demonstrated, the instruments of policy must be at least as numerous as the number of policy objectives [5, pp. 1-4], and one way of viewing moral suasion is that govern-

ment has grasped it as an additional instrument to meet its increasingly numerous economic objectives. But there are many objections that can be raised concerning the use of moral suasion as a major policy instrument. Moral suasion is inequitable in that it rewards noncompliance; it constitutes extra-legal coercion by government without judicial review; it is in violation of the "rule of law"; where promises, implicit or explicit, are involved, it entails the danger of an overly familiar relationship between regulator and regulatee; its *ad hoc* character adds an additional and unnecessary element of uncertainty to business decisions; and it may frequently be used in lieu of (i.e., as an excuse for not implementing) more effective legislation.

Indicting as this list may be, it does not prove that moral suasion is inferior to other types of policy instruments. All policies have opportunity costs both in terms of their administrative and enforcement costs as well as their allocative effects on the economy. Whether moral suasion is inferior to other instruments or whether a partially effective moral suasion policy is even superior to a policy of doing nothing at all depends upon the relative costs and effectiveness of alternative policies and the value system within which the relative costs and benefits are weighed.

The purpose of this paper is not to examine the ethical or political or even all the economic implications of this increasingly popular policy instrument. Rather, the objective is to point out the necessary conditions for moral suasion to be an effective policy instrument. It will be argued that (1) the necessary conditions for a moral suasion policy to be successful in achieving any desired goal constitute a special, not a general, case in the economy; and (2) the presence of these necessary conditions may well be promoted by existing trends in the economy so that over time we may expect to see a continued increase in both the incidence and effectiveness of policies implemented through moral suasion.

Since the concern of this paper is with moral suasion only in the economic sphere, moral suasion will be defined here as the attempt to coerce private economic activity via governmental exhortation in directions not already defined or dictated by *existing* statute law. It is, in a sense, the extreme case of "rule of men" as opposed to "rule of law."

The meaning of the appelative *moral* in the term moral suasion is not at all clear. (Whoever heard of an economic policy of immoral suasion?) Possibly the purpose of the appelative is to distinguish *pure* moral suasion, an appeal for altruistic behavior, from other types of persuasion which are backed by threats of punishment and/or promises of reward. However, as a practical matter, pure moral suasion rarely has been used as serious economic policy.[1] Also, it is not at all clear how altruism enters utility functions, if in fact it ever does. Fortunately, the question of the degree to which altruistic considerations influence economic behavior is not crucial, for the basic difference between moral suasion and direct suasion via the "rule of law" is not the threat

[1] An exception might be appeals to patriotism in times of national emergency. The "Buy War Bonds" campaign of World War II, for example, was rather successful. To be sure, there were other incentives provided via rationing policies and in the interest paid on the bonds. But the interest rate was not *raised*.

of punishment or promise of reward. Rather, it is that punishment does not *automatically and uniformly* fall upon the (apprehended) noncomplier to moral suasion as it does upon the violator of statute law. For where moral suasion is buttressed by implicit or explicit threats, even if the policy fails, the threats might not be carried out, and if only a minority does not comply the moral suasion policy is likely to be deemed a success, and no follow-up action is required or taken. (The immorality of moral suasion is quite clear here. It rewards the noncomplier and punishes the socially cooperative and conscientious.) Although, in some unworldy sense, moral suasion might carry with it no threat of punishment or promise of reward, in a real sense, if the policy constitutes anything more than innocuous pontification to the economy as a whole, it carries at a minimum the implied threat of future legislation. In addition, the threat of fully utilizing existing regulatory powers; the danger of incurring government displeasure and inspiring legal prosecution on completely unrelated grounds; the possibility of being pointed out for public ridicule and abandonment or the promise of reciprocating favors may also play a role in instances of moral suasion, both successful and unsuccessful.

Assuming intelligent government, moral suasion is exerted in the economic sphere only in instances and directions which promote the national economic welfare (as viewed from the government's objective function). Assuming rational, profit-oriented economic units, existing private economic activity is already maximizing each unit's own economic welfare. The sphere for moral suasion, or any other economic policy, is where individual profit-seeking activity does not maximize the national welfare, i.e., where the particular and the social interests diverge. Thus, moral suasion must encourage private economic units to undertake actions which are unprofitable and which they would not undertake otherwise.

It is worth noting that the strongest protagonists on the one hand and the strongest opponents of moral suasion policies on the other, both in the abstract and on particular issues, use the lack of one or the other of the above assumptions as the main foundation of their position. Proponents of moral suasion argue that it is to the long-run benefit of private units to comply with the government persuasion ("What's good for the United States is good for General Motors . . .") implying that private units are presently acting irrationally and not maximizing (long-run) profits. On the other hand, opponents of moral suasion argue that government's position is misguided; that in fact compliance with government wishes would *not* increase national welfare (". . . and vice versa"). It is not in the national interest, they argue, to hold down aluminum or steel prices to "subsistence levels" or to take firemen off trains, etc. Neither side addresses itself to the only real dilemma: the situation where both government and private units are acting rationally in the light of their own objective functions. In what follows, intelligence and rationality are assumed on the part of all parties.[2] Eschewing from this discussion the cases

[2] To discuss moral suasion in any other context is in reality to discuss education, itself. Although not of direct concern here, the educational aspect should not be ignored. Education may be good even if it masquerades under the title of moral suasion, and educational benefits are frequently used to justify—or rationalize—moral suasion policies. This is the

where government policy is in fact not promoting the national economic welfare or where private activity is in fact not maximizing each individual unit's welfare in no way defines away the problems involved in implementing economic policy through moral suasion. On the contrary, it only disjoins those dilemmas which can be potentially resolved through intelligent and rational discourse from the dilemmas in which private and public objective functions, rationally and intelligently arrived at, are still in conflict.

Within this frame of reference, it can be somewhat tautologically asserted that moral suasion can be an effective economic policy whenever the expected cost of noncompliance is made to exceed the cost of compliance. However, there are conditions which must exist in order to so design a moral suasion policy, and these conditions severely limit the size of the set of potentially effective moral suasion policies. There are two necessary conditions for the success of a moral suasion policy. The first is a long-run condition only; the second is both short- and long-run.

1. *The public must support the government's position.* Strong involvement with the public interest increases both the scope for altruism as well as the probability that threats will be carried out. A glare of publicity can increase the power of persuasion *ex ante* (by increasing the expected cost of noncompliance) and the degree of censure on non-compliers *ex post.* However, this is only a necessary condition for an effective moral suasion policy in the long run, for fear of public displeasure is only one of the possible threats or promises with which government might back a moral suasion policy. In the short run it may be possible to establish sufficient expectations of other costs for noncompliers. But in the long run, given that economic policies are made in a democratic framework, the public must support these policies politically. This is particularly true when moral suasion is used recurrently against the same group.

2. *The populations to be persuaded must be small.* Moral suasion appears to be completely ineffective when exerted upon a large population. Fewness makes noncompliers readily identifiable and places responsibility for the success of the policy specifically and directly upon a small number of individual units so that credit for success, or blame for failure, can be levied. The analogy with central banking practices in the United States and England is an obvious one. Moral suasion is a cornerstone of English, but not U.S. monetary policy, presumably because in England there are only five major banks which need to be persuaded. A noncooperator can be immediately identified and held up for censure. On the other hand, the Federal Reserve uses a superstructure of legalistic controls to pursue the same ends. There are too many commercial banks to identify culprits.

---

case with the wage-price guideposts, for example. The function of mediators in labor-management disputes is usually explained on educational grounds as well. Similarly the reverse case, in which government is in some sense misguided, is not an irrelevant one. The government is not tutelar nor infallible and its objective function is not necessarily equivalent to the *national welfare.* It is not *necessarily* equivalent to anything even remotely approaching it. Rather it is more equivalent to some such concept as the *majority opinion* or the *compromise of the majority power.*

Furthermore, with only five banks, not only are the effects of the actions of any one bank readily visible, but any one bank itself may determine the success or failure of the policy. When there are many thousands of units, thousands may refuse to cooperate and the policy may still be deemed a success and no retaliation is levied on noncooperators. In sum, as the size of the population to be persuaded *decreases*, the probability that punishments will actually be levied on noncompliers *increases*.

The necessity for a small population to be persuaded is particularly apparent in cases of divergent firm and industry interests, for here the opportunity cost of compliance for any one unit increases as the number of units which comply increases. In essence, the greater the number of compliers, the greater the cost of compliance. This is the composition problem familiar in agriculture where it may be to the interest of all farmers to cut production in order to raise price, but not to the interest of any one farmer to do so. If the short-run average cost curve is flat, then a small number of noncompliers can produce the market share relinquished by a large number of compliers. Contrariwise, if the average cost curve is U-shaped, then cutting output can raise costs to compliers. In either case, compliers quite likely lose absolutely and always lose relatively to noncompliers. For moral suasion to be effective here it must impose a high probability of punishment on noncompliers and for this, as I have argued, fewness is necessary.

Fewness also implies a closer and more direct correspondence between individual action and public interest. If the cost of compliance is small and the fewness condition is met, moral suasion backed by altruism alone might be effective.

The failures of modern day programs for business to voluntarily restrict investment and banks to curtail credit abroad; for vacationers to restrict their tourism abroad; or for business to pass on to consumers all "savings" from excise tax cuts can be credited directly to the fact that the population which the government was attempting to persuade was too large.[8] (This was Emperor Julian's problem also.) On the other hand, government's relative success in imposing the wage-price guideposts in specific oligopolistic industries and in high-level mediation of labor-management disputes can be credited to the fact that the fewness condition was met.

The fewness condition imposes a severe limitation on the applicability of moral suasion as an instrument of policy, and it cannot be artificially satisfied

[8] One possible exception to the fewness rule, although again associated with a time of national emergency, was the implementation of the Defense Production Act during the Korean War [3, pp. 196-97] [3], [6], [7]. One aspect of this act, administered by the Federal Reserve, was for voluntary restraint by lending institutions in extending credit for nonwar and nonessential purposes. It is generally considered to have been a successful policy reaching a pinnacle of success when investment banks refused to handle state bonds issued to finance a veterans' bonus. The reasons for the relative success of this policy are probably: (1) patriotism in time of war may be a temporary exception to the fewness rule; (2) there was an attempt to achieve fewness by administering this program via regional Credit Restraint Committees consisting of representatives of local lending institutions; and (3) given the existing demand for credit at this time, the cost of compliance may have been so small that a negligible cost imposed on noncompliance could make the policy effective.

by arbitrarily delineating a small population to be persuaded. Generally, the population to be persuaded must be as large as the population which policy-makers desire to affect. One cannot, for example, successfully exert persuasion on some of the firms in a given industry to cut prices. The exception of course is where persuasion is exerted on only the price leader of an oligopolistic industry; and such industries meet the fewness condition by definition.

Fewness also implies the existence of sufficient market power to affect the public interest. Given that some level of restraint is required on economic activity, insofar as competition declines as concentration increases, substitute restraints must come from either increased governmental controls or from moral suasion. This offers a possible reason why we have observed and may continue to observe increasing use of moral suasion as an economic policy. Greater concentration in the economy increases both the effectiveness of moral suasion as well as the need for restraining policies.[4] In addition to concentration generally, the continuing growth of firms into national vis-à-vis regional markets in response to transportation and other technological improvements encourages the use and promotes the effectiveness of moral suasion. Concentration in the national market meets the fewness condition far better than does an equal degree of concentration in regional markets. In the latter, identification with the *national* interest is not close; a moral suasion policy is more difficult to administer; the probability that implied punishments will actually be levied is much less; and insofar as intermarket competition exists on at least the regional boundaries, the need for restraining policy is less as well. The geographic organization and regional bargaining practices of many labor unions, for example, might explain why labor is (as many argue) less susceptible to moral suasion than is business whose markets are more likely to be national in scope. Increasing concentration and rise of firms into national markets along with the increasing involvement of government in the economy, a possibly more representative government in a more complex economy, and a strong desire to maintain "free institutions" even where they may conflict with other policy objectives, undoubtedly explains the rise in popularity of policies implemented through moral suasion.

J. T. ROMANS*

[4] It is well known that the evidence in support of the view that concentration is increasing in the economy is not overwhelming. The state of the art of measuring industrial concentration is such that the results depend upon the particular measure and time interval adopted. In terms of the long run and in terms of expansion and concentration in the national market, however, the evidence supports the view that concentration is increasing [1] [4].

* The author is assistant professor of economics at the State University of New York at Buffalo. He is indebted to his colleagues at this institution and also to Daniel Garnick and Robert Spitze for critical comments.

REFERENCES

1. HENRY ADLER EINHORN, "Changes in Concentration of Domestic Manufacturing Establishment Output: 1939-1958," *Jour. Am. Stat. Assoc.*, Dec. 1962, *57*, 797-812.

2. EDWARD GIBBON, *The Decline and Fall of the Roman Empire*, Vol. 1. New York, Modern Library, 1932.
3. WALTER W. HAINES, *Money, Prices and Policy*. New York 1961.
4. SAUL S. SANDS, "Concentration in United States Manufacturing Industry, 1904-1947," *Internat. Econ. Rev*., Jan. 1962, *3*, 79-93.
5. J. TINBERGEN AND H. C. BOS, *Mathematical Models of Economic Growth*. New York 1962.
6. U.S. Congress, Senate Committee on Banking and Currency, *Defense Production Act, Amendments of 1951*, Hearings, 82nd Cong., 1st sess., Washington 1951.
7. ———, *Defense Production Act, Amendments of 1952*, Hearings, 82nd Cong., 2nd sess., Washington 1952.

The Supply of Storage Revisited

" . . . prices quoted at one time in a futures market, for two dates of delivery, stand in a relation which in general *does not* reflect expectations regarding events that may occur between the two delivery dates." So stated Holbrook Working [12, p. 1255] in his fundamental article on the price of storage.

Working illustrated his point in terms of the U.S. wheat market. It is typical during a crop year following a relatively sparse harvest that the May (pre-harvest) wheat futures contract will trade at a premium over the September (post-harvest) future. To Working, the magnitude of this premium was in general *not* a function of either the expected size or the expected timing of the forthcoming harvest: " . . . the price of May wheat (in April, let us say) is above the price of wheat for September delivery because the *last* crop was small (perhaps the carryover from still earlier crops was small also, contributing to the effect). So far as suppliers are concerned, it is only supplies *already in existence* which have any significant bearing on a current inter-temporal price relation of this sort" [12, pp. 1255–56]. This observation was based at least in part on some of Working's earlier empirical findings [10] [11].

Working's hypothesis is particularly useful because it places in proper perspective the dominating role which stocks play in determining intertemporal price relationships (i.e. relationships between expected prices for the delivery of a commodity at different points in time). On the other hand, insofar as it reflects the consideration of a special case, the hypothesis is incomplete and in part misleading. An investigation of this point not only clarifies the theory of intertemporal price relationships, but what is of greater interest, it also yields a theory explaining the dynamic behavior of commodity spot prices.

I. *The Supply of Storage*

The fundamental relationship which gave rise to Working's hypothesis is the "supply of storage" curve, illustrated in Figure 1. For very low current

inventory levels, the spot price of a commodity is at a substantial premium to the forward price as a reflection of the high marginal "convenience yield" which the aggregated processors and merchants gain from holding the small aggregate inventory. The convenience for manufacturers comes in the form of avoiding plant closings, and maintaining at least a modicum of cost coverage against finished product price quotations. For merchants, stocks are necessary for a continuing flow of trading. As the aggregate inventory increases, the marginal convenience yield declines to zero, and the expected forward price exceeds the spot price by carrying costs (warehousing fees, insurance, shrinkage, etc.). Finally, given very large aggregate inventory levels commodity holders can be induced to carry additional stocks only by the expectation of a return, or risk premium, for so doing.[1]

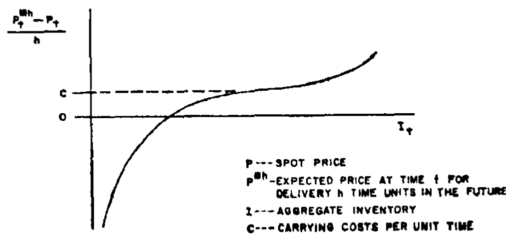


FIGURE 1. SUPPLY OF STORAGE CURVE

The supply of storage curve portrays the expected price change over some finite interval as a function solely of the *current* aggregate inventory level,

$$\frac{P_t^{*h} - P_t}{h} = f_h(I_t) \tag{1}$$

where,

f_h = Supply of storage function for an interval of h time units.[2]

The interesting point is that equation (1) is questionable on both theoretical and empirical grounds.

First, the hypothesis involves something of a logical inconsistency. Con-

[1] See Working [12] and Brennan [2] for more detailed discussions of the supply of storage curve. The supply of storage function presented and discussed here relates to a market devoid of futures trading. The introduction of futures trading into a commodity market tends to flatten the supply of storage curve somewhat by virtue of the fact that such trading allows additional persons (i.e., futures speculators) to own stocks during times of surplus, and to supply stocks during times of shortage. But the general shape of the curve shown in Figure 1 remains unchanged. For discussions of the role played by futures trading in supply of storage theory, see Cootner [4] [5], Stein [8], and the author's dissertation [9].

[2] We are assuming here that expected consumption over the horizon interval, h, is constant, since the convenience yield of the inventory is clearly a function of the amount of coverage it provides for expected needs. Relaxing this assumption would complicate but leave essentially unchanged the arguments which follow.

sider equation (1) written for a general time interval in the pattern of price expectations,[3] starting at horizon time, h, and having a length, dh:

$$\frac{P_t^{*h+dh} - P_t^{*h}}{dh} = f_{dh}(I_t^{*h}) \tag{2}$$

where,

P^{*h} = Expected spot price h months in the future;
h = Horizon time of initial expectation;
dh = Interval between expectations;
I^{*h} = Inventory level expected h months in the future.

Now if we let dh approach zero, so that the inventory level can be assumed to remain constant over the very short interval, dh, we have,

$$\frac{dP_t^{*h}}{dh} = f(I_t^{*h}) \tag{3}$$

where,
f = Incremental supply of storage function.

Then the supply of storage function linking the spot price with the price expected at some non-zero horizon time can be derived as follows:

$$P_t^{*h} = P_t^{*0} + \int_0^h f(I_t^{*h})dh \tag{4}$$

But since the expected spot price at a zero horizon (P_t^{*0}) is simply the spot price, we have the general supply of storage function for a specific horizon,[4]

$$\frac{P_t^{*h} - P_t}{h} = \frac{1}{h}\int_0^h f(I_t^{*h})dh \tag{5}$$

That is, *the spread between the spot price and the price expected at some future time is a function of expected inventory behavior over the intervening interval.*

Figure 2 provides an empirical example of the effect of changes in inventory expectations on intertemporal price relationships. The context is the cocoa market, and futures prices are used instead of expected spot prices. During the 1959–60 autumn and winter period, market estimates of the size

[3] The rationale for writing equation (1) in this form is that if equation (1) explains the intertemporal price difference between the present and some future time, then with a suitable replacement of variables, it should also explain the intertemporal price difference between two future points in time. This is akin to Muth's "rational expectations" hypothesis: "... expectations, since they are informal predictions of future events, are essentially the same as the predictions of the relevant economic theory" [6, p. 316].

[4] To my knowledge this formulation is original to Samuelson [7, pp. 217–18], though he did not put it in quite this way.

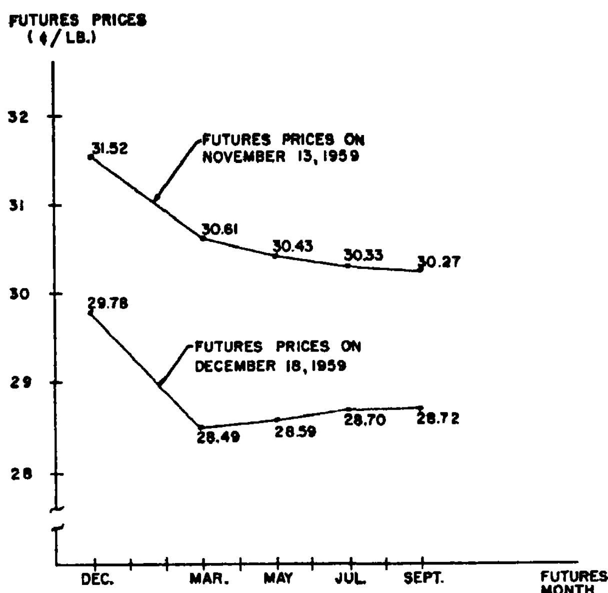


FIGURE 2. COCOA FUTURES PRICES, NOVEMBER–DECEMBER, 1959

of the African main crops being harvested increased significantly. The result was a decline in the entire futures price structure during the interval from mid-November to mid-December. The crops did not become fully available to the market in time to prevent a relative shortage during December; hence, the existence of inverse carrying charges between the December and March futures, at both of the illustrated dates. But the increase in crop expectations was large enough to cause the market to shift from expecting a continued relative shortage after December to expecting something of a surplus. This shift in expectations is indicated by the shift in spreads between the successive futures beginning with March, from inverse carrying charges in November to positive carrying charges in December.

Equation (1) is a special case of equation (5) in that it relates to those instances which arise, especially in the case of an agricultural commodity with a highly lumped harvest, where the expected future inventory pattern during a large portion of each season can be approximately stated in terms of the current inventory level (e.g., after the harvest the inventory tends continuously to decline until the next harvest). That is, one can write,

$$I_t^{*h} = g(I_t, h) \tag{6}$$

which when substituted in equation (5), yields

$$\frac{P_t^{*h} - P_t}{h} = \frac{1}{h} \int_0^h f[g(I_t, h)]dh \tag{7}$$

which is consistent with equation (1) and Working's conclusion if,

$$f_h(I_t) = \frac{1}{h}\int_0^h f[g(I_t, h)]dh \tag{8}$$

where,

f_h = Supply of storage function for an interval of h time units;
f = Incremental supply of storage function.

II. *The Spot Price Function*

The supply of storage theory embodied in equations (3) and (5) explains the behavior of intertemporal differences in price expectations in terms of expected inventory behavior. But as was indicated earlier, the supply of storage concept is also useful in explaining dynamic spot price behavior, given certain further assumptions.

Consider the following system of equations describing a general commodity industry:

$$C_t = C^e - b(P_t - P^e) \tag{9}$$

$$H_t = H^e + e_t \tag{10}$$

$$I_t = I_0 + \int_0^t (H_t - C_t)dt \tag{11}$$

$$\frac{dP_t^{*h}}{dh} = a(I_t^{*h} - I^e) \tag{12}$$

where,

$$b > 0; \qquad a > 0; \qquad C^e = H^e;$$

and,

C = Consumption
P = Spot price
H = Production (harvest)
I = Inventory
P^{*h} = Price expected at horizon interval, h (i.e., at time $= t+h$)
I^{*h} = Inventory level expected at horizon interval, h
P^e = Equilibrium price
C^e = Equilibrium consumption
H^e = Equilibrium production
I^e = Equilibrium inventory

The harvest rate is postulated as varying randomly about a constant (equilibrium) rate. The equilibrium values of price and consumption are so named because, when price is at its equilibrium value, consumption is at its equilibrium value and is equal to equilibrium production. When the expected inventory level is at its equilibrium value in the supply of storage

function—equation (12), which is a linear version of equation (3)—the expected price remains constant.

Now we can solve the above model for the expected future behavior of price and inventory, if we assume that expected consumption is related to expected price by equation (9), that expected production is constant at H^e, and that expected inventory behavior can be derived according to equation (11):[5]

$$C_t^{*h} = C^e - b(P_t^{*h} - P^e) \tag{13}$$

$$H_t^{*h} = H^e = C^e \tag{14}$$

$$I_t^{*h} = I_t + \int_t^h (H_t^{*h} - C_t^{*h})dh \tag{15}$$

Taking the second derivative of (15) with respect to h, we have,

$$\frac{d^2}{dh^2} I_t^{*h} = \frac{d}{dh} H_t^{*h} - \frac{d}{dh} C_t^{*h} \tag{16}$$

or, using equations (12), (13), and (14),

$$\frac{d^2}{dh^2} I_t^{*h} - abI_t^{*h} = - abI^e \tag{17}$$

which, given the inventory at the beginning of the expectation period (i.e., I_t, the current inventory), and the restriction that both a and b must be positive, has the stable solution,[6]

$$I_t^{*h} = I^e + (I_t - I^e)e^{-\sqrt{ab}\,h} \tag{18}$$

so that, as the horizon becomes large, the expected inventory approaches the equilibrium level. Substituting (18) into (12), and integrating with respect to h, yields,[7]

$$P_t^{*h} = P_t^{*0} + \int_0^h a(I_t - I^e)e^{-\sqrt{ab}\,h}\,dh \tag{19}$$

[5] These assumptions are a direct application of Muth's rational expectations hypothesis, in that the expected future values of the variables in the system are interrelated in the same manner as are the actual values. The random element in production is not predictable, and is assumed constant at its (zero) expected value.

[6] This is only one of an infinite set of possible solutions to equation (17), given the initial inventory level and no other boundary conditions. This particular solution is based on the behavioral assumption that for very large values of h, the inventory will be assumed to be approximately at its equilibrium level. This is the only solution to equation (17) which does not eventually lead to an exponential explosion in the values of the system's variables, which explosion would involve the implausible combination of price and inventory values simultaneously either both above or both below their respective equilibrium levels on a continuous basis.

[7] Note that equations (18) and (19) constitute a special case of equations (6) and (7).

or,

$$P_t^{*h} = P_t + \frac{a}{b}(I_t - I^e)(1 - e^{-\sqrt{ab}\,h}). \tag{20}$$

Now the expectation that the inventory will approach its equilibrium level implies that the price is expected to approach some constant level (see equation [12]), and this must be the equilibrium price level since a constant inventory implies a consumption rate equal to the production rate.[8] Therefore, allowing the horizon time, h, to become large in equation (20), we have,

$$P^e = P_t + \frac{a}{b}(I_t - I^e) \tag{21}$$

or,

$$P_t = P^e - \frac{a}{b}(I_t - I^e) \tag{22}$$

Within the confines of our special case, this result is of great importance. It says that if: (a) in arriving at their expectations people assume that expected behavior is generated by the same equation system that they feel generates actual behavior (i.e., Muth's rational expectations hypothesis holds), and (b) people behave as if their expectations were certain to come true (i.e., their expectations constitute certainty equivalents), then the current price is a function of the current inventory level alone.[9]

Now consider a generalized version of equations (9) through (12), such that both consumption and production are functions of *lagged* price (as is likely to be the case in general), and the supply of storage curve is curvilinear. A general solution of the resulting equation system is no longer attainable (the differential equation corresponding to (17) becomes high order and non-linear). Nevertheless, given certain behavioral assumptions about the generation of expectations in actual commodity markets, we can rescue some of the potential generality of equation (22).

First, it is common in commodity markets for there to exist published

[8] Samuelson [6, pp. 211–19] shows that this stability characteristic of a model having the general form of equations (18) through (21) holds as long as the demand curve has a negative slope,

$$dC_t/dP_t < 0 \qquad \text{for all } P_t$$

and the supply of storage curve has a positive slope,

$$\frac{d}{dI_t^{*h}}\left[\frac{dP_t^{*h}}{dh}\right] > 0 \qquad \text{for all } I_t^{*h}$$

and the behavioral assumption footnoted above is made.

[9] Using similar assumptions, Beckmann [1, p. 10] reaches a similar conclusion. While the model presented above is continuous and linear, Beckmann's model is discrete and non-linear (he in effect uses a kinked supply of storage function), but the results are analogous.

forecasts of production and consumption (and therefore inventory) behavior over some finite horizon period, usually one year or less. These expectations can be substituted into equation (5) to yield a relationship between the spot price and the price expected at the end of the finite horizon interval, h_1,

$$P_t = P_t^{*h_1} - \int_0^{h_1} f(I_t^{*h})dh \tag{23}$$

where,

P = Spot price;
P^{*h_1} = Spot price expected at horizon time, h_1;
I^{*h} = Inventory expected at horizon time, h.

Now if the expected behavior of the inventory over the horizon interval can be approximately stated in terms of its beginning value (I) and its ending value (I^{*h_1}) in such a way that equation (23) is integrable, then we have the approximation,

$$P_t = P_t^{*h_1} - F^{h_1}(I_t, I^{*h_1}) \tag{24}$$

where,

$$F^{h_1}(I_t, I_t^{*h_1}) = \int_0^{h_1} f(I_t^{*h})dh \tag{25}$$

Second, if we can further assume that after the end of the horizon interval, h_1, both the price and inventory are expected to approach gradually their expected equilibrium values,[10] then again using equation (5) we can write,

$$P_t^{*h_1} = P^e - \int_{h_1}^{\infty} f(I_t^{*h})dh \tag{26}$$

And if the expected behavior of the inventory over the period after h_1 can be approximately stated in terms of its expected values at $h_1(I^{*h_1})$ and ta infinity (I^e, its expected equilibrium value) in such a way that (26) is integrable then we have,

$$P_t^{*h_1} = P_t^e - F^{\infty}(I_t^{*h_1}, I_t^e) \tag{27}$$

[10] If the characteristic dynamic behavior of the actual commodity system is not purely damped, this assumption may violate Muth's rational expectations hypothesis. For example the lag structure of the system may be such as to generate long-run fairly damped (i.e., irregular) oscillations around an equilibrium state in response to exogenous random disturbances. But if the system structure is complex enough to generate oscillations, Muth's hypothesis that market expectations are based on an awareness of that structure (at least to the same degree that economic theorists are aware of the structure) may be unreasonable. As long as the oscillations are fairly damped around relatively constant equilibrium values, it may be more reasonable to assume that market participants at any point in time expect the system variables to approach equilibrium in a purely damped (non-oscillatory) fashion.

where,

$$F^{\infty}(I_t^{*h_1}, I_t^e) = \int_{h_1}^{\infty} f(I_t^{*h})dh \tag{28}$$

Combining (24) and (27) yields,

$$P_t = P_t^e - F^{h_1}(I_t, I_t^{h_1}) - F^{\infty}(I_t^{*h_1}, I_t^e) \tag{29}$$

Again within the limits set by the underlying assumptions, equation (29) is potentially of fundamental importance. It indicates that the spot price can be approximately stated as a function of the current inventory, the market's inventory forecast for some finite horizon, and the expected long-run equilibrium levels of price and inventory.

F. HELMUT WEYMAR*

* The author is an economist with National Biscuit Company. This paper presents part of the results of thesis research carried on at MIT, and is in large measure the outcome of several provocative discussions with Professors Paul H. Cootner and Paul A. Samuelson.

REFERENCES

1. M. J. BECKMANN, "On the Determination of Prices in Futures Markets," in M. J. Brennan, ed., *Patterns of Market Behavior*, Providence 1965.
2. M. J. BRENNAN, "The Supply of Storage," *Am. Econ. Rev.*, Mar. 1958, *47*, 50–72.
3. ———, "A Model of Seasonal Inventories," *Econometrica*, Apr. 1959, *27*, 228–44.
4. P. H. COOTNER, "Common Elements in Futures Markets for Commodities and Bonds," *Am. Econ. Rev.*, Proc., May 1961, *51*, 173–83.
5. ———, "Returns to Speculators: Telser vs. Keynes," *Jour. Pol. Econ.*, Aug. 1960, *68*, 396–404.
6. J. F. MUTH, "Rational Expectations and the Theory of Price Movements," *Econometrica*, July 1961, *29*, 315–35.
7. P. A. SAMUELSON, "Intertemporal Price Equilibrium: A Prologue to the Theory of Speculation," *Weltwertschaftliches Archiv*, 1957, *79*, 181–221.
8. J. L. STEIN, "The Simultaneous Determination of Spot and Futures Prices," *Am. Econ. Rev.*, Dec. 1961, *51*, 1012–25.
9. F. H. WEYMAR, *Dynamics of the World Cocoa Market*, unpublished doctoral dissertation, Massachusetts Institute of Technology, 1964.
10. H. WORKING, "Price Relations between July and September Wheat Futures at Chicago since 1885," *Wheat Studies*, Mar. 1933, *9*, 187–238.
11. ———, "Price Relations between May and New Crop Wheat Futures at Chicago since 1885," *Wheat Studies*, Feb. 1934, *10*, 183–228.
12. ———, "The Theory of the Price of Storage," *Am. Econ. Rev.*, Dec. 1949, *31*, 1254–62.

Hidden Unemployment 1953-62—A Quantitative Analysis by Age and Sex: Comment

In a recent article in this journal [1] Thomas Dernburg and Kenneth Strand presented a labor market model which was used to estimate hypothetical labor force participation ratios and unemployment rates by age and sex for the years 1954 and 1962 under assumed conditions of 4 per cent total unemployment in both years. Their estimates of the distribution of unemployment at 4 per cent total unemployment showed a heavier concentration among young and old males and all female groups (marginal workers) in 1962 than in 1954, supporting one conclusion that "The economy has not yet been able to adjust to the sizable shifts in the structure of the labor force that have taken place over the decade." This is an important finding. I submit, however, that it does not necessarily follow from the model which the authors presented.

Before elaborating, it will be useful to review first certain aspects of the authors' basic equations. Their estimates of unemployment in the model derive from two separate sets of equations on employment and labor force participation by age and sex. The underlying data used in estimating the equations were divided into 14 age-sex groups using monthly observations for the period 1953-62 inclusive. The labor force and employment equations took the following forms, respectively:

(1) $(Li/P)_t = a + a_1(E/P)_t + a_2(X/P)_{t+2} + a_3(1/P)_t + a_4(Pi/P)_t + r,$

and

(2) $(Ei/P)_t = b + b_1(E/P)_t + b_2(1/P)_t + b_3(Pi/P)_t + r,$ where

Li/P is the ratio of labor force in age-sex group i to total working-age population;

E/P is the aggregate employment-population ratio;

X/P is the ratio of new unemployment compensation exhaustions to total population;

$1/P$ is the reciprocal of total population;

Pi/P is the ratio of population in group i to total population;

Ei/P is the ratio of employment in group i to total population; and r is a residual-error term.

In a third equation the authors explained the exhaustions ratio in the current period as a function of lagged employment and lagged exhaustions ratios.

The above coefficient a_1 captures the response of group labor force to total employment, while b_1 captures the response of group employment to total employment. The reciprocal of population accounts for trend in the dependent variables in both the labor force and employment equations, while the variable Pi/P measures the effect of relative population shifts. The exhaustions ratio attempts to take account of the pressure on additional workers to enter the labor force before unemployed primary workers use up their unemployment benefits.

Consistent with earlier findings [2] [3] [4] [5] [6], the authors found

that the labor force ratios of marginal workers were strongly and positively related to aggregate employment, while the labor force ratios of prime-age males were relatively autonomous. The employment of all age-sex groups was shown to benefit in varying degrees from rising aggregate employment. Combining the results of the labor force and employment equations yielded the net unemployment outcome, which in the authors simulation clearly showed the growing disadvantage of marginal workers vis-à-vis primary workers.

Of main concern here are the age-sex employment equations, particularly the magnitude of the coefficient b_1 which captures the response of group employment to total employment. The Dernburg-Strand correlations were run on the period 1953-62, but the average total employment-population ratio in the regression period was below its 1955-57 levels when the overall (officially reported) unemployment rate was about 4 per cent. The value of b_1 in the employment equations is in fact consistent with a total unemployment rate in the decade averaging 5.2 per cent. It is apparent, therefore, that b_1 is measuring the response of group employment to total employment in a *loose* labor market characterized by high overall unemployment. Nevertheless, the authors drew upon this coefficient to simulate employment by age and sex in a *tight* labor market characterized by an assumed 4 per cent total unemployment rate. By so doing, they are implicitly assuming that the response of group employment to total employment does not vary in different stages of the cycle. Since the correctness or incorrectness of this assumption is critical in estimating the distribution of both employment and unemployment, it should not go unchallenged. Contrary to Dernburg and Strand's implicit assumption, one might well expect the relation of group employment to total employment to vary by stage of cycle.

To give one illustration: suppose that, as the economy moves from a 5 per cent to a 4 per cent or lower total unemployment rate, the available supply of experienced primary workers begins to grow short. At some point, to satisfy their labor needs, employers step up their inservice training programs and accelerate the upgrading of available marginal workers. At the same time, as more jobs become available, workers may become more willing to take advantage of education and training opportunities. If such adjustments are triggered or accelerated by a tight market situation, then one would expect employment to flow more rapidly toward marginal workers as they are substituted for scarce primary workers. Hence, the coefficient b_1 in eq. (2) would be greater for marginal workers in a tight market than in a loose market, and smaller for primary workers in a tight market than in a loose market. (The less apparent possibility that the response of labor force to total employment might vary in different stages of the cycle, which would also influence unemployment, will be dealt with below.) Other things equal, a larger share of total employment going to marginal workers in periods of low overall unemployment would reduce their unemployment relative to primary workers. In this event, the estimated "full employment" distribution of unemployment in 1954 and 1962 could be quite different from the Dernburg-Strand results.

The hypothesis that group employment ratios *vary* in relation to total em-

ployment ratios in different stages of the cycle can be tested by dividing up the postwar employment experience into cycle stages, with each stage representing a different degree of labor market tightness. For this purpose the following simple variation of the Dernburg-Strand employment equation was tried, using seasonally adjusted quarterly data for the period 1947(Q4)-1965(Q1):

$$(3) \qquad (E+A/P)_t = a + b_1(E+A/P)_I + b_2(E+A/P)_{II} \ldots . + b_5(E+A/P)_V + b_6T,$$

where A is armed forces, T is a trend term, the numerals I-IV referring to successive stages of employment expansion, with stage V including recessions. Thus, $(E+A/P)_I = (E+A/P)$ in those selected periods of the cycle designated as stage I and $(E+A/P)_I = 0$ in all other periods; $(E+A/P)_{II} = (E+A/P)$ in stage II and $(E+A/P)_{II} = 0$ in all other periods; etc. A given stage can include observations from more than one employment cycle, in which case the character of demand in the separate cyles is averaged out to some extent. In order to gain sufficient observations for each of the independent employment variables, the analysis was extended back to 1947 and forward to 1965. Since this longer period contains the Korean War, armed forces were added to employment in an attempt to correct for the distortions evident in the employment data in the early 1950's, which amounts to the assumption that most persons in the armed forces would have been otherwise employed. The inclusion of armed forces does not seriously affect the net regression coefficients in nonwar periods where fluctuations in employment dominate.

Partly because of the ready availability of certain data, eq. (3) differs in several respects from Dernburg and Strand's employment equation (aside from the stage-of-cycle divisions). But these differences are restricted to population or trend expressions which change but gradually over time, and which are generally not correlated with the business cycle. Specifically, eq. (3) ignores the Dernburg-Strand population variable Pi/P; uses T rather than $1/P$ for trend; and expresses group population in the denominator of the dependent variable in place of Dernburg and Strand's total population. There is no reason to expect that these specification differences, which involve secular influences only, would affect the relative difference between the stage-of-cycle coefficients $b_1 \ldots b_5$. Accordingly, of sole concern here is simply the question of whether or not there is any significant difference *between* the stage-of-cycle coefficients in each age-sex group. True, the addition of secular variables or their respecification in the stage equation would no doubt influence the *level* of the net regression coefficients, so no conclusions will be drawn based on such levels in the stage equations.

A description of the five cycle stages used in eq. (3) follows. The selection of stages was guided by observing the behavior of both the aggregate employment-population ratio and the total unemployment rate as indicators of labor market tightness. In the four expansion stages the aggregate employment ratio mostly rises, while in recession it mostly declines. Over the postwar period it ranged between 54.1 and 57.8 per cent.

Stage I. This first stage of expansion includes 15 quarters from three separate cycles: 1950(Q1)-1950(Q2); 1958(Q3)-1959(Q1); and 1961(Q3)-(1963)(Q4). The aggregate employment ratio in these quarters increased from slightly over 54 per cent to about 55 per cent, averaging 54.4 per cent. (Observations from the initial stages of the 1947-48 and 1954-55 expansions are excluded here because the aggregate employment ratio in those periods was predominantly above the 55 per cent upper range of acceptance for this stage.) The total unemployment rate in this stage declined from a high of 7.3 per cent to a low of 5.5 per cent, averaging 6.0 per cent.

Stage II. This stage consists of 15 observations from four postwar expansions: 1950(Q3)-1950(Q4); 1954(Q4)-1955(Q2); 1959(Q2)-1960(Q2); and 1964(Q1)-1965(Q1). The range of the aggregate employment ratio is roughly between 55 and 56 per cent (for which the 1954-1955 expansion period now qualifies). The aggregate employment ratio averaged 55.2 per cent in this phase, indicating a cluster of values around the low end of the range. The unemployment rate declined within a range of 5.4 to 4.2 per cent, averaging 5.0 per cent.

Stage III. Included here are the 14 quarters 1947(Q4)-1948(Q4) and 1955(Q3)-1957(Q3). That the 1948 period is included in this relatively late stage reflects both the strength of aggregate demand and the choice of a starting date for the analysis. In this phase the aggregate employment ratio fluctuated in the 56 to 57 per cent range, averaging 56.5 per cent. At the same time the unemployment rate varied in a narrow range between 4.3 per cent and 3.8 per cent, averaging 4.1 per cent. This stage represents a relatively tight labor market in which the 1955-1957 experience dominates.

Stage IV. This continuous 11 quarter period from 1951(Q1)-1953(Q3) represents the tightest postwar labor market, and includes the abnormalities of the Korean period. In this phase the aggregate employment ratio increased from 57 to nearly 58 per cent, averaging 57.2 per cent. Conversely, the unemployment rate fell from 3.5 per cent to nearly 2.5 per cent, and averaged 3.0 per cent.

Stage V. This final stage includes 15 quarters taken from four postwar recessions: 1949(Q1)-1949(Q4); 1953(Q4)-1954(Q3); 1957(Q4)-1958(Q2); and 1960(Q3)-1961(Q2). The aggregate employment ratio declined sharply to a recession low of about 54 per cent, and averaged 55 per cent over the 15 quarters, while the unemployment rate rose from a low of 3.7 per cent to a high of nearly 7.5 per cent, averaging 6.0 per cent.

The results of this model are shown in the table. The regression coefficients of only one age-sex equation, females age 20-24, are not statistically significant. The fact that the cyclical regression coefficients for this group are negative (compared to Dernburg and Strand's positive result) reflects the incomplete specification of the stage model. Had the stage equations been specified in the manner of Dernburg and Strand, including the variable Pi/P, the cyclical regression coefficients for females age 20-24 would probably have been positive. But, as stated earlier, the main interest here is the extent of difference between the stage-of-cycle coefficients, not their levels. Further com-

TABLE 1—THE REGRESSION RESULTS

	a	b_1	b_2	b_3	b_4	b_5	b_6	R^2
Males 14–19 years	−33.3	1.489 (0.211)	1.488 (0.208)	1.502 (0.204)	1.494 (0.202)	1.492 (0.209)	−0.156 (0.007)	.97
Males 20–24 years	−116.3	3.593 (0.271)	3.578 (0.267)	3.525 (0.262)	3.577 (0.259)	3.574 (0.268)	0.057 (0.008)	.89
Males 25–34 years	44.2	0.864 (0.153)	0.876 (0.151)	0.876 (0.148)	0.891 (0.146)	0.858 (0.152)	0.028 (0.005)	.82
Males 35–44 years	70.5	0.434 (0.124)	0.440 (0.122)	0.449 (0.120)	0.451 (0.118)	0.429 (0.122)	−0.003 (0.003)	.79
Males 45–54 years	58.2	0.616 (0.137)	0.624 (0.135)	0.627 (0.133)	0.631 (0.131)	0.610 (0.135)	0.001 (0.004)	.78
Males 55-64 years	62.0	0.394 (0.200)	0.402 (0.197)	0.430 (0.193)	0.416 (0.191)	0.406 (0.198)	−0.022 (0.006)	.76
Males 65+years	2.67	0.785 (0.181)	0.785 (0.179)	0.790 (0.176)	0.781 (0.174)	0.795 (0.180)	−0.278 (0.006)	.99
Females 14–19 years	2.47	0.475 (0.181)	0.471 (0.178)	0.494 (0.174)	0.492 (0.172)	0.480 (0.179)	−0.061 (0.006)	.89
Females 20–24 years	63.2	−0.391 (0.233)	−0.367 (0.230)	−0.354 (0.226)	−0.343 (0.223)	−0.391 (0.231)	0.016 (0.007)	.39
Females 25–34 years	6.71	0.445 (0.157)	0.443 (0.155)	0.443 (0.152)	0.456 (0.150)	0.446 (0.155)	0.054 (0.005)	.69
Females 35–44 years	−24.1	1.086 (0.110)	1.080 (0.108)	1.075 (0.106)	1.070 (0.105)	1.086 (0.109)	0.120 (0.003)	.96
Females 45–54 years	−24.4	1.041 (0.224)	1.031 (0.220)	1.031 (0.216)	1.020 (0.213)	1.038 (0.221)	0.273 (0.007)	.97
Females 55–64 years	−7.5	0.546 (0.176)	0.538 (0.173)	0.551 (0.170)	0.531 (0.168)	0.540 (0.174)	0.257 (0.005)	.98
Females 65+years	−11.9	0.370 (0.144)	0.373 (0.142)	0.375 (0.139)	0.360 (0.138)	0.377 (0.143)	0.024 (0.004)	.41

parisons of these equations to the Dernburg-Strand findings would not be particularly enlightening because of differences in both model specification and the length of the regression period.

Turning to the extent of similarity or difference between the the net regression coefficients $b_1 \ldots b_5$ in each group in the table, it is apparent by even casual observation that the coefficients are not significantly different from one another in all stages of the cycle. I would add that this is also true if trend is eliminated from the stage equation, and if the model is based on first differ-

ences. Nor are there significant differences when the independent variabl are regrouped into fewer broader stages in order to increase the number observations in each stage.

Thus, it appears that substitution among employment groups, insofar as is reflected in the relation of group employment to total employment cyclic ly, does not accelerate in a tight market, but is a steady gradual proc throughout all stages of the cycle. Since the relationship tested does n vary throughout the cycle, it can properly be expressed in equation form combining all observations of the aggregate employment ratio in a single ind pendent variable as Dernburg and Strand did. The hypothesis of a short-r nonhomogeneous adjustment mechanism between age-sex specific and tot employment ratios is tentatively rejected.

But an equilibrating mechanism in the employment market can also be of secular nature, as might be further indicated by the presence of trend in t above cyclical regression coefficients $b_1 \ldots b_5$. For example, a significant positive trend in the cyclical coefficients of young, old, and adult female wor ers in periods of rising demand could lessen the degree of divergence in the e timated distribution of unemployment, and negate the Dernburg-Strar finding of a worsening structure of unemployment rates based mainly on a c clical analysis. But a test showed that such is not the case. For young and o workers the (linear) trend in the cyclical regression coefficients was eith negative or not statistically significant. Not surprisingly, there was a positi trend in the regression coefficients for most adult women who are an increa ing fraction of total employment. But women also had a positive and offse ting trend value in their cyclical regression coefficients on the labor force sic (i.e., when L is substituted for E in the dependent variable of the above stag model), which *cetcris paribus* leaves their relative unemployment position ur improved. The residual values from the employment equations for the groups appeared essentially noncyclical and unrelated to other economic var ables such as wages or hours, but these variables were not explicitly include in the model.

If the labor supply sensitivity of marginal workers to rising demand shoul lessen as full employment is approached or attained, then their labor forc would grow less rapidly relative to employment, which would exert downwar pressure on their unemployment rates. But this seems unlikely. It would b more reasonable to suppose that the labor supply sensitivity of these group would remain the same, or possibly increase in successive stages of expansio as job opportunities improve. Analysis of scatter diagrams of labor force-pop ulation ratios against employment-population ratios indicates an unchangin cyclical relationship, as do the results of substituting labor force for employ ment in the dependent variable of the above stage equation. Trends in the cy clical regression coefficients for young and old workers in the labor force-em ployment relation were not statistically significant.

In summary, the foregoing results tend to confirm the Dernburg-Stran as well as my own earlier finding [3] that adjustments on the employmen (demand) side of the labor market have not kept up with shifts on the labo

force (supply) side, indicating a worsening distribution of unemployment rates under hypothetical conditions of relative full employment.

ALFRED TELLA*

* The author is an economist at the Board of Governors of the Federal Reserve System.

REFERENCES

1. THOMAS DERNBURG AND KENNETH STRAND, "Hidden Unemployment 1953-62: A Quantitative Analysis By Age And Sex," *Am. Econ. Rev.*, March 1966, *56*, 71-95.
2. ———, "Cyclical Variation In Civilian Labor Force Participation," *Rev. Econ. Stat.*, Nov. 1964, *46*, 378-91.
3. ALFRED TELLA, "Labor Force Sensitivity To Employment By Age, Sex," *Indus. Rel.*, Feb. 1965, *4*, 69-83.
4. ———, "The Relation Of Labor Force To Employment," *Indus. and Lab. Rel. Rev.*, Apr. 1964, *17*, 454-69.
5. W. G. BOWEN AND T. A. FINEGAN, "Labor Force Participation and Unemployment," in Arthur M. Ross, ed., *Employment Policy and the Labor Market*, Berkeley 1965, pp. 115-61.
6. JACOB MINCER, "Labor Force Participation And Unemployment: A Review of Recent Evidence." Paper presented at the 3rd Annual Conference on Unemployment and the American Economy, New York, June 14-16, 1965.

Forecasting and Analysis with an Econometric Model: Comment

In the March 1962 issue of this *Review*, Professor Suits[1] discussed the University of Michigan's econometric model and its use as a predictive tool. This note presents the results of tests which were designed to evaluate the model's forecasting performance. Before presenting these results, it is important to consider the methods which may be utilized to evaluate the forecasting performance of econometric models, for there is no consensus on *the one* best procedure.

I. *Evaluation Procedures and Concepts*

The first distinction is between *ex post* and *ex ante* forecasts. In *ex ante* forecasts the *true* values of both the exogenous or lagged endogenous variables are not known at the time that the forecast is made and must be estimated. In a forecast prepared in the fourth quarter of a year for the following year, for

[1] We may also be interested in testing the predictive performance of only a portion of a model. In this case, we would be interested in evaluating the predictive performance of a single equation, say, that explaining the behavior of automobile sales. The appropriate procedure in this case is to utilize the structural equation explaining automobile consumption.

example, some of the data are not yet available and even the quarterly or other available figures may be preliminary and subject to substantial revision. Observed forecasting errors in this case may be due either to an incorrect specification of the model or to errors in the estimates of the predetermined variables.

Ex post forecasts use the actual values of the predetermined variables to calculate the predicted values of the endogenous variables. Any errors which result are therefore attributable only to the model itself. These *ex post* estimates may be obtained either for the sample period or for elapsed time periods beyond the sample when the values of the predetermined variables are known. To test the predictive performance of a model in this manner it might be preferable to use data beyond the period of fit, for if the *ex post extrapolations* yielded good predictions, more confidence could be placed in the model's ability to predict.

While it has been argued that the "true test" of a model is its ability to forecast *ex ante,* it is necessary to utilize the *ex post* approach to properly evaluate a model. It can then be determined whether the forecast errors resulted from the model or from incorrect data projections. In addition, it is possible to determine whether the *ex ante* forecast was "correct" due to offsetting errors within the projections and/or the model. In other words, we can distinguish between the ability of the analyst in estimating the predetermined variables and that of the model in simulating the economy.

Another problem arises in the evaluation of a model: Given the actual and either the *ex ante* or *ex post* forecasts, how can the accuracy be determined? A number of alternative criteria may be used to determine the accuracy of a forecast. These criteria include (1) the percentage of turning points in business activity which are forecast correctly, (2) the degree to which the model's rates of change corresponded to the direction and extent of the observed changes, and (3) the performance of the model relative to some naive method of forecasting.

It is inappropriate merely to count the number of true turning points forecast for two reasons. First, a model which is expected to predict quantitatively should be judged on that basis and attention should not merely be confined to the few turns which occurred. In addition, there is the question of whether a model which predicts "a miniscule advance" when a miniscule decline occurs can be judged as being completely wrong. In other words, a 0, 1 scoring system for accuracy is inappropriate because the extent of the error matters. A method which utilizes information about the absolute discrepancy between the forecast and the observed change should be employed, for this also permits comparison of a particular model's forecast with the accuracy of other forecasting procedures and naive methods.

One such measure is Theil's [2] inequality coefficient:[2]

[2] Theil indicates that his coefficient, unlike the standard deviation of the forecasting errors, has the advantage that it also covers the case in which the forecasts are consistently over- or underestimated. He also shows that the U coefficient can be decomposed into three components to determine the sources of error.

(1) $$U = \frac{\sqrt{\frac{1}{n}\sum (P_i - A_i)^2}}{\sqrt{\frac{1}{n}\sum P_i^2} + \sqrt{\frac{1}{n}\sum A_i^2}}$$

where P_i and A_i are the predicted and actual values respectively. This coefficient is confined to the interval between 0 and 1 with a value of 0 indicating perfect prediction and a value of 1 showing perfect inequality.

The U coefficient may also be calculated for forecasts which are generated by the two naive models;

(2) $$X_t = X_{t-1} \quad \text{and}$$

(3) $$(X_t - X_{t-1}) = (X_{t-1} - X_{t-2}).$$

If forecasts are made in terms of first differences, the predictions generated by the first naive model are always identical to zero

(4) $$(\text{i.e., } \Delta X_t = 0 \quad \text{or} \quad P_t = 0).$$

Consequently, the U coefficient for this naive model will always assume its largest possible value, 1. The predictive performance of any forecasting model which also predicts changes will *a fortiori* always be equal to or better than that of the first naive model. However, predicted changes which are obtained from the second naive model do not automatically generate a U coefficient which approaches either of its limits. Since it is expected that the accuracy of an econometric model will exceed that of naive methods, a model's U coefficient may be compared with a relevant standard, the U coefficient of the second naive model. While no rigorous test has been developed to judge whether the difference between two U coefficients is statistically significant, the U coefficient of the model should be lower than that of the naive method.

II. *Evaluation of the Suits Model*

There are several ways in which the Suits model will be evaluated. First, the forecast *changes* in GNP for a number of years are compared with the observed *changes* by means of the U test. The actual values of the predetermined variables for 1962 are then utilized with the inverse matrix which Suits published to determine the accuracy of the 1962 *ex post* forecast. Any errors are then attributable to the model and not to misestimates of the predetermined variables. Finally, the inverse matrix and predetermined variables contained in the 1962 model are used to generate *ex post* predictions for 1964 by assigning the actual 1964 values to the predetermined variables. This procedure tests whether the structure of the 1962 model was applicable to the 1964 economy.

Suits has prepared a table of the observed and predicted changes of GNP generated by his model for the years 1953-1964. These data may be used for

the U test and are presented in Table 1. The forecasts of Table 1 are the predicted changes which were presented for a given year in November of the previous year based on data for earlier months. They are *ex ante* forecasts and not the *ex post* internal forecasts which would be generated by the present model if the actual values for that year were assigned to the predetermined variables. Second, the predicted changes of Table 1 often differ quite markedly from the forecasts and/or the differences between the forecasts and actual changes which Suits presented in his AER article [1, p. 123]. The discrepancy is the result of including the error in the previous year's estimated level in the prediction for the subsequent year. This error is included in the article but not in Table 1 where only the predicted *changes* are presented. The proper ap-

TABLE 1—CHANGES IN GROSS NATIONAL PRODUCT, FORECAST AND ACTUAL
(Billions of 1954 dollars)

Year	Forecast	Actual
1953	13.0	15.5
1954	−4.5	−5.9
1955	4.4	29.6
1956	9.0	8.2
1957	9.1	7.7
1958	−2.9	−7.3
1959	9.0	27.3
1960	8.5	11.3
1961	7.4	7.8
1962	27.5	27.1
1963	14.1	15.9
1964	23.9	24.1

Source: Research Seminar in Quantitative Economics, "Econometric Model of the U.S. Economy," (Version used to forecast the economic outlook for 1965), mimeo, Table 3, p. 16.

proach, as Suits himself suggests, is to compare only the actual and forecast changes. This is what Table 1 does.

A comparison of the observed and predicted changes for 1953-1964 indicates that the model had only two serious failures. The upswings of 1955 and 1959 were badly underestimated. In addition there also seems to be a modest underestimate of the 1958 decline and 1960 movement. Otherwise, the model appears to have performed remarkably well. This result was confirmed when the U test was applied to the forecasts and actual changes. The inequality coefficient for the model was .30 whereas the naive model's coefficient was .57. Thus, using this test the Suits model must be considered superior to naive methods.[3]

The next test was to insert the actual 1962 values of the predetermined

[3] In 1965, there was again an underestimate of the increase in GNP. The predicted increase was $15.8 billion, the actual was $24.6 billion. These data became available after the U coefficients had been calculated for 1953-1964. If this observation had been included the difference between the two U coefficients would have narrowed since the naive prediction for 1965 was $24.1 billion.

variables into the 1962 model[4] and to generate the *ex post* predictions. The *ex post* predictions are substantially higher than the *ex ante* forecasts of the model and are also larger than the actual changes which occurred. The model itself is only designed to generate forecasts of the change in privately produced GNP, exclusive of imputed services. Consequently the comparisons of the *ex post* forecasts must always be made with similarly defined actual changes in the relevant variables. For instance, the actual increase in GNP which occurred in 1962 was $28.5 billion (in 1954 dollars). This must be compared with the *ex ante* forecast of $27.5 billion and the *ex post* prediction of $35 billion. It is thus apparent that the *ex ante* forecast was much more accurate than the *ex post* prediction. The entire amount of this $6.5 billion discrepancy between the *ex post* prediction and the actual change is attributable to the model's internal errors in forecasting privately produced GNP, exclusive of services. The errors are not attributable to the exogenous variables such as housing, plant and equipment, exports, government expenditures, or services, for they were assigned their actual values. This information is presented in an aggregated form in Table 2 and in a disaggregated form in Table 3.

Several exogenous variables were underestimated in the *ex ante* forecasts, thus producing the near perfect correspondence between that forecast and the actual change. For instance, the *ex ante* estimates for exports, plant and equipment, and housing were $0.0, $1.3, and $0.1 billion respectively. The actual values were $1.7, $3.0, and $1.9 billion respectively. (These data are shown in Table 3). On the other hand, government purchases of goods and services were overestimated by more than $2 billion. Another important *ex ante* underestimate was the increase in OASDI transfer payments. These were estimated at no change; an increase of $1.8 billion occurred. This contributed an increase of about $2 billion to gross private GNP. The evidence thus indicates that when the actual values of the predetermined variables are inserted into the model that an overestimate of the increase in GNP is predicted.

This overestimate can also be observed in Table 3 where the *ex ante* and *ex post* estimates of changes in the disaggregated components of GNP are compared with the actually observed changes. The size of the discrepancy is the same as in Table 1, as it must be, for the same model and inverse matrix were used. However, the interesting phenomenon to be noted involves the predicted change in consumption which is larger than the predicted change in disposable income.[5] The *ex post* predicted change in consumption was $20.5 billion and that for income was $17.7 billion. This indicates that the other determinants of consumption expenditures, such as liquid assets, might have been given too much weight in the model.

It is also important to remember that data are constantly being revised and it is difficult to know which are the "actual" values of the predetermined variables that should be inserted in the model. In our case, we have used as the 1962 values the data for 1962 which were available in 1964. Since these re-

[4] We refer to the Michigan model by year because the model is constantly being revised. The 1962 model was the one which was published in this *Review* in March 1962.

[5] Both consumption and income changes are net of imputations.

TABLE 2—ESTIMATES OF CHANGES IN GROSS NATIONAL PRODUCT FOR 1962: SUITS' EX ANTE FORECAST, EX POST FORECAST, AND ACTUAL CHANGE

(Billions of 1954 dollars)

	Suits		Actual Change
	Ex Ante Forecast	*Ex Post* Forecast	
Change in privately produced GNP (exclusive of imputed services)	25.3	31.3	24.8
Change in income originating in government	0.9[a]	1.8[c]	1.8[c]
Change in imputed service	1.5	1.9	1.9
Total change in GNP	27.7[b]	35.0	28.5

[a] This figure was obtained from Suits' article by subtracting Suits' estimate of government purchases from the private sector from total government purchases of goods and services. The sum of changes in government purchases from the private sector plus the change in government wages is greater than the change in government purchases of goods and services. It is due to changes in government imports and sales to the private sector.

[b] This figure differs from Suits' figure of 27.5 [1] due to rounding errors.

[c] It was impossible to obtain data of government purchases of services from the private sector. Consequently, all of the increase in government purchases of services was attributed to the government sector. This somewhat reduces the *ex post* forecasts of the Suits model and makes the comparison more favorable to the model. The figure which is presented (1.8), is the change in government wage payments to its employees. The figure for government purchases from the private sector which was used in the reduced form equation was obtained by subtracting this figure from total government purchases of goods and services. This estimate of government purchases from the private sector is somewhat lower than what it must have been in actuality (see footnote[a]). The *ex post* estimate of privately generated GNP would have been increased by 1.304—multiplied by this discrepancy, while the actual data would only have been changed by the amount of this discrepancy.

vised data might not be applicable to the 1962 model, it should be noted that this approach has introduced a possible source of error into the evaluation. While this bias is likely to be inconsequential, it must still be noted.

Two additional tests of the model were made. First, the 1962 *ex post* residuals of the structural equations of the 1962 model were examined to determine whether the biases were also observed in these equations. Finally, the 1962 model was utilized to make *ex post* predictions for 1964. Both tests indicated that the 1962 version of the model tended to overestimate changes in GNP.

The analysis (Table 4) of the residuals of the structural equations indicated that all of the consumption expenditure equations were uniformly overestimated or biased. The inventory and import functions, however, yielded excellent predictions. This evidence once again indicates that structural change has occurred and that the inclusion of the liquid asset variable may cause the overestimates. It is also interesting to note that the 1963 version of the model did not include liquid assets within the consumption expenditure equations, but this variable is reintroduced in several consumption equations in the 1964 version of the model and in the automobile equation of the 1966 version.

TABLE 3—ESTIMATES OF CHANGE IN GROSS NATIONAL PRODUCT AND COMPONENTS IN SUITS' EX ANTE AND EX POST FORECASTS, AND ACTUAL CHANGES IN GROSS NATIONAL PRODUCT AND COMPONENTS

(Billions of 1954 dollars)

	Suits 1962 Forecast		Actual
	Ex Ante	*Ex Post*	
Gross National Product	27.5	35.0[d]	28.5[d]
Consumption			
Automobile	4.5	5.0	3.1
Other Durables	1.9	2.8	1.3
Nondurables	5.3	6.5	4.8
Services[a]	5.5	6.2	5.6
Gross Private Investment			
Plant and equipment	1.3[c]	3.0[c]	3.0[c]
Residential construction	0.1	1.9[c]	1.9[c]
Inventory Investment	3.0	4.8	3.5
Exports	0.0[c]	1.7[c]	1.7[c]
Imports	1.9	2.2	2.0
Government Expenditures on Goods and Service[b]	7.8[c]	5.5[c]	5.5[c]
Disposable Income	n.a	17.7	14.8

[a] Includes imputed services.
[b] Does not include imputed expenditures.
[c] Estimated exogenously.
[d] Detail does not add to GNP due to rounding.

TABLE 4—EX POST 1962 PREDICTIONS OF THE STRUCTURAL EQUATION OF THE 1962 SUITS MODEL AND ACTUAL 1962 VALUES OF SPECIFIC EXPENDITURE CATEGORIES

(Billions of 1954 dollars)

	Prediction	Actual	Residual
Automobiles	4.3	3.1	1.2
Other durables	2.4	1.3	1.1
Nondurables	5.9	4.8	1.1
Services (no imputations)	4.1	3.7	0.4
Durable inventory	3.2	3.3	−0.1
Nondurable inventory	0.1	0.2	−0.1
Imports	1.9	2.0	−0.1

When the 1962 version of the Michigan model was utilized to generate *ex post* predictions for 1964, the same overestimates were obtained. This prediction was calculated by assigning the actual 1964 values to the predetermined and exogenous variables which are contained in the 1962 model. The model predicted a $33.7 billion (1954 dollars) increase for 1964. This must be compared with an actual change of $23.4 billion. Thus, all the evidence seems to indicate that the 1962 version of the Michigan model had a tendency to generate *ex post* predictions which overestimated changes in GNP.

III. *Conclusions*

On the basis of our evaluation of the model we may conclude: (1) A comparison on the basis of U coefficients indicated that the model performed substantially better than the naive methods; (2) The *ex ante* forecast for 1962 is substantially better than the *ex post,* which overestimates changes in GNP; (3) The residuals of the structural consumption equations are all positive indicating that structural bias may have existed in this version of the model; the *ex post* overestimate of 1964's change as predicted by the 1962 model reinforces this conclusion; (4) Finally, it should be noted that the observations apply only to the 1962 version of the model. Later versions of the model have not been evaluated.

H. O. STEKLER*

* The author is a visiting professor at the Board of Governors of the Federal Reserve System. The research was performed at the University of California, Berkeley, and was supported by research money provided by Mitchell, Hutchins & Co. He gratefully acknowledges this financial support and the comments which Robert S. Einzig made upon an earlier version of this paper.

REFERENCES

1. D. B. SUITS, "Forecasting and Analysis with an Econometric Model," *Am. Econ. Rev.*, March 1962, *52,* 104-132.
2. H. THEIL, *Economic Forecasts and Policy,* Amsterdam 1961, pp. 32-35.

Errata

In the September 1966 issue, Nissan Liviatan's article on "Multiperiod Future Consumption as an Aggregate" contained some printing errors, which are corrected below:

P. 830, line 11, for: $(1+r_1)C_0$, read: $(1+r_1^0)C_0$.
P. 831, line 1, for: $1/(1+r^0)$, read: $1/(1+r_1^0)$.
P. 833, first equation, delete: $\equiv H(C_1, \ldots, C_n)$.
P. 835, equation (5), for: $T(C_1, \ldots, C_n; K_0)$, read: $T(C_0, C_1, \ldots, C_n; K_0)$.
P. 835, para. 2, line 12, for: $f'(F_0-C_0)f'(K_1-C_1)$
read: $f'(K_0-C_0)f'(K_1-C_1)$.
P. 839, para. 3, line 12, for: value of H, read: value of D.

BOOK REVIEWS

General Economics; Methodology

Economic Advice and Presidential Leadership: The Council of Econo
Advisers. By EDWARD S. FLASH, JR. New York: Columbia Univers
Press, 1965. Pp. x, 382. $8.95.

One wonders whether, on preliminary form, there was ever a more unpr
ising piece of legislation than the Employment Act of 1946. The title it
was a hedge; reference to full employment was too controversial. Any men
of methods by which employment would be sustained was avoided. To h
hinted at deficit financing would have been fatal. Three economic advi
were provided which, it could be foreseen, was a certain formula for e
less disagreement. They had no visible executive authority. A Congressic
Committee was authorized but with no legislative powers, a point emphasi
in its original name. As a substitute for solid function, an impressive systen
reports was provided for. I remember discussing the legislation in 1946 v
Milton Gilbert who was one of its architects. We agreed that it was princi
ly a gesture though probably a useful one. He thought it more useful tha
did.

Yet in retrospect the Employment Act appears as the most important si
piece of economic legislation of the postwar years. The Council of Econo
Advisers which it created has, a few early months apart, functioned har
niously as a powerful general staff on economic policy. It has far outstrip
the Secretary of Commerce and the Federal Reserve Board in influence. P
ably it is now more powerful than the Treasury. The Joint Economic Com
tee, as it has been retitled, if a less spectacular success, is the most respe
forum for economic discussion in the Congress. How did it come about

I should suppose there are quite a few reasons. The President needed
nomic advice. The CEA gave him men of his own selection committed to
success of his administration. Short terms for both members and staff (st
lized by such admirable civil servants as David Lusher) prevented harde
of the arteries. Chairmen were allowed, in general, to choose their own
leagues. Economists joining the Council and staff uniformly abandoned t
occupational tendency to be fancy and worked in a tradition of careful, em
ically-based judgment. A strong sense of professionalism on the part of Art
Burns protected the work of the Council and staff from the destructive pre
ences of such Paleozoic statesmen as Sinclair Weeks and George Humph
during the Republican years. And two men of remarkable energy and pur
—Leon Keyserling and Walter Heller—made the Council exceedingly har
ignore under the Democrats.

This brings me at long last to Professor Flash who has written a com
hensive history of the CEA which is billed also as an analysis of the source
its power. It is reasonably good. The author has gone over the record
care. There is a wealth of chronological detail on the important decisions

also on some of the unimportant ones. He has a competent view of the economic issues.

Yet it isn't perfect. For one thing, a solemn, even lugubrious effort to get at the organizational and administrative verities keeps dulling his points. One of his points is that, in dealing with the President, the Council derives power from its knowledge of economics. It takes a long time to say this. Various of the other sources of the Council's success mentioned above can be found. But again they have to be excavated from under the words. There is also far too much detail on the coming and going of staff or the not terribly historic observations of the members. Nor does everything get in. I should have thought that one of the fairly important issues in the tax reduction debate of 1962 was whether it wouldn't durably impair allocation as between the public and private sectors and be irreversible. If so, more spending would be better. In any case, these contentions came up, some will recall rather tediously, at the sessions I attended.

Finally, the author sometimes allows words not so much to obscure meaning as to eliminate it. Here is part of his conclusion:

> If the experiences of the Council of Economic Advisers are indicative, the knowledge-power relationship implicit in the advisory function is both important and enlightening, and this study of the Council's operations should contribute to a broader understanding of that relationship. It should in all probability suggest an analytical approach that isolates and identifies the operating dynamics and intricacies of the relationship between knowledge and power, adviser and leader.

If this means what it seems to mean, I venture to think that it doesn't mean anything at all. Flash is a man of obvious intelligence. It is too bad that the more bogus traditions of scholarship did not allow him to write a straightforward and succinct history with no pretense.

J. K. Galbraith

Harvard University

Economics: Principles of Income, Price and Growth. By R. Murray Havens, John S. Henderson, Dale L. Cramer. New York and London: The Macmillan Company, 1966. Pp. xvi, 623.

This is a competent principles text for college level courses. It has deficiencies, like all texts. The nonpecuniary justification for another close substitute, like this one, must therefore run in terms of nuances that appeal to the tastes of particular instructors or to students. One can certainly discount at the outset, however, the authors' assertions that the assumptions employed are "realistic." Almost everybody claims this metaphysical virtue.

According to the blurb on the cover, the product is differentiated chiefly by "particular attention to the concept of growth . . . examined as an integral part of the whole subject rather than as a separate entity." I agree that the book scores on this account, a feature that will become increasingly popular in introductory texts, it seems to me. In addition to a modest, helpful interweaving of growth aspects at a number of relevant points, there is an adequate spe-

cial section on the growth topic as such. On the other hand, the book lacks even a modicum on the growth problems of less-developed economies. This is a serious omission to me; but omissions of particular areas of the subject are, admittedly, a matter of taste.

Why not contrast this omission with the book's more than 100 pages (one-sixth of the total) of capable discussion on international "trade"? Isn't this too heavy a weight on that special topic? Doesn't such weight reflect the interests of the authors rather than the "balanced needs" of students? Is it justified, as the authors claim, by "the growing importance of the international sector" (like merchandise exports relative to GNP in 1929 equal .05, and in 1965 equal .04?)?

Two other features of the work seem to me important for an appraisal: (1) national income determination is tied in with monetary theory, and (2) the presentation of price theory (properly called "value theory") is built largely around imperfect competition rather than pure competition so far as the production side of the market is concerned. Both of these are to be hailed. The separation that has so long prevailed in textbooks between income determination theory and monetary theory has caused endless technical confusion in the minds of students; and the detailed elaboration of the purely competitive theory of the firm and the industry, presented as positive theory and left on the level of first approximation, has left generations of students baffled and repelled by the subject. Let's have more of the different approach in this textbook.

Economics a la Messrs. Havens, Henderson, and Cramer moves from macro to micro, from price theory to distribution theory, and from value and distribution to its two, and only two, applied areas: international economics and growth. I doubt that rearrangement by an instructor would work out. You have to take it this way or leave it, a framework probably approved by most.

This text is a serious candidate, and the specific adverse criticisms among these that follow are not intended to alter that judgment. The macro discussion proceeds, despite an explicit treatment in one of the several "high level" appendixes, without benefit of the distinction between scheduled and realized magnitudes. As a result, to take one example, the concept of aggregate supply is initially (e.g., pp. 53, 55) thoroughly clobbered: the poor reader is told that it is the 45° line, and that it is simply "real income." Later, things are patched up, and another type of aggregate supply function, having a different purpose, is introduced, but I believe the damage will by then have been done. The effective lack of a schedule concept appears to me to be the most serious technical weakness of the book. In an "aside," the authors venture the judgment that the use of scheduled magnitudes, as distinguished from *ex post,* involves "misplaced ingenuity"! On the micro level, the schedule seems to have at least a *nominal* existence (e.g., pp. 38, 42).

On the positive side, in the theory of production and revenue, the physical input-output relationships, on the assumption of short-run diminishing returns, are first presented, *then* the *same* cost functions are used in connection with the construction of the standard revenue relationships. This is excellent pedagogy. How can a student understand the shape of the standard cost functions if he or she doesn't yet know the input-output functions from which

those cost functions are drawn? Probably only Samuelson can unravel this pedogogical mystery. Unfortunately, explicit reference to a production function is made only much later (pp. 381 ff.), and not tied in with the earlier input-output, diminishing returns table.

Now for a grab-bag of running comments: the investment concept bypasses human training, despite the ground swell of current argument in favor of its inclusion; the assertion of a tendency toward equilibrium is unfortunately at times considered to make equilibrium empirically the "most likely value"; the obsolete term "business man" is used to refer to the top decision-making individuals in a contemporary firm; the owners of corporations make income payments to factors (via representative democracy?); pure profits are "connected" with uncertainty, but just how is still a mystery (and it ought to be told); the treatment of government deficits as savings offset is made bland by a lack of reference to historical controversy; differentiated oligopoly suffers at the expense of pure oligopoly; the definition of oligopoly spans 14 pages (fewness on page 300, recognition of mutual interdependence on page 314); "industry equilibrium under oligopoly" is, perhaps not unreasonably, a heading without a treatment; clock time and analytical time are not clearly distinguished; changes in scale and in technology are not analytically disengaged; the long-run planning curve of the firm turns up in time to save competition, and for the same old story-book reason (diseconomies of large management); the creation of deposits is explained by using the excellent T account method; and there is, happily, a moderate amount of factual and institutional material despite the authors' allegation that such material is minimized. There is, as usual, no analysis of urban problems, or of regional growth phenomena, and no population or location theory.

This book is rather nationalistic, even though it has a big section on international economics. I think the special areas section ought to treat not only the problems of development of less developed countries, but also comparative systems. It could be taken in part from the international trade section. We all have separate courses in that important subject. The rest of the required space could be obtained by making the book larger.

Like all texts, this has its deficiencies; but it is in general a competent candidate.

HAROLD G. VATTER

Portland State College

Economic Enquiry in Australia. By CRAUFURD D. W. GOODWIN. Duke University Commonwealth-Studies Center Publication No. 24. Durham: Duke University Press, 1966. Pp. xv, 659. $12.50.

Professor Goodwin has made a comprehensive study of Australian economic enquiry, both popular and scientific, from the early colonial period to the onset of the Great Depression in 1929. In presenting his findings he has adopted a similar formula to that employed in his companion volume on Canadian economic thought, published under the same auspices in 1961. The material is divided into two sections, the first of which deals with the application of imported economic ideas to the policy problems of a developing country,

while the second charts the emergence of professional economic studies in Australia. The only other work with which this book can usefully be compared is Professor La Nauze's *Political Economy in Australia* (1949), which is a more concise work concentrating on three figures, Jevons, Hearn, and Syme, whose reputations extend beyond Australia. Goodwin's main object has been "to identify the presence of ideas and methods of enquiry rather than to assess the relative importance of individual contributors." Some incidental evaluations are made of the quality of reasoning and insight displayed in discussion, but for the most part he is less concerned with the history of economic analysis than with the history of economic opinion and theory in its relationship to policy and environment. Since he believes that the economic attitudes which the settlers brought with them or acquired through experience were important in determining the pattern of Australian development, he describes his work as "an essay both in economic history and in the history of economic doctrines."

For much of the nineteenth century, quite understandably, Australian economic literature was the by-product of policy debate, with the works of better-known writers in the mother country and America being used mainly as texts to be drawn on to justify or condemn particular courses of action. In a series of chapters which cover such topics as tariffs, land disposal, money and banking, economic fluctuations, and railway economics, Goodwin documents the wide variety of positions adopted, and the uses made of borrowed theory in Australian controversy. Although the agenda differed somewhat, the Australian policy debates mirror those taking place elsewhere at this time; none of the propositions of neo-classical marginalism, however, seems to have spoken to their condition. It is perhaps surprising to note the extent to which the laissez-faire tradition exerted a continuing, and frequently inhibiting, influence in the new setting.

While much of the literature considered here is ephemeral, in some fields Australia was well served by her economic commentators. This was the case, for example, with railway economics where, despite the limitations of contemporary thinking and technique, the informed public was presented with substantial accounts of the alternative costs and benefits of different types of railway development. The "amateur" in the railway and public utility field, it would seem, performs better than his counterpart in monetary discussion. Though Australia had its share of heretics, the chapter on economic fluctuations shows the tenacity of the doctrine of the impossibility of overproduction. The Australian writers were handicapped by their isolation, by the inadequacies of the existing corpus of knowledge, and by the comparative novelty of their condition. But is the situation so different today when modern communications place the latest techniques at the disposal of developing countries?

Acute problems of arrangement and emphasis are encountered in handling a mass of evidence drawn from popular and semipopular writings over such a large time span, especially where there are no dominant figures or schools of thought to give backbone to the account. Goodwin's method has been to treat each policy topic separately and chronologically. This involves a certain amount of repetition, with the reader having to revert to the beginning of the

period with each new topic. There is also some danger of the reader losing sight of the larger issues posed at the beginning of the study. By the same token, however, the arrangement may be convenient to those who wish to refer to particular questions.

In the second part of the book the rise of an objective science of enquiry is traced. After a lengthy chapter on the collection of economic statistics, Goodwin digresses from his stated aim of not considering individual contributors by drawing attention to the work of two unorthodox writers in the field of macroeconomics. The chapters devoted to the struggle to gain recognition for economics in the universities provide interesting comparative material for those with a taste for the history of academic communities. Given opposition from sections of the general public who associated economics with one cause or another which was unacceptable, it is perhaps not surprising that the early curricula should have been so unenterprising; the more adventurous courses were given outside the universities. La Nauze's conclusion that the history of economics in Australian universities during their first sixty years is an "arid story" is amply confirmed.

This was a bold undertaking which adds to our understanding of the transference of economic and social ideas. It is the kind of contribution to intellectual history that only an economist could have made. It may also shed light on Australian economic history, though it must be said that Goodwin is more successful in showing how economic circumstances shaped Australian thinking than in demonstrating the more difficult process by which ideas influence action.

DONALD WINCH

University of Sussex

Economics. By RICHARD G. LIPSEY AND PETER O. STEINER. New York: Harper & Row, 1966. Pp. xxxii, 760.

Does a new textbook have to be significantly different from existing ones to justify its publication? Apparently many economists believe so. Reviewers of textbooks in this journal often begin by describing the features of the new book that justify its publication; it's up-to-date, it uses more or fewer graphs, micro is taught before macro, it contains material gleaned from recent journals, etc. Not only reviewers, but authors and publishers are similarly eager to demonstrate that their new book has something different.

But it is probably more important to know if a new product is substantially similar to existing ones than to know how it differs. Does it do what existing ones do? Is it just about as good or bad? Let me begin then by stating that I believe Lipsey and Steiner is not very different from the *leading* textbooks now in use in the United States to teach elementary economics. It covers the usual topics that most of us consider the elements of economics: micro, macro, international trade, growth, and development. The level of difficulty is about the same. The material is usually well organized, the writing is always lucid. Some people will like the competition better than it, but it should find a substantial group of admirers. And it would be just about as difficult to predict

who will fall into one group and who into the other as it would be to predict who will smoke Kents and who Marlboros.

Lipsey and Steiner, like any good book, has strengths and weaknesses. I can only give generally the things that I liked about it best. The value and distribution sections are second to none. The authors resist better than most implying that economists know more than they really do. No student should put this book down with the feeling that economics is a pat subject. They should have the sense that something has been learned, but that almost nothing is certain. And, if the attitudes of Lipsey and Steiner are contagious, students should be optimistic about the discoveries yet to come from careful scientific enquiry and some of them will be eager to make a contribution.

The authors are more explicitly concerned with methodology than is usually the case. They insist again and again that economics must be concerned with positive rather than normative statements. And this is all to the good. What they fail to do is indicate that this is not easy to do. Economists cannot explore all the implications of any action. They must restrict their attention to important economic effects. And the economist himself must decide what is important and what is economic. In making this decision, values creep in where only analysis is supposed to dwell. Difficulties also exist when the economist tries to restrict his attention to whether a particular policy will achieve its objectives, for who is to decide what the objective of a policy is? If an economist believes that Congressmen vote for minimum-wage laws because they believe that it will lead to their gaining more votes than they will lose, is it his task to sample public opinion to see if the legislators are correct? And even the way a conclusion is stated may determine the reaction to it. For example, the authors argue that the monopolization of an industry may reduce output and raise price. But they could just as well state, assuming full employment of resources, that the monopolization of an industry will lower prices and raise output in the rest of the economy. The reactions to the two ways of stating the conclusion would differ. The economist cannot state every conclusion every possible way; what he thinks ought to be is likely to determine how he states his results.

The book contains some mistakes and sometimes it misleads. The authors argue that, in the absence of rising factor prices, average and marginal costs for a firm are constant in the long run. Since if a firm doubles all inputs it can at least double output, costs will only increase if factor prices increase. This argument misses an essential point. What is at issue is what happens to costs for *a* firm in the long run. If *everything* is doubled, where there was one firm before there would now be two. The very notion of a firm implies that something is fixed. If a firm doubles its productive capacity, it will not, if it is to retain its identity, operate with two chairmen of the board of directors. A farmer can vary his output by adding more inputs; he can double his land, seed, fertilizer, etc., and in some societies he can add a wife and have more children. But it would be unrealistic to assume that he can also add a husband.

The reader is told that the purchase of a $1,000 machine that will last for ten years and that will produce an annual income of $120 after wage, raw ma

terial, and depreciation costs, yields a 12 per cent return. As they put it, $120 is 12 per cent of $1,000.

The authors conclude that a lump-sum tax, a per unit tax, or a tax on accounting profits is entirely passed onto the consumer in the long run in perfect competition. Although they have a superb discussion of economic rent in later chapters, they overlook the burden that will fall on specialized factors.

In Chapters 37 and 41 they argue that changes in relative prices are the crucial factors in achieving equilibrium in the balance of payments. Devaluation cures a balance-of-payments deficit by altering relative prices, not by reducing demand in the devaluing country and raising it elsewhere; what happens to aggregate demand in relation to aggregate supply is not mentioned. Only later, after their presentation of national-income analysis, do they introduce the possibility of correcting a deficit by reducing demand.

Lipsey and Steiner insist that economists cannot validly argue that a twenty per cent reduction in tariffs will raise national income, but they can argue that we are better off today with trade than we would be without it. If they mean by this that, because of obvious cost differences in different countries, economists can argue that world output could be made larger with some trade than with no trade, I suspect that no one would disagree. But they seem to mean that we are clearly better off at present with the existing volume and composition of trade than we would be without any trade. But this conclusion is no more established than the one about the gain from a 20 per cent tariff reduction.

The short-run supply curve for an industry is said to be the horizontal sum of the supply curves of all the firms in the industry. They do not mention that this depends on the unlikely assumption that factor prices are constant to the industry in the short run. They argue that the marginal revenue product of a factor must decline if the demand curve is negatively sloped and if there are diminishing returns, since marginal revenue will decline if the demand curve is negatively sloped. But if elasticity increases as demand decreases, marginal revenue can increase when price falls.

The distinction between the *ex post* and *ex ante* equality of saving and investment is ignored. The necessary equality of saving and investment is not mentioned anywhere. They assume in the beginning of their analysis that inventories are constant. Presumably this assumption is made to avoid this discussion, although the reader is not told why. And in one section of the chapter in which inventories are assumed constant, they are clearly allowed to vary.

Chapter 51 on Government and the Circular Flow of Income is the poorest chapter in the book. The possibility of government financing of deficits by printing money receives virtually no attention. The distinction, from the point of view of the national debt, of financing a deficit by sale of bonds to the Federal Reserve Banks as opposed to sales to the public is ignored. The material in this chapter and the further association of prices with the money supply—Part X is titled "Money, Banking, and Prices"—will, I am sure, leave the impression that an increase in the money supply is necessarily inflationary. If, after reading this book, students were asked to comment on the view that depressions should be cured by government spending, but deficits should be

financed by the sale of bonds rather than the creation of money, since the latter would only be inflationary, the results would be almost universal agreement. The treatment of the burden of the debt in this chapter will raise the hackles of many. Although the authors devote most of their attention to the case in which the sale of bonds leads to a reduction in consumption, they explicitly—ignore the views of Bowen, Davis, and Kopf (this *Review*, September 1960). And why consumption should be altered by the sale of bonds is not even hinted at. The chapter on Household Consumption gives no reason to expect any relationship. They also argue in this chapter that there is a limit to the debt, but their demonstration is wrong. They assume that the government's deficit is equal to 5 per cent of national income every year and that income grows at 2.5 per cent a year. They conclude that eventually interest payments on the debt will exceed the national income, although Domar showed years ago that interest payments as a fraction of the national income would approach twice the rate of interest.

The authors devote considerable attention to the Quantity Theory. In their view this theory asserts that any excess supply of money leads directly to an increase in the demand for commodities; none of the excess supply will appear in the securities markets. I do not know where in the "classical" literature they would find the justification for this proposition. In equilibrium, desired money balances may be proportional to expenditures, but this does not imply that excess money holdings must lead to a direct increase in the demand for commodities. In many versions of Keynes's *General Theory*, prices are proportional to the money supply, if full employment is assumed. Ricardo examined the case in which banks increased the money supply by creating new currency. He concluded that prices would be proportionally higher, but the money did not, in the first instance, lead to an increase in the demand for goods; the money first appeared in the securities markets.

The errors in Lipsey and Steiner do not seriously detract from the book. Who can write 735 pages of economic principles without making a mistake? If economics cannot be learned better from Lipsey and Steiner, it can be learned as well.

ARNOLD COLLERY

Amherst College

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Capital and Growth. By JOHN HICKS. New York and Oxford: Oxford University Press, 1965. Pp. xii, 339. $5.00.

This book is unmistakably Hicks. It has, naturally enough, the vices of its virtues. Every idea is thought through from first principles, almost as if it had never been thought before; and there are very few references to the literature, even when the literature has taken the problem further than it is carried here. Complicated problems are explained in words of one syllable; and two-sylla-

ble problems are slighted a little. Still, the virtues far outweigh the vices. Anyone who wants to introduce himself or his students to the large volume of recent work on the structure of equilibrium growth will turn to Hicks. This is exposition made into a fine art. And it is more than that; there are some interesting new ideas as well. One could hardly expect otherwise.

The best part of the book, I think, is Part II on Growth Equilibrium. It covers the Robinson-Sraffa-Samuelson theory of steady-state growth with discrete production processes, and covers it with great clarity and understanding. The main tool is the Samuelson factor-price frontier, or wage equation, as Hicks calls it. Among the good things, I mention only a clear statement of the duality between the price and quantity equations—though Hicks does not exploit it fully as Michael Bruno has now done—and a novel treatment of income distribution in steady states. There is a chapter on what Hicks calls Traverse, the process of moving from one steady state to another. This chapter doesn't get very far; but it is an intrinsically difficult problem and no one has got much further. Even so, it contains a neat piece of analysis showing that when the "Uzawa condition" is not satisfied, so that the capital-goods industry is more capital-intensive than the consumer-goods industry, there is no full-employment path leading from one steady state to another with a different savings rate, and a correspondingly different growth rate.

In this context, when he comes to introduce saving behavior, Hicks limits himself to the two assumptions that have dominated the discussion so far: savings are proportional either to total output or to profits. It would be more natural to connect savings explicitly with the stock of wealth, in the manner of Modigliani and much of the recent econometric literature. Fixing the wealth-income ratio, say, could hardly affect the characterization of steady states very much, but that sort of saving function could make a considerable difference in the analysis of non-steady state behavior. Indeed, my colleague Karl Shell thinks that this may account for the unstable behavior that has been observed by Hahn in certain multisector economic models with mechanical saving behavior. In such models, the assumption that expectations are fulfilled generates paths that diverge from the steady state, except for very special initial conditions. Shell conjectures that if producers maximize present value and savers maximize utility, full competitive equilibrium leads to the steady state.

It seems clear to me that at this stage of the game the difficult job of studying non-steady state behavior should have the highest priority. Hicks turns away from it with a paragraph that I want to quote at length. "The kind of analysis which has emerged from our discussion of the Traverse could no doubt be carried further; but I shall not make any serious effort to carry it further in this book. It would evidently be necessary, if we were to elaborate it, to introduce some particular assumptions about investment policy: to assume that businesses, in carrying out their difficult task of adapting an inappropriate capital stock into something more appropriate, will go about that task in some particular way. But it is hard to see what particular assumption of this kind there can be that is especially deserving of study. Even in macro-

analysis . . . there are no compelling principles that can be relied upon to perform this function; there, however, it was possible to find some simple rules (capital stock adjustment and so on), the consequences of which it was at least of some interest to work out. Similar exercises could presumably be performed, even with a disaggregated model; but it would be blatantly obvious that they would be no more than exercises.Perhaps this is unduly pessimistic; all I ought to say is that I myself do not see any interesting way of taking this analysis further."

Every economic theorist has had that feeling; yet sometimes I think it mistakes the beginning of economics for the end. When we run out of compelling principles, we may have to steel ourselves to settle for merely accurate ones.

Part III is called Optimum Growth. It contains an exposition of the von Neumann model of expansion, a discussion of the Turnpike Theorem, and some remarks on the Ramsey problem, the problem of defining and characterizing an optimal path of capital accumulation. The treatment of the Turnpike Theorem really does succeed in getting at the heart of a complicated result in a clear and elementary way. And the discussion of the von Neumann model is only a bit less successful. These chapters, and the ones preceding, make accessible a whole range of difficult and important modern theory.

The chapter on optimal saving programs is not so good. It does not take sufficient account of the most recent advances in this field, perhaps because many of them are still unpublished. Nor are the less fancy methods Hicks would like to apply really adequate to the task. He criticizes Ramsey and his contemporary followers for the assumption they all make, that consumption bundles at different points of time are independent goods in the usual sense. But it seems a little beside the point to insist that the assumption is not true, because none of those who use it actually believes it. They would gladly substitute a better assumption if the mathematical techniques were available. The important question is whether the theory gives wrong results on that account. It is of course true that an intertemporally additive utility function fails to penalize irregular consumption paths, and fails to penalize falling, as distinct from low, consumption. But since the optimal paths generated under the independence assumption always show smoothly increasing consumption over time, it is hard to believe that introducing those penalties would make a lot of difference.

Part IV, "After Growth Theory," contains two quite independent chapters. The first has to do with the place of money, securities, financial intermediaries and a hierarchy of interest rates in a growing economy. There are some perceptive remarks, but the argument suffers from the absence of Tobin and Gurley-Shaw. The last chapter is one I ought to hold near and dear. It is about the conditions that need to hold in a many-good economy for one to be able to define a "production function" in "labor" and "capital" whose partial derivatives will do the right things. But I am too much of a rank methodological opportunist to care. I have never thought of the macroeconomic production function as a rigorously justifiable concept. In my mind it is either an illuminating

parable, or else a mere device for handling data, to be used so long as it gives good empirical results, and to be abandoned as soon as it doesn't, or as soon as something better comes along.

ROBERT M. SOLOW

Massachusetts Institute of Technology

Theory of Production. By RAGNAR FRISCH. Chicago: Rand McNally; Dordrecht: D. Reidel, 1965. Pp. xiv, 370. $11.00.

It is good to have Professor Frisch's influential lectures on the theory of production at last available in English. They provide an authoritative restatement of the fundamentals of the classical theory, for a world of many commodities. Elementary mathematics is the natural language, and is much used. The style is crisp, direct, and unashamedly technical. The volume is a treatise rather than a textbook, with few embellishments to entertain the reader, who must be charmed rather by the rhythm of careful and exact reasoning. Occasional eccentricities of terminology and a rather sketchy treatment of the intertemporal and dynamic parts of the theory do little to detract from the evident distinction of the work.

One may, however, regret that it has become available so late. In his preface, Professor Frisch anticipates the charge of being unfashionable. "These good old types of analyses we shall however never be able to dispense with," he says, referring to the marginal analysis. "We will in the end always need to fall back on them." The programming methods of the new economic theory find no place. The appearance of Frisch's book might encourage us to consider whether the older language of production functions and marginal products provides easier methods or more truths than the newer ways.

The lectures are based on three distinctions. First, the distinction between technical and economic considerations; second, the distinction between the quantity-adjuster (or perfect competitor) and the pure monopolist (other cases are left aside in these lectures); third, the distinction between a smooth (everywhere differentiable) production function, and one that is not (called here the distinction between continuity and limitational factors). The one-product producer is studied first. The various technical possibilities of production are explained in detail, with emphasis particularly on the case where production is eventually a decreasing function of any particular factor, and on the technical possibilities that will, when economics is brought upon the scene, lead to constant costs or a U-shaped cost curve.

In the economic sections, the marginal relations describing cost-minimizing and profit-maximizing behavior are derived, both for the perfect competitor and for the monopolist. These are discussed in considerable detail. The case of a nondifferentiable production function is treated separately, but in fact the treatment is restricted to the case where all smooth parts of the production frontier but one are parallel to coordinate planes. Other cases are dismissed, surprisingly, as exceptional (p. 250).

The concluding chapters deal with depreciation and what the author calls reinvestment—the process of continual replacement to keep the size and com-

position of the capital stock constant. It is odd to find depreciation treated without any mention of interest rates. Since the rate of depreciation of a capital good is the rate at which its value is falling, it surely cannot be evaluated unless one knows interest rates now and in the future (or the market tells us the second-hand prices without our having to do any calculations). I am afraid that the student might be misled by Frisch's treatment, especially when he adds together the rates of capital service available from a given capital population in *different* years. It would be an unusual producer who regarded output in each of two years indifferently.

Finally, we must ask: does Frisch vindicate his claim that the traditional way of doing production theory, with a great deal of complicated differentiation, is the fundamental way, or the best way? I am not persuaded. It might be claimed that the presumably important case of U-shaped costs cannot be dealt with by the linear theory of production. Yet, although no treatment seems to have been published, it is quite easy to see how it would go; and the convenient language of concavity and convexity, of production sets and supporting hyperplanes, would provide a more direct route to the useful conclusions. It is, I think, one of the advantages of the modern theory that it forces one to consider what results one wants and for what purpose. We do not want systems of equations for their own sake. We want to know how a firm could calculate its best policy from the kind of data it actually has available; and we want to formulate results about the way in which output supplies and input demands depend upon the economic environment of the producer. There are really rather few results of the latter kind in the traditional production theory: the more special ones are conveniently derived by calculus, but the important general ones are usually more readily—and more generally—proved by the modern theory. As for computation of actual production policies, the methods of programming have proved much more powerful.

Yet the traditional theory of production is an impressive edifice, and Professor Frisch has given an impressive account of its fundamentals.

James A. Mirrlees

Trinity College, Cambridge

The Theory of Profit. By D. M. Lamberton. Oxford: Basil Blackwell, 1965. Pp. vii, 211. 37s. 6d.

This closely written book may be viewed as a study of the theory of the firm, rather than of *the* theory of profit, for the author is concerned with "the firm's major areas of decision, pricing and investment, in an attempt to establish the relationship between the nature of the firm and profit" (p. 7).

The point of view is disclosed in the preface:

> The organizational model of the firm which I have suggested raises serious issues for current controversy about the theory of the firm itself. I believe that future development should be away from the traditional static, welfare-oriented approach to business behaviour and toward a theory of disequilibrium behaviour of the business firm.

There is much merit in this position, or in the view that "economists have

begun to show a rudimentary awareness of the importance of organization and information but hitherto there has been no attempt to probe the significance of these resources of the firm for the theory of profit" (p. 16). Many "orthodox" economists since Marshall would probably have accepted this proposition. Still, Lamberton may be right in his emphasis.

Along with competent surveys of the state of profit theory, Lamberton offers an interesting analogy between organizational change and technological change, as well as the effect of uncertainty on a theory of profit maximization. While these ideas, too, are not novel, Lamberton seeks to weave them into a more inclusive theory.

More dubious are propositions to the effect that "a management team will be more proficient the longer it has been a going unit" (p. 35), or that, rather than speak of the firm or the entrepreneur, we adopt the view that "in particular emphasis is [to be] placed upon organization and information" (p. 41). That this becomes distressingly vague is shown by his argument that:

> But in all this activity it is the organization that is important. The emphasis should be shifted from the elusive entrepreneur to the firm, and to the study of the firm's perceptive capacity, communication system, and response mechanism. For the time being it seems wise to seek only a theory of decision-making . . . (p. 35).

I suspect that we shall still find ourselves talking of firms—as Lamberton does—and of entrepreneurs or entrepreneurial groups. To be sure, they may now become "managerial teams." Maybe the new name is better. But it is not a new *idea*.

Profit maximization, satisfactory profits, and sales maximization are considered as among the firm's objectives. While empirical studies are cited, the analysis of this part is inevitably inconclusive (Ch. 4). The chapter on "The Investment Decision," which considers marginal efficiency of capital and accelerator notions, will be useful as a guide to the literature, tying together some of the macroeconomic studies to the theory of the firm. In an era of multiproduct firms Lamberton does well to remind us that "details of profit on a product, rather than a firm, basis need to be known" (p. 145).

The final portion of the book is devoted to an interpretation of profit statistics as given us by the accountant; much of the problem comes in extracting imputed interest, rent, and excessive managerial salaries out of the totals—if we are ever clear on what "profit" really is. A chapter is devoted to the relations of market position, pricing, and investment. Much more work is needed on this set of topics for they contain little short of the workings of the enterprise economy.

While it is possible to conclude that Lamberton's own views on the theory of profit are not communicated with the clarity he intends, this is a stimulating book on the central concept of the capitalistic economy. Many incisive ideas can be derived from it and pursued in greater detail. Having sensed the elusiveness of the subject, Lamberton should be well prepared for a more definitive statement of his views.

It is a pity that recent studies of macroeconomic determinants of profit mag-

nitudes are omitted; these may be very important in revealing the operations of the enterprise economy; they deserve interpretation at the level of the firm. Empirical evidence on the near-constancy of factor shares also begs for evaluation in terms of the theory of the firm.

SIDNEY WEINTRAUB

University of Pennsylvania

On Competition in Economic Theory. By P. W. S. ANDREWS. London: Macmillan; New York: St. Martin's Press, 1965. Pp. ix, 141. $3.25.

Mr. Andrews criticizes received static micro theory on numerous scores and feels that "economics will have to take a good hard look at its methodology and where it has been getting." He is in search of "a framework for further work on individual industries of a truly analytical kind, in which relevant factors in the internal structure of the firm would be linked with the analysis of the external circumstances in the industry." He is highly concerned about the "apparent victory of the theory of the firm" at the expense of a theory of industrial structure. He is critical, too, of the assumed falling demand curves of the firm with their "implicit theorizing," the treatment of selling costs and advertising which assume consumer irrationality, the analytical independence of cost and demand functions which marginal equilibrium theorists take for granted, and other aspects of micro-equilibrium analysis.

The framework and elements of Andrews' own approach are intermingled with his review and criticisms of static micro theory. Interpreting freely, the following preconceptions and elements seem to be basic for Andrews:

1. Consumers and buyers are assumed as "rational;" consequently, products and product differentiation are assumed as "real," and advertising and selling are assumed to be basically "informational," not merely wastefully persuasive.
2. Oligopolies and oligopolistic interdependencies are generalized throughout the economy; the collusive solution of some economists in received theory is rejected, however.
3. He has high confidence in "open competition" defined in terms of the entry of new competition both within given established industries and cross-entry from *other established* industries.
4. The "ring fence" about oligopoly within which economists play their games is removed by actual and potential open competition. Thus, the special connotations of oligopoly in equilibrium theory disappear; short-run disequilibrium must be analyzed in terms of industrial structure.
5. The demand curves facing the individual enterprise are not derived directly from consumer demand but from the supply prices of actual or potential competing suppliers.
6. A case-by-case approach is necessary in terms of the important particular variables and structural relations even though it may obscure the niceties of full price-output equilibrium in favor of a looser conceptualization of short-run equilibrium or balance with environment.

This reviewer is in agreement with most of Andrews' criticisms of static

micro theory, and with much of his own positive approach. Structural analysis in the setting of the environment of the enterprise is both reasonable and necessary. It would have been helpful, however, if Andrews had been more explicit as to the character and requirements of his own industrial structure approach. Does he have in mind the market structure approach of the so-called field of "Industrial Organization" in the United States? There is a notable lack of reference to this U.S. literature. There are references to some of the contributors, but only in terms of Andrews' own writing—as Bain on entry. Chamberlin appears and reappears throughout the volume, but not as a precursor of market structure theory but to the contrary.

One of the most interesting and, perhaps, revealing aspects of Andrews' stimulating suggestions and approach are his conclusions supporting resale price maintenance. He stands among a relatively small minority of economists in his position. Perhaps, the reasons for his favorable view may provide the key as to the specific nature of his own approach. His endorsement of resale price maintenance, insofar as I could determine, stems largely from his view that (1) small and medium sized manufacturers often can and should be maintained among actual and potential competitors, and (2) such survival and continuation requires the presence of specialized dealers offering valuable pre-sale services (as in the book trade) that would be lost or weakened in competition with large, diversified, limited service enterprises using price-leader tactics. Andrews uses the Net Book Agreement in England to illustrate how received static micro theory based on Chamberlinian large group ("monopolistic competition") analysis has led the majority of economists into allegedly unsound recommendations with respect to public policy.

Now, Andrews' approach in terms of the impact of open competition, especially cross-entry in competition, and the difficulties, if not impossibility, of establishing cost functions for individual products in retail competition, could lead to just the reverse conclusion when couched in terms of competitive functioning under given market structures. Andrews' own judgment and recommendations would appear to reflect a social judgment as to the public benefits derived from encouraging and maintaining a larger number of small and medium sized producers and of specialized retailers than might occur if resale price maintenance were prohibited.

In essence, it would appear that the lack of sharper positive formulation in Andrews stems from a relative overemphasis upon the particular industrial facts and of judgments of public benefits in contrast with a framework in terms of competitive generalization. Along these lines it is extremely difficult for the most determined, objective scholar to avoid the pitfalls of rationalizing the business *status quo*. The formulation of a general theory would appear to require that the approach be oriented first from a framework of generalization about competitive functioning, modified then in each case by special factors, rather than the reverse.

E. T. Grether

University of California, Berkeley

Capital Longevity and Economic Growth—An Analytical Study. By S. K. BHATTACHARYYA. Calcutta: Bookland Private Ltd., 1965. Pp. vi, 146. Rs. 20.

The book under review is, with minor changes, the author's thesis for the Ph.D. degree at the University of London. The study sets itself the task of putting "the relationship between capital longevity and economic growth on a more integrated and realistic basis than has been the case so far." This is an interesting and important problem since theoretical studies of optimal life of capital equipment have usually been carried out at the micro level, while the corresponding problem has been given a rather superficial treatment in macroeconomic growth models.

As preparatory work the author surveys some of the microeconomics of capital longevity (the term used for economic lifetime, in contrast to "durability," which is used for physical lifetime). He criticizes earlier approaches for neglecting variations in operating costs—or rather the relationship between variations in repair and maintenance expenses and the output capacity of equipment. The author's attempt to remedy this is suggestive, but a precise and convincing theory is not presented. Then follows an attempt to include the impact of technical progress. There is no doubt that the author comes out with the correct general conclusions, but the analysis is a little bit obscure because of an unjustified use of "the law of indifference in the market." The missing link in the analysis is some explicit assumptions about foresight or expectations.

The macroeconomic analysis is opened by a survey of growth models which deal explicitly with technical progress and depreciation in some form or other, covering models by Solow, Domar, Haavelmo, Strumilin, and the present reviewer. Many recent extensions of these models (by Massell, Kurz, Phelps, Kemp, Thanh, and others) ought to have been included, but were probably not available when the thesis was written. The survey is competent as far as it goes, emphasizing the aspects of the models that are most relevant for the author's problem, but one might wish for more careful economic explanation of some points. For instance, in connection with a growth model by the present reviewer in which *gross* investment is assumed to be a constant fraction of gross income, the author comments on the dependence of the growth path upon the rate of depreciation (or average lifetime of equipment) without discussing to what extent this dependence is due to the effects of the rate of depreciation on the *net* investment ratio. The more original section of this chapter is an attempt to interpret Strumilin's analysis of "The Time Factor in Capital Investment Projects."

In his survey the author finds that the savings ratio and the rate of deprecition affect the asymptotic growth rate in some models, while they do not so in others. He shows how this is related to the form of the production function. Furthermore he discusses why there will in some models, but not in others, exist an optimization problem with respect to capital lifetime. This discussion is well taken. He goes on to extend some of the models by introducing a spe-

cial cost function for the manufacturing of capital equipment of different durabilities. This, of course, tends to create more of an optimization problem with respect to capital lifetime.

Finally—in the most original chapter—the author introduces a hypothesis according to which there is a trade-off between initial investment costs and later repair and maintenance costs. The implications are discussed both at the micro and macro level. The macro model is an adaptation of the reviewer's growth model (*Econometrica* 1959) in which there is a choice-of-technique-function with substitution between labor and capital at the stage of designing equipment, while factor proportions are "frozen" after the equipment is installed. It appears that the asymptotic growth rate is independent of the choice of the ratio between investment costs and repair and maintenance costs, and also of the "rate of retirement." However, in both these respects there is scope for optimization as regards the *level* of the asymptotic growth path. This optimization is convincingly carried out, and some interesting conclusions are obtained. I should, however, like Bhattacharyya to extend his analysis further, viz., to make an attempt to "translate" his formulae at the macro level back to the micro level so as to see if some natural and comprehensible rules of decisions with respect to retirement and repair and maintenance vs. initial investment costs at the micro level could be given so as to conform with his macro optimization. Furthermore, one might wish for an analysis which would not concentrate only on the asymptotic results.

Most of the mathematics is presented in appendices. At least on one occasion (p. 99-100) the author complicates things unnecessarily, but on the whole the analyses appear competent and reliable.

LEIF JOHANSEN

University of Oslo

Economic History; Economic Development; National Economies

China's Wartime Finance and Inflation, 1937-1945. By ARTHUR N. YOUNG. Cambridge: Harvard University Press, 1965. Pp. xviii, 421. $10.00.

Market Control and Planning in Communist China. By DWIGHT H. PERKINS. Cambridge: Harvard University Press, 1966. Pp. viii, 291. $7.00.

Communist China's Economic Growth and Foreign Trade. By ALEXANDER ECKSTEIN. New York: McGraw-Hill Book Company, 1966. Pp. xvii, 366. $8.50.

The Currency and Financial System of Mainland China. By TADAO MIYASHITA. Tokyo: The Institute of Asian Economic Affairs, 1966. Pp. viii, 278. $7.50.

From 1911, when the Manchu dynasty fell, to 1927, when Chiang Kai-shek's Nationalist government came to power, China had no strong central government to mend its political disunity. And even Chiang's regime, beset as

it was by the Japanese invasion, frequent battles with local warlords, and continuing struggles against the Chinese communists, was far from a completely unifying force. Nevertheless, despite the turmoil, there *was* industrial growth in China from 1911 on, but it was located mostly in the treaty ports and in Manchuria (which was developed largely by the Japanese after their occupation in 1931). Industrial growth was confined to small areas and hardly touched the masses of Chinese; it was a thin foreign ring around the edges of one of the world's poorest and most miserable countries. For China proper there was little or no economic development in the 1930's and 1940's, but there was an abundance of inflation.

Arthur N. Young, who was financial adviser to the Nationalist government from 1929 to 1947, has written an "insider's" account of his and others' futile attempts to halt China's inflation, which commenced shortly after the start of all-out war with Japan in 1937 and lasted through the final curtain in 1949.

The inflation got under way "slowly" in 1938 when prices rose by about 3 per cent each month. The average monthly rate climbed to 5 per cent in the following year and then to 7 per cent in 1940. From mid-1941 to late 1944, prices rose by 10 per cent per month; and by 20 per cent from then to the war's end in 1945. Although Young carries the story only to this point, he does note that, from August 1945 to late summer 1948, prices rose 2500 times, equal to their climb during the previous eight years. And, in the final months before the total collapse and the flight of Chiang to Formosa, the price index also took off and disappeared into the great beyond.

The immediate causes of the inflation, according to Young, were the vast increases in government expenditures and the grossly inadequate tax system. While government spending in current prices about doubled each year from 1937 to 1941, then tripled annually to 1944, and finally jumped fivefold in 1945, in real terms these expenditures actually declined by 30-40 per cent over the period. The tax system, however, simply could not respond adequately to large increases in nominal incomes and spendings. In current prices, budget receipts lagged farther and farther behind expenditures; and, in real terms, they fell by perhaps 75 per cent from 1937 to 1945. The weakness of the revenue system was due not only to the antiquated tax structure itself and to disorganization caused by fighting, but it also reflected the conflict of class interests and the corruption within the Kuomintang's own ranks. Although Young is sympathetic to the Chiang regime, many passages in his book are devoted to the abuses, bribery, thefts, and extravagances of some of its officials and leaders.

Young points out that the ever-expanding budget deficits were almost entirely financed by the creation of money—mainly by new issues of currency. Consequently, nominal money rose sharply after 1937, but it did not keep up with the steep ascent of prices: real money balances fell by more than 80 per cent during the period, reflecting a massive flight from money.

In the face of these powerful inflationary forces, the efforts of Young and others to slow down the price rises were woefully ineffective. The government tried bond sales to the public and later gold sales, price controls, foreign

exchange stabilization, control of interest rates; it received lend-lease, gifts, guarantees, currency backing—all to almost no avail. Young's book, unfortunately, deals mostly with these futilities and very little with the basic difficulties. The first part of the book sets out the facts regarding budget expenditures and receipts. Part two contains a discussion of the extent to which budget deficits were covered by borrowing from the public, borrowing abroad, and foreign aid. The third and longest part takes up the government's attempts to check the inflation by various monetary policies, exchange stabilization schemes, and price controls; and this part ends with an analysis of the causes and effects of the inflation.

For much of his economic analysis, Young draws on two earlier studies of the inflation,[1] one of which, in this regard, is superior to his own work, but both of which tell essentially the same story that he does—not surprisingly, because the Chinese inflation was "classic" in virtually every way. Young concludes that the inflation was primarily due to excessive creation of money, that overall scarcity of goods was not an important element in the price spiral (the contrary view was held widely at the time in Washington), that among the heaviest losers were army personnel and civil servants, and that inflation, as usual, led to corruption and inefficiencies.

The book is particularly interesting in its accounts of the author's battles with Washington over policy measures for China. In one of these accounts, he accuses the U.S. Treasury—Harry Dexter White in particular—of "foot-dragging" during the period when the United States was sending gold to the Nationalist government so that it could be sold to the public in exchange for excess money balances. "The deliberate foot-dragging and obstruction of needed [gold] shipments, despite China's urgent pleas, was primarily the work of White and other subordinates . . . " and "White's efforts in 1944-1945, while blocking gold shipments to China, to promote a US $10 billion postwar loan to Russia, show a strong anti-Chinese and pro-Russian bias" (pp. 296-97, 298).

The two books footnoted above continue the story after Young leaves it: to the resumption of inflation, the monetary reform of August 1948, another in July 1949, and the denouement. Young, in glancing at these later years, concludes that the inflation was a major cause of the Communist victory and that those (in Washington) who aggravated the inflation "must bear part of the responsibility for later tragic events" (p. 298). Aside from other dubious implications of this statement, the implied efficacy of gold sales in China as an anti-inflationary device seems grossly exaggerated.

The hyperinflation was finally halted by the Communists by mid-1950, which cleared the road for economic reconstruction and development. As it turned out, Communist China had one of the fastest-growing economies in the world during the 1950's. Her annual rate of output growth was probably around 9 per cent, though this figure was influenced by the depressed levels of economic activity at the beginning of the decade and by the frenzied output drives of the Great Leap Forward at the end. A succession of disastrous har-

[1] Kia-ngau Chang, *The Inflationary Spiral: The Experience in China, 1939-1950*, New York 1958; Shun-hsin Chou, *The Chinese Inflation, 1937-1949*, New York 1963.

vests starting in 1959, disincentives growing out of life in the communes, inefficiencies from the Great Leap, and the pull-out of almost all Soviet technicians in mid-1960, at first slowed output growth and finally tilted the economy downward. Output may or may not have fallen a bit in 1960, but it almost certainly declined, perhaps substantially, in the following year. Since 1962 or 1963, there has been a resumption of growth but at a rate below that of the 1950's.

The spectacular gains of the 1950's were based in large part on an ambitious investment program, especially in the industrial sector. Gross domestic investment averaged more than 25 per cent of gross output, and it was probably at least 30 per cent if investment in human capital is included. At least half of this figure was capital formation in heavy and light industry. For one of the world's poorest countries, in terms of income per capita, this was a most remarkable achievement.

The relatively large investment expenditures in the industrial and transportation sectors, which grew especially fast in the early years of the decade, created strong inflationary pressures for consumer goods and services, and particularly for food products. Since very little investment went into agriculture, the State attempted to increase agricultural output by social and political reorganizations that were intended to raise incentives and efficiency. Land reforms, mutual-aid teams, elementary and advanced agricultural cooperatives, and communes were the principal innovations along these lines. They were of some success, but in the end they were pushed too far and too fast. Since 1961, the State has played down reorganizational features and has stepped up agricultural investment expenditures.

These were the attempted solutions on the supply side. On the demand side, the heavy investment program required restraint of demand for consumption, or, what is the same thing, sufficient saving to match the investment. The Communists, having just witnessed the devastation in the wake of the hyperinflation on which they rode to power, were determined not to gain the required saving through an inflationary process. Instead, they greatly increased tax receipts and held down government consumption, thereby generating saving in the government sector; they used direct controls and manipulated relative prices to achieve large profits in state enterprises, most of which reverted to the budget and so swelled government saving; they encouraged individual saving by creating attractive financial assets with purchasing-power clauses, lottery features, etc., and by establishing thousands of new banking offices around the country; and, finally, for a time they were able to tap some foreign saving for their investment program. Although the supply of consumer goods did not expand vigorously after 1953, demand was restrained sufficiently to prevent any significant increase in prices.

The study by Dwight Perkins, *Market Control and Planning in Communist China,* covers much of this story, but the material is so poorly organized that the reader is continually struggling to put the various pieces together. It is hard to tell what the basic trouble is, but it may be that the book is organized around sectors and markets—agriculture, industry, labor, consumer goods—

which leads to just as many partial views of the economy, preventing the reader from ever seeing the whole picture.

Perkins apparently pursues two problems. The first is a stabilization-growth problem, as expressed in my paragraphs above: given the huge investment program, how could the required saving with fairly stable prices be obtained? The second concerns resource allocation: what role did "the market" play in allocating resources and goods? While the analysis of the second problem at times is served fairly well by the sector-by-sector approach, the discussion of the first, to which I now turn, decidedly is not.

Perkins shows that the Communists successfully rejected inflation (and forced saving) as a method of financing their investment expenditures. The official retail price indexes record an annual increase of only one per cent from 1951 to 1958, and there are no indications of a prolonged serious inflation after that. These indexes, though, probably have a downward bias, as Perkins explains, since they are based largely on controlled prices which were at times below equilibrium levels. Black-market prices apparently exceeded official prices in 1953, 1956, and 1960-62. However, as Perkins states: "None of these qualifications . . . negates the basic contention that price increases in Communist China have in fact been quite modest" (p. 158). The potential excess demand was largely eliminated by other means.

Perkins describes well the State's attempts to raise agricultural output. Land reform, while primarily a redistribution measure and a means of destroying the landlords, was meant to increase incentives toward land improvement. The regime also used advance-purchase contracts containing state-guaranteed purchase prices, eliminating some of the risks to individual farmers. Later, in November 1953, the State began to use compulsory quotas for major crops, principally for the purpose of controlling the marketed portions of the crops. In 1955-56, peasants were organized in agricultural producers' cooperatives, in which income was distributed on the basis of the amounts of labor and land contributed; later, in the advanced cooperatives, only the labor contribution counted. In the summer of 1958, communes were established, which expanded the basic unit from around a few hundred families to several thousand. The communes were subsequently reorganized: the center of control was located in lower-level units and peasants were encouraged to devote some of their time to small private plots. Finally, the government has in recent years stepped up its investment spending in agriculture.[2]

The author feels that land reforms, while raising peasant incentives, removed many rich peasants "who had more capital and better knowledge of advanced techniques of farming than poor peasants"; that centralization in the form of cooperatives and communes was mostly a failure. He concludes that "there appears to be little that centralization accomplishes that cannot be done better through extension services, taxes, and market and price controls in the context of a free peasant economy" (p. 97).

The lack of success on the supply side made it that much more urgent for

[2] A stimulating analysis of these events is found in Franz Schurmann, *Ideology and Organization in Communist China*, Berkeley 1966, ch. 7.

the government to restrain demand for consumer goods and services. The required saving came in large part from tax receipts and profits of state enterprises, very little from foreign saving. Perkins believes that individual saving did not contribute much, that the drives to elicit such saving were "makeshift measures," that they had a "negative effect on incentives [which outweighed] their usefulness in controlling inflation or in accomplishing other objectives" (p. 176). In my judgment, some of this is correct and some is wrong, but in no instance does the author offer adequate support for his contention.

For example, while government saving through the State budget grew remarkably from 1950 to 1960, owing to rapid growth of budget revenues relative to government consumption and transfer payments, Perkins shows only the budget revenue figures. Further, they are not related to anything at all—to investment, GNP, etc. In fact, there are no GNP figures in the entire book, and yet good estimates exist; worse yet, despite the fact that the investment program is so vital a part of Perkins' story, nowhere does he present aggregate investment data—and they, too, exist.[3] Consequently, it is not possible for the reader to judge how important budget receipts were in financing the total investment program.

In fact, budget revenues rose from around 10 per cent of gross product in 1950 to over 30 per cent in 1958 and even higher in the following year; government saving probably financed about half of aggregate investment in the early years and more than three-quarters later on. Perkins' description of the tax system that largely produced this impressive record is fragmented, but it is clearly shown that the most rapidly growing revenue source was from the profits of state enterprises. Large revenues in the earlier years, according to the author, were also provided by taxes on the net income and gross receipts of business firms and by commodity taxes. All of these were replaced in 1958 by the turnover tax. The agricultural tax, which was imposed on the gross yield of output and the base of which was "normal" rather than actual yield, declined steadily throughout the period relative to total revenues. In 1950 agricultural tax revenues were about one-third of budget revenues but only 5 per cent in 1960.

With regard to individual saving, Perkins' views are unsupported. He emphasizes the unimportance of this saving, and yet his figures show fairly sizable increases in deposits and bond holdings, although, once again, they are not related to anything. Apparently because he feels (correctly) that coercion was used in the savings drives, he is quick to ascribe large disincentive effects (work?) to the resulting saving. He offers no evidence for this except to say that such saving would be akin to a tax—an observation that raises more questions than it answers.

In fact, there must have been very large increases in the real demand for financial assets during the early years of development—not only for the sav-

[3] See Alexander Eckstein, *The National Income of Communist China*, New York 1961; William Hollister, *China's Gross National Product and Social Accounts, 1950-1957*, Glencoe, Illinois 1958; T. C. Liu and K. C. Yeh, *The Economy of the Chinese Mainland*, Princeton 1965; and C. M. Li, *Economic Development of Communist China*, Berkeley 1959.

ings deposits and bonds discussed by Perkins but also for money balances and other financial assets. For one thing, the cessation of hyperinflation would in itself renew real demands for financial assets; and the rapid increase in national income would add to them. Further, several key financial assets were provided with purchasing-power clauses or lottery features; mobile banks were sent to rural areas; new types of savings deposits were introduced; new financial institutions were established; saving through insurance contracts was vigorously pushed. Moreover, the land reforms, by decentralizing ownership and decision-making, probably raised both the demand for and the supply of loanable funds in the rural areas, as many poor and middle peasants, owning land for the first time, had to finance expenditures, and many acquired saving for the first time from sales of grain to the State. In addition, demand for money was probably raised by the increasing efficiency of the payments mechanism: the early consolidations of currency issues, establishment of clearing houses, the reduction of remittance charges. Finally, there were forced saving schemes and patriotic savings drives.

These factors, combining their forces in the early years of the period, must have raised real demands for financial assets far above the growth of income. But it is likely that later on, as the economy was socialized—private businesses eliminated, cooperatives and communes established—the real demand for financial assets lagged behind the growth of national income. This is suggested by some preliminary estimates I have made, which show non-State net domestic saving growing very rapidly from negative amounts in 1950-51 to about 4-5 per cent of NDP in 1953-55, and after that declining. These estimates would mean that such saving financed a negative part of total net investment in the initial years, 15 to 20 per cent of it by 1953-55, and probably a declining portion thereafter.

Perkins' second theme, as I have said, is on the role of the market in resource allocation. He shows that market forces were much more significant in agriculture than in industrial production and construction; in the former, the authorities used varying purchase prices in the early years to influence outputs of certain crops, and, even with the advent of cooperatives, "a free market was still the major determinant of a wide range of subsidiary products" (p. 199); in the latter, the major allocating device was the physical plan targets. Perkins finds that the market played a leading role in the distribution of consumers' goods, but that there was a high degree of physical control over the labor market. He concludes that the market has played "an important role in directing the Chinese economy, but a role subordinate to that of centrally determined physical controls" (p. 200), an assessment that seems obvious and correct.

A thesis which is expressed several times in the book is that central planning may be easier in an underdeveloped economy like China's than in a more developed economy, because of the smaller number of commodities produced, the simpler interdependencies in the production and allocation of these commodities, a relatively simple labor market, and China's ability to borrow technology from highly developed nations (which obviates the need to stimulate technical advance through market forces). However, he also sees impor-

tant drawbacks to centralized controls in China, among them the lack of ade quate data, insufficient numbers of trained people, the sheer size of the coun try, and the large number and diversity of farms. While the pros and cons ar mostly left hanging in the air, Perkins seems to feel that the net advantage lie with the market. Still, as he points out, these economic considerations wer unimportant in China's decision to impose central planning; much mor significant, of course, were ideological and political factors.

The material is poorly organized for a book, the discussion of banking i very bad, some of the tables present nothing but puzzles and mysteries, an the most important data are missing. It is to Perkins' credit, however, tha despite these flaws, he has so many interesting things to say that his book : well worth reading.

Alexander Eckstein's study on *Communist China's Economic Growth an Foreign Trade* examines the extent to which China was able to further her in vestment program (and so her economic development) through internationa trade, loans, and aid. While he does not examine the role of government an individual saving, he concludes that China's growth would have been marked ly reduced without foreign help. This conclusion, however, does not rest on th magnitude of China's foreign trade (on trade benefiting both sides), for he imports were probably no more than 4-5 per cent of her gross domestic prod uct, but rather on other considerations.

It is generally felt that foreign trade may allow a country to increase its do mestic investment in two principal ways. First, if foreign saving is available the resulting import surplus releases real resources, at a given consumptio level, for investment purposes. Second, even if foreign saving is not tappe (i.e., imports equal exports), a country's domestic investment might still b enlarged if it has net imports of investment goods. Suppose a country import investment goods and exports an equivalent amount of consumption goods Then its domestic investment is higher to the extent that it could not hav produced the investment goods at home with the real resources that went int the exported consumption goods.

Eckstein's conclusion, therefore, rests on an examination of these two mat ters; and I shall consider each in turn. To what extent did China have acces to foreign saving? As Eckstein shows, while the answer to this question is un certain, it is probable that China had on balance little or no import surplu during the period 1952-63—and this would also be true for the longer perio of 1950-65. The answer is uncertain because China's imports and exports ca be built up best from the trade data of the countries trading with her; and thus, the data are in terms of foreign currencies. This means that the yua amounts of imports and exports can be obtained only by converting the dollar ruble, etc. figures into yuan at an appropriate exchange rate. Ideally, on would want to value China's exports at the yuan prices that reflect the wort of real resources going into producing them; and to value her imports in yua prices that reflect the worth of real resources that would have been used t produce them domestically.

It is generally agreed that in China prices of investment goods were "high while prices of consumption goods were "low." China's exports, then, consis

ng mostly of consumption goods, would be overvalued in yuan if the official exchange rate were used to convert dollars to yuan. That is, if China exported one dollar of consumption goods at world prices, the official rate would yield a yuan value in excess of the actual value in China's economy. Similarly, imports would be undervalued because they consisted largely of investment goods (up to 1960), the prices of which were relatively high in China. Consequently, if a rate of 2 yuan = 1 dollar is used for exports, a rate of, say, 3 yuan = 1 dollar should be used for imports.

Inasmuch as no-one knows the "correct" rates, Eckstein begins by using the uniform (official) rate of 2.62 yuan = 1 dollar for both imports and exports.[4] His calculations reveal an *export* surplus of 4.1 billion yuan for 1952-63, which, if correct, means that on balance China was an exporter of capital. However, the export surplus is small, amounting to less than ½ of 1 per cent of cumulated GNP for these years.

Eckstein shows that China had import surpluses up to 1955 and export surpluses thereafter. This was her trade pattern over time with other communist countries, in particular with the USSR; the opposite pattern, however, prevailed with noncommunist countries. Her early overall import surpluses were financed by credits (loans, not grants!) from the USSR, and her later export surpluses were used to repay the USSR loans and to extend aid to other countries. Up to the present, China has probably extended more foreign aid than she has received (but not all of her aid has been drawn upon); moreover, her terms have been more generous than those extended to her—she has made outright grants (and probably received none), has given longer maturities, and some of her loans have been interest free.

Thus, according to these data, China did not tap foreign saving over the period as a whole; instead, on balance, she may have exported capital to other developing nations. She has financed her investment program from her own saving, and has used a (small) part of this saving to aid other countries.[5]

However, some studies have suggested that exchange rates other than the official one should be used in computing China's exports and imports in yuan. As Eckstein explains, one study suggests a rate for the export of consumer goods of 1.79 yuan = 1 dollar; another points to a rate for the import of capital goods of 5.91 yuan = 1 dollar. While Eckstein has serious reservations about these rates, he uses them and obtains a substantial import surplus for China during 1952-63, which is equal to 5-6 per cent of her cumulated gross domestic product. In my judgment—and, I believe, in Eckstein's, too—this is clearly too high. For one thing, imports were by no means all capital goods, especially after 1960 when one-third of imports was foodstuffs; and not all exports were consumer goods. More important, such a high import surplus figure is grossly inconsistent with "companion" data on domestic investment, government saving, and what we know about private (non-State) saving. China may

[4] This has been the official rate since mid-1957, when it was indicated that the previous rate of 2.36 = 1 overvalued the yuan.

[5] It should be noted that, although Eckstein does not say so, his data include only commodity trade and not services. However, a recent unpublished study by F. H. Mah, which includes both, shows about the same picture.

have had an import surplus during the period, but it is unlikely that it was very large. And so it is unlikely that China's development was financed to any significant extent by foreign saving for the period as a whole.

Turning now to the second question, to what extent did China further her economic development by importing capital goods? Again, one cannot be certain about the answer, but it is fairly clear that China imported capital equipment, chemicals, and metals in heavy volume up to 1960, including complete plants from the USSR with technicians and advisers; but then mostly food products from 1961 on. She exported some investment goods—to North Korea, North Vietnam, and other countries—but mainly food products and textiles, especially the latter after 1960. Data in Eckstein's study and elsewhere suggest that China's net imports of investment goods amounted to 10-15 billion yuan for the years 1952-63, or to about 3-5 per cent of her total domestic investment.

Eckstein, going after the problem differently (because the above estimates come from some rather shaky figures), computes the extent to which net fixed-capital investment would have fallen if China had not imported machinery and equipment (a component of fixed-capital investment). This estimated decline, for the period 1952-56, is about 10 billion yuan—that is, from approximately 43 billion yuan to 33 billion yuan. The higher (or actual) figure was associated with a 27 billion yuan increase in net national product; so the lower figure, it is assumed, would have yielded an increase in net output of 20 billion yuan—which is 7 billion below the actual increment. For the period 1953-57, this signifies a reduction in the annual growth rate from 6.3 per cent to something less than 5 per cent.

It should be noted that these calculations apply to the 1953-57 period, when net imports of machinery and equipment were at their peak. Moreover, Eckstein assumes that the ratio of equipment investment to total fixed-capital investment is constant (40 per cent); hence, a decline in the numerator, owing to a decrease in imports, would reduce the denominator by a multiple of the decline in equipment investment. If one assumed, instead, that the other components of fixed-capital investment would not be affected by the reduction in equipment imports, the importance of the imports would of course be much less. Also, Eckstein assumes that, if equipment imports decline, capital goods could not be domestically produced with the real resources released from the counterpart exports. To the extent that such substitution could occur, the output growth rate would be less affected by a decline in imports. On the other hand, the actual yuan value of machinery and equipment imports may have been higher than the level suggested by the exchange rate of 2.62 = 1. If so, the elimination of those larger imports, everything else the same, would have a larger impact on the output growth rate.

So the matter is pretty much up in the air. But there is little doubt that, at least up to 1957, China's growth rate was boosted by her import surpluses and by the large capital-equipment component of these imports. Thereafter, and especially after 1960, her growth rate probably suffered, as she ran export surpluses to repay debts and to extend aid to other countries, and as her own capital-goods exports probably surpassed her import of such goods. So it is

quite likely that the higher growth rates in the earlier years and the lower ones in the later years were significantly influenced by foreign trade and aid factors.

Eckstein has more in his book than that. His beginning chapters on China's economic development and the pre-conditions for it are highly informative and analytically pleasing. And, at the other end of the book, there is much worthwhile detail regarding China's trade with individual countries, both communist and noncommunist. Eckstein's study is an important one.

Since Perkins has some material on government saving—the tax structure and the system of direct controls as a device for raising profits of state enterprises—and since Eckstein takes up foreign saving and trade, it would have been nice if Tadao Miyashita had completed the job by analyzing the role of the financial system in eliciting individual saving. But, while his book on *The Currency and Financial System of Mainland China* provides some information on this question—though really little more than Perkins gives us—its strengths lie elsewhere: in descriptions of the Communist financial structure and of the problems it has faced in the past two decades.

Miyashita gives a rather complete picture of the financial system in China, including street addresses for some of the financial institutions and photographs of the new currency issues and of the head office of the People's Bank of China—the central bank. When Arthur Young was financial adviser to the Kuomintang, there were four main government banks, a few dozen, highly influential foreign banks, some modern Chinese banks, and several hundred so-called native banks. One of the government banks was the Central Bank of China (organized in 1928), but it never did attain the status suggested by its name; there was no strong monetary control during this period. Chinese banking was heavily concentrated in Shanghai, and only rudimentary credit and saving facilities existed in rural areas where most of the people lived.

When the Communists came to power, the Central Bank of China was replaced by the People's Bank, which issued all of the currency and conducted ordinary banking business, making most of the short-term loans and having large deposit liabilities to nonfinancial units. Of the other three government banks under the Kuomintang, the Bank of China became a joint state-private institution specializing in foreign exchange and overseas Chinese remittances; the Bank of Communications attained the same status, specializing in loans for capital construction until October 1954, when this function was taken over by the newly established People's Construction Bank; and the functions of the Farmers Bank of China were assumed by rural credit cooperatives, the People's Bank itself, and the Agricultural Bank of China (reorganized in November 1963). Several other new financial institutions have been created, monetary controls highly centralized in the People's Bank, and credit and saving facilities greatly extended.

Miyashita presents an excellent, detailed account of the development of financial control. The distinctive feature of the system is the large number of special funds supplied to economic units, each of which can be used for only one purpose and no other. Budget funds differ from credit funds in that the former are grants to be used for investment and the latter are loans for oper-

ating expenses. The budget funds themselves are divided into subfunds according to use—water conservancy works funds, forestry funds, etc. Some funds paid out of the budget ("fixed sum liquid funds"), however, are for normal operating expenses, while "excess fixed sum liquid funds" are credit funds for exceptional, temporary current expenses. Since July 1961, 20 per cent of the fixed sum liquid funds have come from the People's Bank (by way of the budget) as short-term loans. There are also commodity funds and noncommodity funds and many other categories. In addition, the authorities have established currency control in which most payments are made by check through the People's Bank and currency holdings are tightly limited, any excess having to be deposited almost immediately.

The author devotes two chapters to the currency policies of the Communists in the areas they controlled before 1949; he discusses the problems connected with the State's unification of the dozens of currencies outstanding when the Communists came to power, the stopping of hyperinflation, the socialist transformation of private banking in Shanghai, and the development of the rural credit cooperatives.

All in all, the book is full of information but devoid of critical evaluation; except for a few miscellaneous numbers, the author is not interested in statistical material. He *is* interested, though, in treating his subject accurately—even going so far in this direction (as do Perkins and Eckstein) as to call the present capital by its correct name, Peking, instead of Peiping.

These books leave little doubt that the economy of the People's Republic of China (correct name, at last!) has been made over from top to bottom and back again. There have been bold experiments (some failures), good and bad luck, and a lot of turmoil, but beneath it all a substantial improvement in the quantity and quality of the country's human and nonhuman resources. While these studies understandably concentrate on the machines and buildings, China's dramatically successful efforts to raise the health, education, and welfare standards of her people would also seem to be worthy of attention. It apparently is true that our profession's "China experts," for some reason, have shied away from this topic.

John G. Gurley

Stanford University and Center for Advanced Study in the Behavioral Sciences

Development and Economic Policy in the UAR (Egypt). By Bent Hansen and G. A. Marzouk. Amsterdam: North-Holland Publishing Company, 1965. Pp. xv, 332. $9.80; 70s.

The purpose of this important book is to provide a strictly economic analysis of the profound socio-economic changes experienced by Egypt since the end of World War II. It is not intended to be "an encyclopedia on the Egyptian economy," and many interesting and "important special questions are superficially dealt with," indeed, some are "not even mentioned." The book is a series of analytical surveys "by economists for economists" of the "main economic trends, problems and policies" in postwar Egypt. Economic philosophies of the "Hegel-Marx-Rostow" type are eschewed and the stress is placed

on economic analysis based on "just ordinary modern economic theory" (p. xiii).

If "ordinary theory" means simply "non-mathematical," we agree with Hansen and Marzouk that there is, for instance, very little connection between Hansen's contribution to this book on development and policy and his highly analytical *Lectures in Economic Theory* (Cairo, 1964), given at the Egyptian Institute of National Planning presumably during the time this book was being written. Indeed, on some key subjects (e.g., pricing and efficiency, and inflation, the budget deficit, and the exchange rate) the distance between theory and its application is disconcerting to an economist, be he teacher or advisor. Yet, having read this book, one can agree with the authors' statement that despite its shortcomings ordinary modern economic theory does bring us a "long way towards a full understanding of the economic problems of developing countries" (p. xiv).

Essentially, Hansen and Marzouk have written a series of survey essays on various aspects and sectors of the Egyptian economy. After an initial bird's-eye view of the economy during the past fifty years, cast primarily in terms of national income and product aggregates,[1] each survey essay (one to two chapters in length), focuses on a major segment of the economy. They start, appropriately enough in the case of Egypt, with the population problem. They admit that Egypt is densely populated, its population highly concentrated along the Nile, its agricultural frontier long since closed (the completion of the High Dam at Aswan will not significantly alter this situation), and, the recent findings of Professor Abu-Lughod of Smith College[2] notwithstanding, that Egypt not only is overurbanized, but that it is increasingly becoming more so—the degree of urbanization, they say, has increased by a fourth since World War II. But having said this they conclude that most of the pessimism concerning Egypt's high rate of population growth (2.9 per cent since 1947) is unwarranted since the data of the 1950's (on which the rate is based) are misleading due to factors peculiar to that decade. For instance, they maintain that the rise in the fertility rate was due mainly to "speeded-up births," and therefore is temporary. They also note that there was a temporary increase in marriages at the very end of 1959 so that population projections based on the data of the 'fifties give too high a future growth rate. They thus feel that the optimism of the "transition population theory," wherein industrialization, urbanization, and improved education will ameliorate the population problem, best describes the eventual result of the income-population race in Egypt. Later they state that this process is already occurring, what with the average productivity of both land and labor increasing in agriculture. With real in-

[1] In a Statistical Appendix (pp. 317-25), which enchances the value of the book, are the results of the various Institute of National Planning *Memoranda* written by Hansen (and D. Mead) on the national income, outlay and savings of the UAR during the period 1939-1962/63.

[2] "Urbanization in Egypt: Present State and Future Prospects," *Econ. Develop. and Cult. Change*, Apr. 1965, *13* (3), 313-43. Her main point is that, in the future, "*Overconcentration* of the urban population in relatively few cities, rather than "over-urbanization" *per se*, appears more serious and likely to emerge as the key bottleneck in any program of industrialization."

comes of agricultural labor increasing by 15-20 per cent since 1950 they feel Egypt is far from "the Malthusian nightmares usually associated with the agricutlural situation in underdeveloped countries."

There follow two essays on agriculture and industry. A chapter analyzing the structure of each sector is followed by one evaluating the government's policies in these areas. There is a wealth of information in these four chapters: we choose only the following for detailed comment. We are told that while there is considerable seasonal unemployment in agriculture "about 25 per cent of the total permanent labor force, calculated on an annual basis," nevertheless the oft-repeated assertion that much of Egypt's agricultural labor force is suffering from disguised unemployment with a marginal productivity of near zero is incorrect (pp. 61, 64, 161, 298). What is worse, much of the investment planning in Egypt has been based on this mistaken "standard assumption of surplus labor in agriculture." Although the Five Year Plan puts the number of required agricultural workers at 3.2 million while actual employment in 1960 was 4.2 million, Hansen and Marzouk say this does not necessarily imply disguised unemployment with near zero marginal productivity in agriculture. If anything, this indicates only the extent of seasonal unemployment[3] and so does not justify a zero shadow price of labor which they maintain has been used by Egypt's planners.

According to their estimates the value of the marginal product (VMP) for agricultural labor is positive and roughly equal to the wage rate. As Hansen has more recently stated,[4] "The Egyptian fellah is an economical and calculating peasant" who would not employ labor at a positive wage if the resultant increase in production was "near zero." Using data of the 1950's they demonstrate this equality between wages and the VMP. At that time the annual wage (for 300 days) approximated £E35. Output per laborer averaged between £E90 and £E110. Since the labor coefficient of the Cobb-Douglas production function for this period was 0.3 the VMP of labor "should be about one-third the average output," or some £E33. Thus, for the 1950's the VMP did approximate the annual wage. They add "the equality of wages and marginal value productivity holds true back to 1937, at least." So disguised unemployment is banished, as is Lewis' contention that industrial expansion can occur "with little or no increase at all in money wages," although Table 5-18 indicates money wages have been constant since 1956!

In any event disguised unemployment as a widespread phenomenon in Egypt reappears when the authors state "it is in the Government sector that disguised unemployment in Egypt is to be found" so that "if zero-productivity of actually employed labour is . . . anywhere in Egypt, it is most probably in Government administration." They also stress that, unlike the disguised unemployment in industry, which is but a temporary phenomenon of the government's "Great Employment Drive" of 1961—when the productivity of in-

[3] Their analysis makes use of the 1950 Agricultural Census since the data for 1960 "has not yet been published" (p. 63).

[4] B. Hansen and M. El Tomy, "The Seasonal Employment Profile in Egyptian Agriculture," *Jour. Develop. Stud.*, July 1965, *1* (4), 404.

dustrial labor fell, despite a big increase in industrial investment—that "over-employment" in Government will persist "because the Government has committed itself to employ *all* university graduates." But according to a theorem of Professor Georgescu-Roegen an oversized bureaucracy is a normal economic phenomenon *only* in overpopulated economies. So we must continue to classify Egypt as an overpopulated underdeveloped country with a severe employment problem, despite government edicts designed to eliminate unemployment and a high growth rate (8 per cent since 1955) in its industrial sector. And maybe the planners were right, since even the authors would agree that Egyptian industry could siphon off large numbers from the pool of surplus government employees without any loss to national output.

The entire complex problem of the interrelationship between the foreign-trade deficit, inflation, the savings-investment process and planning via direct controls or through a heavy reliance on the price mechanism is brilliantly analyzed in the remainder of the book (Chs. 7-11). As long as Egypt continued to be a planned market economy, the authors favored direct controls as against the price mechanism since the conflict between equity and efficiency tended to create "a shortage economy." But as the extent of government ownership of industry widened, and all foreign-trade activity was placed under strict government control, "this dilemma simply disappeared" when Egypt, after July 1961, became "an Arab Socialist state." Writing in late 1963 they attributed whatever difficulties Egypt experienced from its program of deliberate industrialization mainly to "inadequate price policies and to an incompetent and bureaucratic regulation of foreign trade." But by November 1964, in their preface, the authors concluded that the major problem had become a "general domestic excess of purchasing power" owing mainly to a governmental deficit approaching 12 per cent of GNP. Unfortunately, economic analysis is not sufficient to evaluate the consequences of this policy, since the budget deficit is a "politically given factor," and so the resultant balance of payments deficit and the entire question of an "equilibrium exchange rate" become "essentially political questions." Notwithstanding this disclaimer these chapters contain some of the finest economic analysis in the entire book and illustrate "ordinary modern economic theory" at its best.

Not too long ago it could rightly be said that Egypt suffered less from a scarcity of information than from a surfeit of bad statistics. Hansen and Marzouk have gone a long way in giving the student of the Egyptian economy the analysis of the data which was lacking in previous works. But at the very outset they state that what still is needed for a deeper understanding of the economic problems of countries like Egypt is "above all detailed, reliable empirical information—statistical data and descriptions of institutions." Yet repeatedly they note that the data they need—on prices, employment, production, financial reports, government spending, etc.—are not available. They do not comment on why the more recent data are unavailable, leaving the reader to assume that an inevitable publication lag exists.[5] But writing in 1965, in the article already cited, Hansen has justifiably complained: "the figures from the 1960 agricultural census are actually available in the Ministry of Agriculture,

[5] See footnote 3 above.

but it has not been possible for us to make the Ministry release these figures (or even to let us have access to them). This lack of cooperation has forced us to work with the old data." Small reward for such noble efforts.

All in all this book gives a highly competent analysis of recent economic development in the Egyptian economy. Yet it certainly does not replace Issawi's *Egypt in Revolution*. Indeed these two books complement each other quite well, Issawi's being strong on historical description, institutional, and organizational detail, but weak and spotty, and generally deficient, in its application of recent analytical advances. Almost the opposite can be said of the book under review. A general economist who wanted to read but one book on present-day Egypt probably would benefit more from Issawi, but the economist seriously interested in Egyptian economic development will just have to read both and to consult them conjunctly.

WILLIAM O. THWEATT

Vanderbilt University

Development Planning: Lessons of Experience. By ALBERT WATERSTON, WITH C. J. MARTIN, A. T. SCHUMACHER, AND F. A. STEUBER. Baltimore: The Johns Hopkins Press, 1965. Pp. xix, 706. $10.75.

Albert Waterston's book, based on seven years' study at the World Bank of some fifty country experiences, is a major contribution. Every economist concerned with development planning, whether as a teacher in a university or as a practitioner in development activities, should at least plunge into it. It is a voluminous book, as epitomized by a 58-page appendix listing all of the national development plans for all countries of which the author and his assistants were aware. It reaches some significant conclusions, supported by a wealth of examples; the reader may not find them to be uniformly persuasive, but they certainly demand serious consideration. And it is full of distinctions, observations, proposals, cautions, criticisms, and above all descriptions of individual country experiences, which are sure to be thought-provoking.

There are three main themes. The first is that most low-income countries today are not yet ready to carry out effectively comprehensive development planning. Administrative systems cannot handle both rationalized public investment and guidance for private investment, so they do neither well. Relationships in the econometric models used are unreliable, particularly the capital-output ratio and its implication that raising investment is the way to more rapid growth. Emphasis on aggregative analysis distracts planners from preparing projects and fitting them together, so progress is hobbled by a shortage of sound projects. As a consequence, premature comprehensive plans typically fail or become irrelevant.

On the other hand, a project-by-project approach is likely to lead to misallocations because inconsistent criteria for approving projects are used, and to disruptions or inflation because overall resource constraints have not been checked. Waterston's conclusion is that most underdeveloped countries today will accomplish more if they pursue an intermediate approach—integrated public investment planning. The principal sector priorities for public investment are usually apparent, the investment funds available from budgetary

saving and foreign capital inflows can be readily estimated, and an invent of possible projects can be quickly adjusted to the sector priorities and available funds using simple administrative and economic criteria. Thoı comprehensive planning is conceptually preferable, a more practical appro today is to get an integrated program of sound public projects moving ahe vigorously.

A corollary is that plan targets should be realistic even if unpalatably m est. They should not be derived from needs, which are limitless, but from re istic estimates of available resources. Indeed, planners should correct inve ment proposals in the light of past administrative effectiveness of governm agencies, and should insist on prior commitments by political leaders on pol measures which the government is prepared to adopt.

This first theme clearly deserves serious consideration by everyone c cerned with how best to accelerate material progress in low-income countr today. I believe that Waterston has persuasively demonstrated that a ma reallocation of planners' efforts is often called for. Relatively less staff ti should be devoted to plan formulation, particularly aggregative models a broad sector allocations, and relatively more should be devoted to the t most notable weaknesses limiting performance under the plans—project pr aration and evaluation, and implementation procedures. On the other ha Waterston's prescription to defer the attempt to make development planni comprehensive, and to settle for modest targets quickly attainable through i proved public investment practices, seems to me quite unsuited to the urger of the development problem. Governments of most low-income countries tod are seeking substantially accelerated growth rates, not just rationalization the use of available public funds, and the task of planners is to discover p terns of adjustment in all parts of the economic system which will contribı as much as is economically and politically feasible to this overriding go From this standpoint, is it so clearly wrong to set targets on the optimis side as a stimulus for additional effort and mobilization of resources? Can t right sector priorities and project selection criteria really be established simply without thorough aggregative analysis? While the allocation planners' efforts should in many cases be shifted in the directions Waterst suggests, it seems to me that the present emphasis in most countries up comprehensive planning, aggregative analysis, and ambitious targets provid the appropriate foundation for such shifts in planners' efforts.

The second recurrent theme is the crucial importance of implementation an integral part of the planning process. Preparations for implementati should be made in the design stage—undertaking sector, feasibility, and eng neering studies on a broad scale to ensure sufficient sound projects; posi sharp policy alternatives to political leaders to elicit clear policy decision identifying specific agency responsibilities, performance targets, and asso ated policies for implementing parts of the plan; defining time schedules a progress measures for approved projects. Implementation during the plan p riod should then be checked by clearly established follow-up procedures— reporting system for projects which emphasizes regularity and promptne even at the cost of limiting the information requested; annual progress eval

ations for cabinet-level consideration; annual plans as the operational links between multiyear objectives and current implementation problems.

Again Waterston's conclusions are persuasively argued. Since his survey implies that most countries are lagging in implementation, there are more negative than positive examples of country experiences in support, but his suggestions of lines to follow in improving implementation seem bound to strike responsive chords among operating planners. One notable gap in his treatment of implementation, however, is that there is no explicit detailed examination of use of policy instruments to guide private development activities. This neglect presumably stems from his emphasis on rationalized public investment planning. But for countries with extensive private activities attempting comprehensive development planning, there are many significant issues to be considered—choosing price adjustments or quantitative controls, preferring instruments with broad or with narrow coverage, determining the mix of consultation procedures and inducement or regulation, accommodating short-run fiscal-monetary adjustments and long-run development stimuli. It would be illuminating to have some lessons of experience on these points too.

The third main theme which runs throughout the book is Waterston's conclusion that the principal obstacles to effective development planning in most low-income countries today are administrative and political, not economic. Accordingly, the last eight of the seventeen chapters deal specifically with administrative and organizational topics. Once again there are many persuasive insights—locating the planning agency in the chief executive's office rather than in the finance ministry or a separate planning ministry, establishing a small cabinet committee for making major planning decisions rather than a large commission with outside members, coordinating all arrangements for foreign technical assistance in a single agency, screening projects by a central working group including representatives of operating ministries, avoiding a separate organization to supervise plan implementation, establishing programming units in operating ministries, and numerous others. I would question whether, given the general administrative constraints which he describes, and his stress on prompt improvement of public investment activities, Waterston has not envisioned too limited a role for the central planning agency itself. Thus he suggests that it should not permit itself to be drawn into preparing projects and sector programs because of difficulties in operating ministries, and should be happy to leave coordination of foreign financial assistance to the ministry of finance, as well as preparation of the annual capital budget. This seems to me an unduly self-denying approach for the agency most actively concerned with obtaining prompt achievements under the plan. But as in the rest of the book, his judgments are bolstered by many examples of country experiences, and must be weighed seriously.

In sum, economists who wish to go beyond theoretical analysis of economic development to its application by today's governments in low-income countries will find reading Waterston's book a most stimulating experience.

Paul G. Clark

Center for Development Economics, Williams College

The Development of Social Overhead Capital in India 1950-60. By J. M. Healey. Oxford: Basil Blackwell, 1965. Pp. ix, 180. 35s.

The author describes the growth in India of certain important types of social overhead capital—transport, power, and irrigation—and discusses their sources of financing, methods of pricing, choices of technology and other related matters of policy interest. His intention is to explain their role in the Indian development effort and to shed light on some basic economic and political problems which confront most developing economies.

The study describes how public investment expanded from an initially modest rate to about 7 per cent of national income, representing by the end of the period about 40 per cent of the entire national investment. At the same time profits (surpluses) of public overhead enterprises (with the notable exception of forestation) remained stagnant or declined. This contributed to the relative (and even absolute) decline in government savings during the period, which in turn led to an increasing need to rely on borrowing. Prices and rates for services by the railways and irrigation and power authorities were not rationalized during the period to provide surpluses. In addition, in the case of irrigation and power projects, rapidly rising deficits resulted from the steep rise of interest charges due to a sudden expansion of capacity after 1951 and from the increase in real investment costs as the exploitation of resources for such purposes was extended to less endowed areas.

The author provides a description of some of the failures of the Indian pricing policies as well as general recommendations about the introduction of more rational methods of pricing. In these, however, he is not always consistent. For instance, he correctly recommends that the cost of subsidizing certain rail services (including passenger travel) should be financed—if subsidization is desired—from government general revenues and not with a discriminatory rate structure. At the same time he endorses discriminatory pricing of irrigation water according to size of land holding in order to offset the lack of an adequate progression in rural taxation.

Because of its impotance to industrialization, transportation policy is given special attention. The Indian railways failed to meet demand in the second half of the decade because of a number of factors identified in the study, such as the underestimation of the increase in the average length of haul, an excessive rate of replacement of wagon capacity, the failure to eliminate bottlenecks on particular routes, etc. The author properly notes that underpricing of railroad services contributed to demand-induced pressures on the already overburdened rail capacity, but does not identify the long run consequences of the discriminatory underpricing of coal transport which exist even in slack periods and which distort the location of industries and the choice of technology. (As a matter of interest, soon after the close of the decade under investigation the pressure on rail capacity vanished because the planned expansion in traffic failed to materialize.) The question of road versus rail remains unresolved and no previously unknown information is presented which could shed light on this rather difficult problem. However, the discussion of the relationship between road improvement and savings in vehicle operation costs should be of interest to planners.

The last part of the study examines employment policies and technology relevant to irrigation, river valley and construction projects. The author claims that there is an "economic case for raising the wage rates of unskilled project workers since this would raise efficiency more than proportionately." Even if one accepted the rather tenuous statistical evidence on the relationship between wages, workers' calorie intake, and capacity to work, one must still question the meaning of the "economic case." An increase in the calorie intake leaves the labor force just as unskilled as it was before; at best it can increase the supply of unskilled labor measured in efficiency units. The result in a labor surplus economy may turn out to be a net reduction in the number of employed. Thus we are confronted with a pure welfare problem unrecognized by the author: the trade-off between the level of consumption of the employed labor and the rate of employment. Furthermore, higher wage rates may motivate substitution of capital for labor, a result the author seems keen to avoid. In fact, he himself is in favor of increasing labor intensity as much as possible and argues that because of low utilization rates for equipment "mechanized methods have only a narrow economic advantage over manual methods." This may or may not be so, but the evidence is inconclusive. Comparisons are given only on the cost side and not on the benefit side. Yet, because the time needed to complete a project with alternative construction methods will vary with the method, the stream of benefits (revenues) will also begin at correspondingly different dates. Hence, the economic advantage of the alternative technologies can be determined only with the help of present value computations. When interest rates are high, relatively short differences in construction time may be decisive.

In summary, the study does contain interesting and worthwhile material about a crucial period of Indian planning. However, it is more successful in its descriptive parts (though the discussion is mostly based on information made available during the period under investigation which in a book published five years after the terminal year of the study period need not be the case) and somewhat less instructive in its analytical efforts. The latter could have been given more cohesion and greater unity if there had been an attempt to provide for the analysis of the price and investment policies a general frame of reference such as a clarification of the relative weights attached to the diverse purposes of the Indian development effort.

LOUIS LEFEBER

Brandeis University

Trade, Finance, and Development in Pakistan. By J. RUSSELL ANDRUS AND AZIZALI F. MOHAMMED. Stanford, Calif.: Stanford University Press, 1966. Pp. xii, 289. $8.75.

The title describes this book's contents with some accuracy. Three chapters (81 pages) are concerned with foreign trade, including detailed discussions of principal export crops, the changing pattern of Pakistan's foreign trade, and the balance of payments since 1947. The bulk of the text (170 pages) deals with "finance," both public and private, including central and commercial banking, special problems of agricultural credit, the capital market, and cen-

tral and provincial government finances. Changes in price levels are discussed in a separate chapter. "Development" is the concern of the entire book, though there is nothing to tie together the various aspects that the authors choose to discuss. There is no thesis to the book, and the closest it comes to a framework is in the last chapter, on "Development Planning," which reviews briefly the efforts and results to 1965, when the book was completed. This chapter, which is the one of most general interest, is weakened by the fact that the data on the economy for the Second Plan period were out of date before the book was published. That is a characteristic of all studies dealing with Pakistan, however, since the statistical base has been undergoing a continuing improvement process.

The major changes in the level and composition of imports and exports are reviewed in two chapters. The crucial nature of Pakistan's failure to devalue with the United Kingdom and India in 1949, and the subsequent effects of the Korean War boom, which kept the full effects of the failure to devalue from being felt until 1952-53, are described very well, particularly with regard to the financial and institutional arrangements. The principal features of the overall changes in trade since Partition were: (i) the decline in "traditional" exports in relative and in absolute importance in export earnings; (ii) the emergence of new exports, particularly manufactures of cotton and jute; (iii) the rapid shift in the composition of imports, notably the decline in cotton textiles and other consumer goods, and the rise of metals, and machinery and equipment, both in absolute and in relative terms; and (iv) the shift from exports of foodgrains at and shortly after Partition to heavy imports of foodgrains under PL 480 during the late 1950's and the early 1960's. Overall, there was a brief period during the Korean War boom when export earnings exceeded imports because of unusually high export earnings, and a period in the early to mid-1950's when payments were close to receipts due to tight import controls and a devaluation in 1955. Finally, there was a widening gap between imports and exports in the late 1950's and early 1960's due to the rising flow of foreign aid of various sorts, which financed about 45 per cent of imports during the Second Plan period. Unfortunately there is little reference to the activities in the productive sectors of the economy and to the movements in composition of national expenditure that accompanied and caused the movements in foreign trade levels and composition. The extremely rapid rate of growth of manufacturing industry, the stagnation of agricultural output up to 1959, the sharp increase in the rate of investment and the surge in agricultural growth after 1959, are not associated adequately with the changing patterns and levels of trade. The only policy variable that is discussed in detail is the exchange rate, though there is some discussion of the import control system.

The legal and institutional features of the growth of banking and the increase in money supply and credit are dealt with extensively. Bank deposits and credit expanded rapidly relative to the rest of the economy through the entire period. Government deficits were financed through the banking system in the mid- and late 1950's, and to some extent again in the mid-1960's. Private trade in export crops and in imports was heavily financed by commercial bank credit, but its share in total advances fell over time, as commerce was

gradually being replaced by the rapidly growing manufacturing sector as a claimant on commercial bank credit. There is some discussion of the policy of the State Bank and its relation to the government and to the commercial banking system, but one of the strange features of the book is its failure even to refer to Richard Porter's *Liquidity and Lending* [4], which deals with monetary policy extensively, and which is also one of the best pieces of empirical research yet published on any aspect of Pakistan's economy.

Credit to agriculture is generally not forthcoming from the commercial banking system, and the arrangements to meet the needs of agriculture are discussed in a separate chapter. The cooperatives and the special banks providing agricultural credit are discussed in great detail, as are the system of government loans, and the traditional lending practices from landlords or village moneylenders. Agricultural credit is and has been in a state of bad disrepair, but there is not, to my knowledge, the general consensus that Andrus and Mohammed allege, that "the cooperative represents the ideal machinery not only for dispensing credit but for meeting the marketing, supply, and other service needs of the millions of small agriculturalists in the country" (p. 157). This is really an article of faith with the authors. There is no empirical evidence that suggests that credit needs would be met best by a cooperative movement. The authors argue that cooperatives to pool land and other resources are necessary to attract institutional credit, that only institutional credit can meet agricultural credit needs, and that, due to a lack of such credit, "a landless rural proletariat has emerged and its ranks are continually swelled" (p. 162). The authors reject credit administered through private distributors of capital goods and current inputs as a possible remedy for the problem. The enormous boom in private irrigation tubewells in West Pakistan recently has suggested that credit may have been less of a constraint than profitable investment opportunities for private agricultural investment and availability of capital goods and purchased inputs at reasonable prices.

The small but growing capital markets in Pakistan are discussed to the extent that data allow. Gross business of insurance companies expanded at rates of over 20 per cent per year since the early 1950's, in response to the expanding urban-industrial-commercial sector. Postal savings institutions have been devices for financing government deficits, and their share in financing assets held by the private sector has been rising, notably at the expense of currency. There is a stock exchange, on which new issues are perennially oversubscribed, but it is too sensitive to manipulation by professionals for it to be indicative of any basic characteristics of the economy.

One of the more interesting chapters concerns the behavior of price levels over the period since Partition. The authors' detailed knowledge of the economy, particularly the trade and financial sectors, enables them to give a good discussion of the response of the economy to the pressures of Partition, the cessation of trade with India, the Korean boom and bust, introduction of import licensing in 1953, devaluation in 1955, the control (and later decontrol) policy of the Martial Law government, and the massive inflow of foreign aid into the economy that accompanied the rise in public and private investment in the Second Plan period. The description of the effects of various changes in

supply and demand conditions in the aggregate and for particular sectors, along with the important differences between East and West Pakistan, will prove interesting reading to anyone familiar in broad outline with Pakistan's economy.

Institutional arrangements in government finance are covered in detail. The problem of federalism, with provinces responsible for major development activities but the central government in possession of the major revenue-raising tax authority, is well treated from an historical-institutional point of view. The continuing importance of indirect taxes in total tax revenue and the changing composition within indirect taxes are discussed, though there is no rigorous quantitative analysis used in the two chapters, or, indeed, anywhere in the book. Excise and sales tax receipts grew most rapidly, steadily rising in importance as domestic production and import substitution occurred in major consumer goods. The authors note with approval the ability of the government to hold down the growth of current expenditures, particularly civil administration. The failure of direct taxes on agriculture to rise, which is a central feature of the revenue developments since Partition, is discussed in detail.

The closest the book comes to an overall view of the economy is in the last chapter, though even there history and institutions dominate much of the discussion. The "failure" of the First Plan is outlined briefly. The Second Plan is reviewed, but the data were unfortunately not complete when the authors were writing this chapter, since they relied heavily on the March 1965 version of the Third Plan, for statistics. The evaluation by Andrus and Mohammed is unemotional, but suggests that things were going well, as most observers would agree. Their concern for the future, expressed also in the chapters on trade and the balance of payments, is that with its poor resource base (little known reserves of oil, iron ore, or coal) Pakistan will not be able to cope adequately with her balance-of-payments problems. For all their general orthodoxy on exchange rates and monetary and fiscal policy, the authors do not seem to be able to convince themselves that one can trade for those resources one does not have. The example of Japan as a resource-poor country does not appear to have come to mind.

It is on the issue of trade and the exchange rate that I have most difficulty in agreeing with the authors when they make their occasional forays into interpretive analysis or prescription. They do not like the floating multiple exchange rate system that the Export Bonus scheme introduced, and they say so in no uncertain language (pp. 76-82). They recognize at times that the currency has been and still is overvalued by the official exchange rate, but they do not appreciate how greatly overvalued it is. They appear to be saying that so long as one does not try to correct partially for a general overvaluation, no one will notice it ("The multiple rate features of the Scheme inevitably raise questions regarding the viability of the official par value . . ." [p. 81]). Thus, like the government of Pakistan, they appear to feel that the partial devaluation is a temporary thing, and when the exchange rate is once again unified, it will be unified much closer to the current official rate than to the devalued rate for minor exports (pp. 80-81). With domestic prices of imports averaging

about twice their c.i.f. prices at the official rate in 1964-65 [3], such a view seems most unfounded. The apparent success of the Export Bonus Scheme in getting resources reallocated to the exporting sectors is forgotten when the authors deal with questions of the appropriate exchange rate. They do not relate the performance of the economy in substituting domestic production for imports and in expanding exports to pay for imports, (which problems they see as principal obstacles to future growth) to the price of foreign exchange.

The detailed history of the legal and institutional framework in which trade and finance have developed, which absorbs most of the book, adds to its usefulness as a reference work for the specialist on Pakistan. The passion for detail, unfortunately, tends to stamp out a sense of perspective, particularly since the data for virtually all tables (and there are 51 of them) come from official sources. Availability of data collected by some official agency tends to provide the basis for the discussion. The insurance industry is extremely small, but the government publishes an Insurance Yearbook. Thus, insurance receives considerable attention. As a source of information on Pakistan, however, the book will be extremely valuable, since the authors do include data from sources hard to come by outside Pakistan.

The descriptive nature of the book tends to get out of hand, and the authors do not refer often to the growing body of analytical studies on Pakistan. As mentioned, Porter's excellent monograph on the monetary system [4] is not even in the bibliography. The authors ignore other published studies dealing with subjects they covered: the Export Bonus Scheme [2] (though they do refer to an early and shorter version), the effects of Partition and the development of interwing trade [5], and imports of foodgrains under PL 480 [1].

Because the book is divided so neatly by subject matter, the four chapters on monetary, credit, and capital market phenomena are easily accessible to economists interested in monetary and financial matters. The usefulness of the book to other economists is diluted by its failure to deal with interpretation and analysis of the material that the authors chronical and describe so well. The book's devotion to detailed history and institutions is its principal strength as an essential book in the library of anyone concerned with the economy of Pakistan and is its principal weakness as a book of general interest.

Stephen R. Lewis, Jr.

Williams College

References

1. Christoph Beringer and Irshad Ahmad, *The Use of Agricultural Surplus Commodities for Economic Development in Pakistan.* Karachi, Institute of Development Economics, 1964.
2. H. J. Bruton and S. R. Bose, *The Pakistan Export Bonus Scheme.* Karachi, Institute of Development Economics, 1963.
3. Mati Lal Pal, "Domestic Prices of Imports in Pakistan," *Pakistan Develop. Rev., 5* (4), 547-85.

4. R. C. PORTER, *Liquidity and Lending: The Volume of Bank Credit in Pakistan.* Karachi, Institute of Development Economics, 1963.
5. M. AKHLAQUR RAHMAN, *Partition, Integration, Economic Growth, and Interregional Trade: A Study of Interwing Trade in Pakistan.* Karachi, Institute of Development Economics, 1963.

Technical Assistance in Theory and Practice: The Case of Iran. By JAHANGIR AMUZEGAR. New York: Praeger, 1966. Pp. ix, 275. $15.00.

Mr. Amuzegar states that the purpose of his book is to "explore the possibilities and limitations of foreign technical assistance in promoting the socio-economic aspiration of the emerging nations from the standpoint of *their* interests and *their* needs." The study is concerned with Iran, and with the technical assistance provided by the U.S. Government aid missions. While thus limited the author believes his analysis and results may be of interest in a more general setting. Amuzegar considers three broad issues: (1) Iran's need for technical assistance and her capacity to accept such aid; (2) the supply and quality of technicians from the United States, and (3) the issues involved in a "rational transfer" of U.S. technology to Iran. To examine these issues he establishes ten criteria of *optimum effectiveness* of an aid program. These criteria have to do with clarity and coherence of program objectives, rational selection and coordination of methods, relation of aid program to host country's overall development effort, quality of foreign technical assistants, ability of advanced nation to supply aid, and other commonsensical notions as to what conditions are necessary to make aid effective. He then examines the U.S. aid program's fifteen years experience (1950-65) in Iran in terms of its success in meeting these criteria.

In most instances the aid program fell well short of acceptable performance. The program was widely and thinly spread over all aspects of the Iranian economy, with no central focus or guiding theme to hold it together. Thus it seemed to go in all directions and to lead nowhere. This, in Amuzegar's opinion, was true even after the extraordinary confusion in Iran associated with the oil crisis of the early 1950's was settled. Neither did the aid program fit with the other development programs that were under way. These difficulties were due in part to high turnover of key staff in the aid agency and in the Iranian government and in part to problems of cooperation between the two groups. Chiefly however they are explained by the failure or even the recognition of the necessity in the aid agency to draw up a coherent, consistent aid program. Thus projects foundered, and even when officially marked completed were generally ineffective.

Given the lack of a neatly worked out program with well defined slots to be filled, an extra heavy burden was placed on individual advisors, most of whom were not up to the task. Amuzegar finds U.S. personnel ill suited in many ways. They were equipped to do a reasonably effective job in the U.S. economy, but were often helpless in a different environment. They were not able to design new and ingenious approaches to problems that would fit easily and neatly into the Iranian economy. Emphasis is placed on the great differences

in culture, in work habits, in social organization, etc. between the two countries, and the difficulties created thereby for the direct application of U.S. technical procedures in the Iranian economy.

Also the aid program has been particularly ineffective with respect to fostering institutional and social change in Iran. This failure is explained by a variety of factors: tendency on the part of many U.S. advisors to advocate a naive transplanting of "free enterprise" and "democracy," reluctance of the aid agency officially to support alteration in institutions and organizations affecting the "national ethos" or the national character of Iran, and the belief in the minds of many Iranians that the whole program was politically inspired and existed simply because of Iran's proximity to the U.S.S.R.

There are numerous other arguments as to how and why the program fell short of meeting the criteria of optimality, but the ones noted are the principal and recurring themes. The central chapters of the book consist of citing examples and evidence elaborating and supporting the fact of failure and its reasons.

There is little doubt that virtually all points made by the author are valid and relevant in a study of the aid program in Iran. They are of course not new, and the reader conversant with aid programs in general will not be at all surprised by Amuzegar's report. The author is clearly *not* aiming a broadside attack against the program, and he presents his case with restraint. One might feel that he gives insufficient attention to the difficulties originating within the Iranian establishment. His Chapter 7—"Cooperation by the Recipient"—is concerned largely with why Iranians did not find aid personnel and the aid program effective. No attention is given, for example, to the continuing reduction in the proportion of oil revenues allocated to development projects nor to the reduction in effectiveness of the Plan Organization. These two events were not only disheartening to many, but contributed markedly to the difficulties involved in planning aid or anything else in Iran. A reader might also get the impression that the first two Iranian Seven Year Plans were much more complete, meaningful documents than they in fact were. It would have been helpful as well had the author discussed the frequently heard allegations of corruption among Iranian officials, and the negative effect that these allegations had on certain aspects (e.g., attracting foreign investment) of the aid program. Though there is this one-sidedness, it is understandable and the explicit points are surely valid. Despite this conclusion, however, the book is disappointing not only because it contains little that is new, but because the author missed opportunities to do something much more constructive than he did.

The major sources of information used by Amuzegar are some 450 completion-of-tour reports written by U.S. technicians on their departure from Iran. He tells us that he accepted these largely at face value (p. 9) despite the fact that he says the reports evidenced "conspicuous simplicity in diagnosing the problems involved or deeper understanding of the solutions required" (p. 219). He also did considerable interviewing and consulted other reports prepared by the aid agency as well. His method is essentially to assert something about the program, and then offer numerous quotes as supporting evidence. Quotations

no matter how numerous or how unanimous in point of view do not constitute clinching argument. In several chapters Amuzegar begins with a bit of analysis that indicates some insight and understanding, but rather than using this analysis to examine an aspect of the aid program he invariably lapses into presenting quotations. As a consequence the reader sees the difficulties pointed up, but is never shown exactly *how* and why such difficulties interfere with the aid programs. Such a demonstration can be made only by examining the operation of projects and the results themselves in a more analytical fashion than that permitted by a quotation-dominated approach.

There is considerable repetition in the book; all his points could have been made equally effective in many fewer pages. This is relevant because a more concise presentation would have permitted consideration of other issues pertinent to his subject within the book. We are told in Chapter 1 that a number of projects (research in seed improvement, agriculture training, public health, etc.) were "worth the effort." Of great interest and importance is the question of why these projects seem to work while most others did not. Indeed we would like to know how the author concluded they were "worth the effort." Similarly some technicians were quite effective. Attention devoted to explaining what—if any—common attributes were found among effective technicians would have added greatly to the effectiveness of his own study. Truistic generalizations on these issues are not very helpful.

Finally, and perhaps most importantly, it seems clear that understanding and appraising a specific aid program must be done in the context of a framework of the whole development process in the given country. A short chapter on the general development effort in Iran would have helped. More troublesome is the fact that the scattered remarks on this subject suggest that the author has yet to work out his own ideas. His discussion at the beginning of Chapter 7, for example, indicates a view about development and the role of aid in development that makes them much simpler and much more mechanical than has been shown in most of the development literature. It is important to examine aid projects against demanding standards, but these standards must emerge from a realistic recognition of our current understanding of economic development, and they do not do this in this book. Consequently much of Amuzegar's criticism reflects the state of our understanding about development rather than the peculiar qualities of a particular aid program. Carefully relating the aid programs to a more sophisticated and complete approach to development would thus be more revealing about the contributions that technical aid can and has made than repeating quotations to the effect that the aid program was confused.

So, for the reader unacquainted with problems of technical aid and interested in a readable survey of these problems, the book has merit. In terms of revealing something about the mechanics of effective aid programs, and of seeking an explanation of exactly how and why certain factors affect an aid program, the book is disappointing.

HENRY J. BRUTON

Williams College

Diagnosis of the Brazilian Crisis. By CELSO FURTADO. Translated by Suzette Macedo. Berkeley and Los Angeles: University of California Press, 1965. Pp. xxiv, 168. $5.00.

When Celso Furtado was executive head of the Agency for the Development of the Brazilian Northeast and Minister of Planning in the Goulart government, a friendly critic referred to him as a "master reform monger." Now that he is in exile, the reforming fervor he displayed as an administrator manifests itself in his writings. The present essays, he indicates, have a twofold purpose: one is to define the essence of the problem of underdevelopment; the other is to identify the causes of the current crisis in Brazil.

In the first part of the essays, Furtado reveals himself to be a disciple of those who find the essence of underdevelopment to be a conflict between the dynamics of technological change and the rigidities of the social order. This is the Marxist contradiction between productive forces and social relations—only for Marx this contradiction did not acquire revolutionary implications until industrial maturity had been achieved. Until then the bourgeoisie was a progressive social element which favored and promoted social and economic change. Furtado, however, sees this contradiction ripening for *dualistic* underdeveloped countries during the phase of early industrialization. He seems to attribute this to the failure of the bourgeoisie to dethrone the feudal elements. Among the reasons why the bourgeoisie has not predominated are: first industrialization has not proceeded too far and is too centralized geographically due to externalities and internalities to give the bourgeoisie sufficient political and economic power; second, the political organization favors the rural-based traditional oligarchy as opposed to the populous urban centers; third, the industrial interests are intermingled with the landed interests; and fourth, the growth of class consciousness in the working masses is a very slow process in these societies. Under these circumstances, the bourgeoisie and the feudal elements cannot resolve their many antagonistic interests. Instead, an uneasy alliance between them prevents any realistic plan for national development. Apparently, the contradiction is that the social and political structure takes on a rigidity which is not able to meet the changing demands of an increasingly industrialized, urbanized society.

In the second part of the essays, Furtado diagnoses the post-1962 Brazilian crisis. In his view, the crisis was not caused by economic factors per se. The resource base—excepting coal—and the industrial park are seen as being ample to support and induce a rapid rate of growth. However, the dynamic impulse from these sources has been stifled by numerous social rigidities. The traditional oligarchy, entrenched in the rural-biased state machine, has been powerful enough to frustrate those reforms contrary to its interest. The bourgeoisie, limited to relatively few geographic areas such as Sao Paulo, has not been able to provide effective leadership, etc. But the traditional institutions are no longer adequate, for new forces and problems have been emerging. The first of these is the rapid urbanization which has resulted from industrialization, an expanding market for services, and rural stagnation. This heterogeneous urban population has become the decisive new factor in Brazilian po-

litical struggles in that it constitutes the basis for the recent Populist movement. The second is that the standard of living for a great part of the rural population has been decreasing, even during the rapid growth of the 1950's, as a result of the natural exhaustion of soils and the moving of farms inland from the principal consumption centers. In contrast, there is extreme underutilization of arable land on the great estates. Furtado predicts that unless basic changes are introduced into the social structure, the principal one of which would be a political reform aimed at incorporating the working masses into full political activity, the tendency for the rate of growth to decline will continue and structural problems will worsen.

In the final essay, Furtado examines the social and economic contradictions that abound in the Northeast of Brazil. Though this region is one of widespread poverty, the peasants have typically lacked any sense of class consciousness. Poverty was attributed to the cyclical droughts—a force of nature—rather than to the exploitative social institutions. Fazenda workers lived in an isolation from one another that prevented any effective organization to combat the local feudal power. One startling contradiction, Furtado indicates, recently developed in the archaic sugar industry. After long losing out in the domestic market to the more efficient production of the south, new export possibilities arose for this main product in the Northeast after the Cuban Revolution. The estate owners expanded production by usurping the isolated plots of infertile land previously alloted to peasant workers for subsistence crops. As a consequence, the unit cost of sugar increased even though the peasant's real wage had fallen. Driving the peasants from their subsistence plots had far reaching consequences. For the first time, the peasants who were pushed together in small settlements solidified into a political force which was later instrumental in extending the national minimum wage to the estates. But a new contradiction was created. The region is even less economically viable than before. And the rigidities of the present social organization are not likely to change, for neither the peasants nor the feudal groups are now in favor of any realistic reforms.

These essays are with minor exceptions provocative and stimulating. Despite these virtues, however, the product is not entirely satisfactory. The general contradiction theme has the virtue of simplicity, and is deceptively attractive to activists and reformers who are always searching for grand one-stroke solutions. Furtado partially avoids this pitfall by admitting the complexity of any historical situation. He sets out what he thinks are some of the necessary conditions for economic and social progress, but he does not predict that policies toward these ends will be sufficient. Still, the general contradiction theme seems to raise more questions than it provides answers, and may not be testable.

In this reviewer's opinion, Furtado's conclusions regarding Brazil's social and economic development for the coming years are at the same time too optimistic and too pessimistic. Economic factors do not, by themselves, lead to expectations of complete optimism. Furtado exaggerates the ease with which arable land can be brought into efficient use by ignoring the large infrastructure investments that would be required. He also neglects to recognize the im-

portance of the bottleneck created by Brazil's weak capacity to import. Political reform will not readily remove such obstacles. On the other hand, social and political conditions are not as unsatisfactory as he paints them. While Castello Branco's military regime has neither extended political power to the masses nor brought about all of the needed reforms, it has taken some positive steps. For example, land reform is slowly under way. In addition, the fact that many demagogues have lost their political rights may reduce the dangers of extreme populism. The present government has also reduced the rate of inflation from runaway proportions, and the rate of growth of real product per capita has been increased from an average −.8 per cent in 1962-64 to an estimated + 2 per cent or more in 1965. Finally, a new comprehensive economic and social plan is in process of completion. Thus, while we can all share Furtado's desire for more rapid progress and reform, his gloomy predictions do not seem justifiable.

Given these reservations and criticisms, these essays are worthy of attention. For those who would use them as an only source, however, Furtado's statement that ". . . with the intention of promoting an intellectual mobilization of the left, I wrote the present book . . ." (p. xiii) should be sufficient warning.

DONALD HUDDLE

Rice University

Obstacles to Change in Latin America. Edited by CLAUDIO VELIZ. New York and London: Oxford University Press, 1965. Pp. viii, 263. $6.75.

This book is a collection of essays prepared for a 1965 conference by ten Latin American scholars, most of whom have held responsible administrative positions in national or international development agencies. In a brief introduction Claudio Veliz develops admirably the bases of the views expressed in most of the essays that "the changes which preceded and followed the industrial revolution in Europe . . . [will not] automatically accompany the industrialization of Latin America" and that "the 'classical' Marxist or liberal models of development based on the experience of the more advanced countries of Europe are not applicable to Latin American conditions." The essays, and particularly the Introduction, largely support the model, advanced more than a decade ago by Paul Baran, of the new industrial middle classes joining the landowners in a coalition of property owners who resist the institutional changes which are necessary for rapid economic development.

Five of the essays attempt generalizations concerning Latin America as a whole; these are "Political Aspects of Economic Development in Latin America" by Aníbal Pinto, "Populism and Reform in Latin America" by Torcuato di Tella, "Land Tenure and Development in Latin America" by Jacques Chonchol, "Implications of Foreign Investment in Latin America" by Victor Urquidi, and "Disunity as an Obstacle to Change" by Felipe Herrera. The remaining five essays are concerned with specific countries; these are "Change and Frustration in Chile" by Osvald Sunkel, "Political Obstacles to the Economic Development of Brazil" by Celso Furtado, "The Dynamics of Brazilian

Nationalism" by Hélio Jaguaribe, "Violence and the Break-Up of Tradition in Colombia" by Orlando Fals Borda, and "Mexico: The Lop-Sided Revolution" by Moisés Gonzalez Navarro.

As is indicated by the above list of titles and authors, the book deals largely with the institutional framework within which economic development occurs and is written by experts from the related disciplines of economics, sociology, political science, and history. Together the essays present a sufficient amount of historical-descriptive material to provide, for the economist who is unfamiliar with Latin American history, the background necessary for an appreciation of the importance of the various political issues which are discussed. The book introduces only familiar obstacles to economic development, but it will prove particularly valuable for social scientists from the more developed countries who are interested in obtaining a better understanding of the views of their counterparts in Latin America.

ROBERT L. BENNETT

University of Maryland

The Genesis of Modern Management: a Study of the Industrial Revolution in Great Britain. By SIDNEY POLLARD. Cambridge: Harvard University Press, 1965. Pp. 328. $7.95.

My heart sank when, upon receipt of this volume, I discovered its subtitle. In a single scholarly generation a subject can be ground and sifted enough to cause feelings of panic and dismay when further research comes from the presses. Considering the output of the last decade on the early British industrial revolution, together with the meagre additions made to either hard or speculative knowledge about it, an injunction might reasonably be sought to relieve the sufferings of the innocent victims who must buy and read these tomes in order to maintain their standings as "experts" or "students of the subject."

The news I have for the scholarly community thus might seem a mixed blessing. Pollard's new book is an interesting contribution in general, and sui generis in management studies. Pollard has brought together historical materials on management development which seemed exhaustive enough to me, but which, he says apologetically, were merely the limits of "the powers of a single pair of eyes." However, the book is a short one—as things go in such matters. Of the 328 pages, 53 are devoted to footnotes and 3 are index. The actual minimal burden is therefore only 272 pages.

Pollard opens new territory on a frontier one had thought was getting near its last wispy margins. His focus is upon the way management in business groped its way into existence with the first requirements of the modern commercial and industrial world, a time when the typical manager owned his own establishment. What manner of man was this "hard-bitten owner of dark, satanic mills . . .?" As Pollard describes the "representative" manager (p. 258). And what, precisely, did management contribute to human progress in this early era?

There are several singular aspects in Pollard's answers to these and similar questions. I list three. First, he eschews the subjects usually classified as en-

trepreneurship, concentrating instead upon the men and tactics involved in internal business control. As a result, we have a close study of how men learned to run factories, mines, public utilities, railways, estates, and commercial ventures of all sorts. We find early efforts to adapt known commercial accounting methods to new uses, to set up enterprises physically in ways which were cost reducing, buildings, inventory organization, locations near water, raw materials and so forth. We see early efforts to build "staff" in manufacturing and to set up standards of managerial performance. These he considers as management functions narrowly defined, and these are what he studies. Second, he uses modern management as his exploratory paradigm as he wades through these historical materials, so that there is little blind writing and the reader is spared mental "wipeout" as the pages proceed and the decades, industries, men, and problems slide past. Third, he watches carefully, again on the basis of the modern paradigm, for management activities of the period, which are not such today, which were sloughed off as British society changed, and he shrewdly shows why management no longer faces these problems.

The result is fresh and useful. Pollard's approach to early labor problems which came as industry grew literally on the top of rural England is an interesting mixture of Karl Polanyi and Karl Marx which yields some good dividends. We see the connection between those ancient disputes and problems of developing "disciplined" labor forces in backward lands today—problems to which M.D. Morris pointed a few years ago to the surprise of the partial-equilibrium chaps who took the supply of labor as "given." Pollard's book adds valuable information on the question of whether or not labor was scarce in the new industries. We also see in his book why modern management is made possible by the development of an industrial society, a fabric around the factory which relieves the manager of the need to have a heart of stone or else make like Robert Owen. Schools, hospitals, relief, transport, sanitation and so forth are now problems of society at large. Much of modern managerial practice was discovered long ago, and much had to be rediscovered painfully in modern times. It was not passed on by the pioneers. Pollard tells us why. As I said, the book is a contribution to our knowledge of the Industrial Revolution in Britain in general, and it is a "must" for students of industrial bureaucracy and its development. The footnotes are at the back of the book, but the page numbers are given in the left-hand margins so that the usual deadly struggle to find the coordinates of the scholarly apparatus in such books will not delay the careful reader here. Since "footnotes at the end" are apparently necessitated by printing costs today, one can only hope that the system used in this book will become general practice.

J. R. T. Hughes

Northwestern University

Growth and Structure in the Economy of Modern Italy. By George H. Hildebrand. Cambridge: Harvard University Press, 1965. Pp. xx, 475. $11.95.

There is by now a tradition of studies by American economists on Italy. The latest is Professor Hildebrand's work, the title and size of which make it

appear the most ambitious of all. To title and size alone, however, must any ambition be confined, for this book contributes but little to an understanding of the "Italian case" and its many problems. Still, it is on these problems that the book ought to shed some light. Thus, Part I and a section of the concluding chapter are devoted to a description and interpretation of the growth process in general, from monetary stabilization in the late 'forties to the long boom which, at unprecedented pace and under conditions of internal and external stability, went on until 1962. Parts II and III deal with the persistence of a surplus of labor through the 'fifties and with sectoral and regional inequalities: a lengthy and painstaking analysis of population, labor force and unemployment data and of the evolution of the wage system and a rather summary account of the Southern problem are followed by an attempt to find the common causes of both evils. The concluding chapter examines the recent breakdown of the boom, current issues and future prospects.

The author's commendable intention in approaching these problems—to use economic theory "to illuminate portions of [the] obscure terrain" where history, culture, institutions exert their influence (p. viii)—unfortunately remains unfulfilled. The theory appearing in the book is a demand and supply equilibrium model for the labor market by which the one analytically relevant result of the whole enquiry is reached: that full employment and economic unification were delayed by wage rises in the advanced sector *prior to 1961*. Hildebrand does not argue in terms of the frequently used, and misused, relationship between investment and surplus over wages. His version of the story, largely repetitive of an earlier one presented with greater clarity and vigor by Mrs. Vera Lutz, runs more or less as follows (net of some contradictions). The slow rise of employment (little more than 3 per cent per year, less than 4 per cent for industry) and the persistence of surplus labor in the face of average yearly growth rates of 6 per cent for GDP and nearly 9 per cent for industrial production, together with an "elasticity of output with respect to employment" even higher than in West Germany, point to a labor-saving bias of investment. In the author's opinion this is confirmed and explained by the fact that, notwithstanding surplus labor, industrial wages rose (about 5 per cent a year in money terms, about 3 per cent real, for 1951-61). Wage pressure was made possible by imperfections in the labor market: in the author's view statutory restraints to internal migration prevented the flow of Southern unemployed to the North and were of paramount importance in permitting the upward pressure of wages for unionized labor. True, wages rose much less than output per man, especially in the advanced sectors, so that unit labor costs fell considerably. But this was the result of entrepreneurial counterstrategy to wage rises, i.e., labor-saving investment. Noncompetitive conditions in the advanced sector, on the other hand, allowed entrepreneurs not to pass on the savings in costs to lower prices, reinvesting the higher profits with a "socially uneconomic emphasis upon labor-saving" (p. 370). Thus "anticompetitive determination of labor costs" checked the growth of employment in the advanced sector and diverted scarce capital to labor-saving investment (p. 373 and *passim*), preventing the progress of the weaker sectors and regions.

Such attribution of a major independent responsibility of growth disequilibria in Italy to wage behavior, via factor substitution, is unacceptable. Leaving aside obvious theoretical objections, mention of a few factual points will suffice. (1) Legal restraints, though affecting the transparency of the market, never prevented migration whenever job opportunities were available: migration from South to North, 1951-61, was some 1,200,000 units, half of which occurred in 1959-61, when the boom gathered speed and the unemployment rate fell considerably. (2) Evidence points to some fall of capital-output ratios in Italian industry, with certainly no fall in profit rates: how is this to be reconciled with labor-saving movements along, or shifts of, production functions induced by wages? (3) Labor costs per unit of output fell in Italy relative to those of all other Western industrial countries over the period: should one believe that Italian entrepreneurs were so much better than their foreign colleagues in adapting technical progress to wage movements? (4) Hildebrand, finding any evidence of even limited technical rigidity too scanty, argues that entrepreneurs always find ways to bend technology to factor prices and eliminate rigidities and frowns upon the fast rise of productivity in the Italian advanced sectors (chemicals, metal products, etc.): had wages not risen, he implies, more labor-intensive methods would have been devised. But one should like to be provided with some evidence, even scanty, of more or less full flexibility, *for any given size,* in say, oil refining, steel making, or petrochemical production. At any rate, two factors, neglected by Hildebrand, loom large in accounting for the rapid rise of productivity in Italian industry and especially in its advanced sectors. First, Italian industry after the war was in a position of technical inferiority and began to catch up with the technical advance which had occurred in the West: it was not a matter of substituting factors but of introducing *superior* technologies. Second, in the advanced sectors there is a strict relationship between productivity and scale. It is not by accident that such sectors in Italy show the greatest increase in output, the greatest fall in unit labor costs, and the greatest gains from the export boom.

Failure to see these points prevents an understanding of the whole Italian growth process, which was led by foreign trade, permitted by a lesser increase of wages, relative to productivity, than in most other countries, and speeded up by the interplay between rising demand, investment, and productivity. Lacking, with the one exception examined, an effort of economic analysis and interpretation, what remains is a collection of data, facts, and highly personal opinions. The book can serve as a useful and interesting reference on some matters. But as a source it is neither exhaustive (many official sources or documents are neglected, there is no appraisal of the role of State-controlled enterprises or account of the changing trends of State intervention in the South, etc.), nor always reliable, nor up to date (e.g., 1961 census data are not used). Actually, one has the feeling that most of the book was written before 1959, not only because of the outdated information but also because of the general outlook and the failure to understand the remarkable changes that the structure of the Italian economy and the problems to be faced underwent later. It is thus not surprising that no satisfactory analysis is provided of the recent interruption in the growth process. Among other things, Hildebrand is led to be-

lieve that in Italy monetary policy "would encounter great difficulties in coping with inflation" but "has a somewhat easier task in coping with a fall of effective demand" (pp. 102-3), which is exactly the opposite of what has happened since 1963.

LUIGI SPAVENTA

Perugia University

Polish National Income and Product in 1954, 1955, and 1956. By T. P. ALTON AND A. KORBONSKI, B. MIECZKOWSKI, L. SMOLINSKI. New York: Columbia University Press, 1965. Pp. viii, 252. $6.00.

This book is the third in line from the Research Project on National Income in East Central Europe of the Institute on East Central Europe at Columbia University, and is part of a larger study on the structure, growth, and performance of Soviet-type economies. This is a result of a team effort and the authors are to be commended for undertaking such an important task and providing us with valuable, painstaking, detailed, and especially well documented summary of their research.

The period surveyed is very significant as Poland was emerging from the Six-Year Plan period, characterized by forced and hastened superindustrialization, flagrant disregard of efficiency, and a low consumption level, into a period when the most striking blunders of the Six-Year Plan were being rectified, and the population was being pacified and wooed by increasing the consumption rate and redirecting some investments to agriculture. The study's purpose is to show the relative contribution of various sectors of production to the GNP and to analyze the relative share of output to final uses. The authors "are not concerned with the rate of growth or with the size of Polish economy in comparison with other economies." First, they set their task in Polish current transaction prices (called current market prices) in order to "show the actual articulation of the various sectors," and second, in adjusted factor cost (taking as point of departure the celebrated contribution by Professor Bergson) "to exhibit the allocation of resources in more meaningful terms than market prices alone permit" and to provide "an insight into the dimension of economic activity" in terms of the services of labor and nonlabor productive agents. The factor cost structure is correctly considered as the fundamental aim of the research whereas "market prices" is essentially an intermediate step.

In order to arrive at GNP (including "nonmaterial" production) by sector of production at factor cost, the adjustment consisted in aggregating for each sector of origin the return to labor and nonlabor productive agents. For the former, actual data were virtually accepted and the latter were derived by distributing the aggregate nonlabor returns among the various sectors according to net (depreciated) current replacement costs of fixed and working capital. The result obtained depends on the size and rationality of the numerator, reliability of capital valuation, the rates and reckoning of turnover of capital, correct estimation of depreciation, and the adopted uniform rate of return to capital in all sectors.

As a result of the imperfection of market prices, mainly because of distorted

incidence of turnover tax and, to some degree, the formal character of accounting profits and subsidies, the relative contributions of sectors of production or of final uses are deemed to be seriously misleading as to the relative significance of these sectors in terms of the services of productive agents utilized by the economy. Admittedly the structure of GNP at market prices is a misleading guide for resource allocation but displays the "articulation of economic activities" and those current prices serve for international comparisons of national product and components. Since the sectoral profits do not reflect correctly the share of profit and returns on capital, the authors argue that it is necessary to attribute to the turnover tax the bulk of the returns to nonlabor productive agents. As a result of the deficiency of market prices, generally one zloty worth of consumer goods reflects a smaller resource cost computed at factor cost than does one zloty of investment goods. The results of revaluating GNP at factor cost are far reaching, to wit at Polish market prices the share of industry was about twice as large as that of agriculture, whereas recomputed at factor cost agriculture becomes a slightly larger sector. Also the weight of housing jumped sharply from about 1.5 per cent at market prices to about 11 per cent at factor cost for at market prices the rental rates are nominal and, in adjusting, allowances were made for a rather high rate of depreciation and a uniform return on capital which due to high capitalization in this sector increased the factor cost sharply. Whereas the share of personal consumption in GNP at market prices amounted to about 60 per cent, at factor cost it was about 55 per cent. But by excluding rental, personal consumption indicates a drastic drop to about 44 per cent of GNP.

The fundamental assumption is that the size of GNP expressed in zlotys is identical whether reckoned at market prices or factor costs and within this total the returns for the services of labor roughly correspond to differences in marginal productivity of labor in various occupations as depicted in the recorded market prices, with the residual fully attributed to the nonlabor productive agents. The authors add "whether other assumptions would have greater merit we leave to the reader to consider." For purposes of factor cost adjustment the turnover tax is identified as a return to nonlabor productive agents. Prices of consumer goods include a differentiated rate of turnover tax which is essentially a device for siphoning off purchasing power from the population. Hence the return to nonlabor productive agents is realized through the medium of consumer prices. Consequently there is a divergence between where the tax originates and where it is collected. The profits in various sectors reflect the pricing policy and are dictated by fiscal convenience and, apart from the nature of profit, they cannot be deemed to be directly related to the contribution of nonlabor factors in given sectors. Profits and subsidies are considered to be of a similar nature to turnover tax. They are accounting categories and their source cannot always be equated with the point where they are extracted. In arriving at the GNP at factor cost, profits less subsidies were lumped together with the turnover tax, and the authors consider this as essentially constituting a net return to state ownership and control. The returns to nonlabor factors consist of return to capital, land, and entrepreneurship. Interest was not identified as a distinct return to capital, distin-

guished from profits and returns to entrepreneurship in adjusting the return to capital. It was assumed that the actual return to agriculture at market prices merely consists of return to labor, and no explicit allowance for land as a productive agent was imputed. Hence the returns to nonlabor factors were essentially attributed to capital (adjustments were made to rectify the meager depreciation charges).

The reader should be particularly grateful to the authors for supplying such elaborate appendixes (about 142 out of 242 pages of text), detailed sources, supporting documentation, estimates, and explanations of the methodology of adjustments. Some of the findings and arguments may be controversial. One could question certain assumptions and wish for more elaborate statement and justification of the assumptions' validity and the rationale behind the treatment of turnover tax as a return to nonlabor factors of production. Nevertheless one is impressed with the solid and sober approach to the problem. Undoubtedly this is a useful contribution even if narrow in scope. The reader should find it particularly interesting since the broader economic implications of the authors' findings will be incorporated in a study of Polish economic growth from 1937 to 1962 in preparation under the auspices of the Institute.

GEORGE R. FEIWEL

University of Tennessee

New England: A Study in Industrial Adjustment. By R. C. ESTALL. New York: Frederick A. Praeger, 1966. Pp. xv, 296. $7.50.

In the economist's (but probably not the public's) rank ordering of real, potential, and imagined economic ills, regional economic problems come somewhere near the bottom. However, this chronicle of New England industrial experience over the past forty or so years brings out the enormity of the pressures which can be exerted on a regional economy and the adjustments which are required in order to maintain something resembling prosperity. The decline of the textile industry is the most dramatic case in point. Employing 420,000 persons or 30 per cent of the labor force in manufacturing in New England in 1919, the industry declined at an average rate of 8,000 jobs per year for the next forty years. From such declines, recovery is neither easy nor automatic.

Estall offers valuable insights into the problems of recovery and adjustment in mature industrial economies. He examines the fortunes and misfortunes of the major New England industries with separate chapters on textiles, electronics, shoe and leather trades, paper, aircraft, and machinery manufacture. Other industries are treated in less detail. His discussion is largely confined to manufacturing (which for New England is justified) and relies on published data. The location and structure of industry are explained on such bases as wage differentials, availability of required labor skills (particularly R & D expertise), and proximity to markets and raw materials. He does not trace the causal chain beyond this. For example, no attempt is made to explain why wage differentials exist or to explore the reverse effects, those of industry location on wages.

Rather than examining New England as an agglomerate whole, considerable

attention is devoted to the dispersal of industry within the region. New England's early basic industries were widely scattered. The new industries which have tended to rejuvenate the New England economy since World War II have been geographically concentrated, generally being confined to the Boston and western and southwestern Connecticut areas. The remaining areas of New England have received few spillover benefits and remain in considerable distress. Constructing a crude but, nevertheless, ingenious "Index of Relative Dispersal" Estall shows that the five most widely dispersed industries in New England (lumber and wood, pulp and paper, textiles, food and kindred products, and leather) have suffered either absolute declines in employment or are growing at rates substantially below the industry national average. Together, these five industry groups suffered a 25 per cent drop in employment between 1939 and 1958. On the other hand, the five industry groups showing the greatest index of geographic concentration (instruments and related products, electrical machinery, primary metals, transport equipment and ordinance, and miscellaneous manufacturing) had a 98 per cent rise in employment over the same period. Recent growth in New England has indeed been uneven and localized.

Estall concludes with a policy chapter in which he examines the steps taken to alleviate the widespread pockets of distress remaining in New England. Generally, his argument is that there has been too much promotion at the expense of planning, and that local and, to a lesser extent, state governments have inappropriate jurisdictional units and often inadequate resources to carry out effective "self-help" programs. He's probably right.

This book is a fairly good case study of a region and one that should be completely understandable to noneconomists. It may and should stimulate case studies of industrial development and decline for other regions in the United States. Such studies would be of value to geographers, regional planners, transportation economists, and others working in regional economic development. Although readers familiar with the New England economy will already be familiar with much of what this book contains, Estall intertwines many threads and presents an integrated, overall view of the economic geography of the region. Surely, a reading of Estall's book is recommended to anyone working on the economic development of New England.

J. T. ROMANS

State University of New York at Buffalo

Statistical Methods; Econometrics; Social Accounting

Operations Research and Quantitative Economics: An Elementary Introduction. By HENRI THEIL, JOHN C. G. BOOT, AND TEUN KLOEK. New York: McGraw-Hill Book Co., 1965. Pp. xiv, 258. $8.95.

This book presents a survey of the methods and accomplishments of economics and operations research at a nontechnical level. It is written primarily for the nonspecialist with only a high school background in algebra. The authors have selected a number of simple and interesting examples to illustrate the

various techniques. In spite of the statement on the flyleaf (presumably not written by the authors) that the book "integrates econometrics and operations research," the book does not attempt any such integration. Rather, it provides for each of these an independent elementary introduction.

The operations research techniques described are linear programming, input-output analysis, critical path scheduling, queuing theory, production and inventory theory simulation, elementary probability theory, and elementary game theory, all illustrated by numerical examples. For the most part, the mathematically illiterate reader will be able to follow the illustrations and acquire a certain amount of intuitive understanding of the techniques. In a few places, however, such is not the case. For example, the queuing theory chapter uses the "rabbit out-of-the hat" technique to arrive at formulas for the length of line and average waiting time. The reader gets no sense at all of the why's or wherefore's of such formulas. The selection of operations research topics is representative, the main serious omission being dynamic programming.

The econometric chapters include a simple multiplier model, Klein's model of the U.S., forecasting based on econometric macromodels and on input-output models, scatter diagrams, the least squares method, a very brief description of some properties of statistical estimates, and a chapter on the empirical side of consumer behavior which includes a discussion of index numbers, price and income elasticities, and budget surveys. The authors have succeeded in packing in a large number of concepts in a simple yet intuitive manner which is well suited to the purpose of the book.

The book will not prepare the reader to set up and solve models himself, either of the operations research or of the econometric variety. However, this would be too much to expect in so short a book. The authors have provided an "understandable survey," as promised, and it is recommended reading for economists who are interested in a nontechnical bird's-eye-view of econometrics or operations research.

T. M. Whitin

Wesleyan University

Economic Systems; Planning and Reform; Cooperation

Comparative Economic Systems—Competing Ways to Stability and Growth. By Allan G. Gruchy. Boston: Houghton Mifflin Company, 1966. Pp. x, 908. $9.95.

Professor Gruchy's volume is in fact composed of three separate books. After a general introductory discussion of the functions and goals of an economic system, Gruchy examines "Capitalism" in Book One. Capitalism is represented by the United States, Great Britain since 1951, the Federal Republic of Germany, and France. Book Two, entitled "Socialism," deals in considerable detail with two Scandinavian countries (Norway and Sweden) and with the British economy during the six immediate postwar years. Book Three, called "Communism," discusses the Soviet Union and touches on Yu-

goslavia and Mainland China. Each book is a self-contained unit complete with an historical introduction and with appraisals of the system's performance and outlook for the future. A short chapter on trends and prospects of the three competing systems concludes the volume.

The interest in the study of comparative economics has increased substantially during the postwar years, and with the interest, our knowledge of how individual systems operate. Gruchy had to make two difficult decisions: first, the choice of countries to consider; and second, the particular approach to take and the topics to consider in his study of various national economies.

The author's emphasis is on the United States and the Western Socialist countries. If the choice of countries is to be determined by their importance at present and in the near future, as the author claims, one may well argue for more extensive treatment of the recent French and Yugoslav experiences, which are at least as important as Scandinavian Socialism. The French indicative planning has generated more than casual interest in other West European countries and in the United States, and the Yugoslav experiments with market Socialism have been followed by several countries of the Soviet bloc and considered by the Soviet Union itself. Yet the treatment of both France and Yugoslavia is relatively brief.

Gruchy attempts to integrate the historical, ideological, and institutional aspects of the individual economies. The amount of information he succeeds in making available about each within one single volume is amazing: it includes economic history and the history of economic thoughts, political structure, the resource base for economic development, data on output growth and budgets, national income accounts, and one input-output table. Yet I suspect that the student will be at a loss when faced with such *embarras de richesse*. A cross-sectional comparison of such topics as actual resource allocation, price formation, investment decisions, or economic incentive in the three systems considered is rather difficult and this is one of the basic aims of comparative study. The author's concluding chapter on trends and prospects for the three competing systems, where integration and analysis could have been attempted, is very brief.

Both the choice of countries and of topic coverage are of course a matter of taste. Although a carefully chosen series of paperback monographs on individual countries written by experts in the respective fields (for example, Campbell, Lutz, Nove, Waterston) does offer an attractive alternative to a standard text such as Gruchy's, his book will be welcomed by those who prefer to have extensive information on various countries under one cover and who stress the Western-type economies in their comparative systems courses.

George J. Staller

Cornell University

Political Economy, a Marxist Textbook. By John Eaton. Rev. ed. New York: International Publishers, 1966. Pp. 255. $5.00.

The reviewer's interest in this revision of an obscure British textbook reflects a quest for extensive supplementary reading, suited to English-speaking undergraduates, in a Marxist counterpart of their own "bourgeois" texts

in *Principles*. Such a source should ideally be at or above the intellectual level of the standard Anglo-American courses. It should be shorter and less erudite than Dobb or Sweezy or Meek. It should be less dated than Marx's or Engels' economic pamphlets, or Boudin's digest of *Das Kapital*. It should poison fewer wells, be at once less vitriolic and less preachy, and be rooted closer to the students' own world, than any Iron Curtain text I have yet seen in translation.

Mr. Eaton's book, probably a cooperative venture in fact, is, for the moment, among the "least worst" of a disappointing lot of candidates. Its tone is generally moderate; it blends (conjectural) history and (Marxist) economic analysis attractively; it is short and reads easily; its author(s) is (are) obviously familiar with standard economics, although the theoretical level seems somewhat lower than the remainder. The book, despite its chalky-pedagogical subtitle, is designed primarily for high-school-level study courses in informal circles of "progressive" workers, whose members and leaders have usually had only tangential contacts with any other sort of economics. As a result, it passes our "intellectual-level" test only marginally, with a graduate B-minus. Its treatment of non-Marxian economics serves as an indicator in this respect. Proportionately much longer than the treatment of Marxism in "bourgeois" counterparts, it is on balance no fairer and its level of competence a shade lower. We may be grateful that the "howling hyena—learned lackey of Wall Street—intellectual running-dog of imperialism" type of criticism is absent. Replacing it is the relative mildness of such strictures as the following (p. 27):

> [T]he development of the labour theory of value . . . enabled Marx to show the nature of capitalist exploitation and that capitalism itself was doomed. . . . The defence of capitalism called, therefore, for an attack upon the labour theory of value. Bourgeois theory was quick to sense this and from about 1830 has been in quest of an economic theory that rejected the labour theory of value.

An American re-issue of a 1963 English revision, this text reflects the "competitive coexistence" stand of the Khrushchev period. By contrast, the first edition, reviewed by Abram Harris in the June 1951 issue of this *Review* (pp. 457-59), antedates the demise of Stalin. The differences are staggering, and speak well for the intellectual flexibility of Eaton and his colleagues, if any. The features which raised Professor Harris' blood pressure 15 years ago were, for the most part,

> . . . all spirits, and
> Have vanished into air, into thin air.

One might, for example, describe this edition too as bound "in a strait-jacket of the traditional Marxian theories," but the jacket is at least padded. Harris' 1951 collection of specific "horrors" are mostly gone, and an original Foreword (by Emile Burns), dedicating Eaton's book to rescuing the British worker from the " 'mixed-economy' and 'right-wing' leaders who wish 'to save British capitalism at the expense of the working class and the colonial peo-

ples' " is gone too. "Modern revisionism," in short, has improved substantially upon a Stalinist original. Potential readers would do well not to await a third edition, which may mirror "the thought of Chairman Mao," unmentioned this time!

M. BRONFENBRENNER

Carnegie Institute of Technology

Business Fluctuations

Cost, Prices, and Profits: Their Cyclical Relations. By THOR HULTGREN with M. R. PECH. National Bureau of Economic Research Studies in Business Cycles No. 14. New York and London: Columbia University Press, distributor, 1965. Pp. xxvi, 229. $6.00.

Hultgren has written a book in the best tradition of the National Bureau of Economic Research. It shares in both the virtues and deficiencies of that methodology. This book will become a basic reference on the cyclical behavior of unit costs, prices, and their combined effect on profits and profit margins.

The principal results are by now reasonably familiar, but the solidity of the data basis supplied by Hultgren serves to strengthen the empirical foundations based on Hultgren's earlier pioneering work [2], Soligo [4], O. Eckstein and T. Wilson [1], and myself [3] (the latter three containing econometric studies of labor productivity for the U. S. economy) not to mention several closely related studies using British data. Deflated quarterly sales and costs from the SEC/FTC. *Quarterly Financial Reports for Manufacturing Corporations* for the period 1947-61, benchmarked on annual Federal Reserve Board output indexes, and wholesale prices, provide the basic data. Hultgren's findings are:

(1) Prices rise throughout a cyclical expansion, but most rapidly toward its end. After the cyclical peak, prices continue to rise but at a somewhat slower rate, falling very slightly by the very end of the recession.

(2) Unit costs, principally for labor, typically fall during the early stages of a cyclical recovery and rise moderately in late phases of the expansion. The early cyclical downturn in output is accompanied by the most rapid unit cost increase of any cycle period, while the terminal stages of the recession correspond to modest unit cost declines.

(3) The explanation of this cost behavior depends, by definition, upon average hourly earnings and manhours per unit of output. Earnings increase at all times, but most rapidly when output is expanding. The behavior of manhours per unit of output lies at the core of the observed cyclical pattern in unit costs. Manhours per unit of output (the reciprocal of labor productivity) fall through the expansion phase, but most rapidly during the early recovery phase, while manhours per unit of output stabilize or increase during the early months of a contraction. Finally, overtime is positively correlated with output changes, which tends to reinforce the movements in unit labor cost noted above.

(4) Rising profit margins are concentrated in the expansionary phase—mostly because unit costs fall early in the expansionary phase, and price in-

creases occur late in the expansion. Rising costs rather than falling prices explain why declining profit margins are bunched in the cyclical contraction.

Wesley C. Mitchell's original study, *Business Cycles,* in 1913, emphasized cost-price changes as particularly strategic in generating changes in profit prospects that are basic to business cycles. Hultgren, more than fifty years later, has a more modest purpose; he emphasizes the effect of sales and output on margins. The more restricted scope of Hultgren's inquiry was dictated by the realization that the analysis, based on the reference cycle, specific cycle concept, is inadequate for the development of a reasonably complete cycle model. The reference cycle method of data decomposition provides a valid and often valuable preliminary to analytical explanations. Using it, Hultgren has made a genuine contribution, restricted, however, by his rather strict adherence to the reference cycle concept. *Why* do unit labor costs behave as they do? *Why* do manhours per unit of output follow the pattern actually observed? A dozen specific cycles cannot answer those questions; the econometric studies cited above can in principle provide answers, even though open to numerous trenchant criticisms.

Two matters of research procedure raise questions. First, the main approach used is to establish rates of change during each of ten cycle stages of unequal length. Unless one really believes that these intervals have major analytical significance, which often they do not in my opinion, then the failure to present rates of change per uniform time interval detracts from understanding the process. Second, profits and profit margins are considered net of all costs including depreciation. Since the Treasury regulations have a major influence on permissible depreciation, it would have been advisable to treat out-of-pocket costs separately and thus isolate the influence of arbitrary legal changes.

The antithesis which I have drawn between explanation and description is much too sharp. Truth has not yet yielded to a single methodology in economics. Several flowers should grow; not the least among them should be hybrids which cross NBER thoughtful scepticism and thoughful treatment of the data, and econometric approaches with emphasis on theoretical and empirical explanation.

EDWIN KUH

Massachusetts Institute of Technology

REFERENCES

1. OTTO ECKSTEIN AND T. A. WILSON, "Short-Run Productivity Behavior in U.S. Manufacturing," *Rev. Econ. Stat.*, Feb. 1964, *46*, 41-54.
2. THOR HULTGREN, *Changes in Labor Costs During Cycles in Production and Business,* Nat. Bur. Econ. Research Ocassional Paper 74. New York 1960.
3. EDWIN KUH, "Cyclical and Secular Labor Productivity in U.S. Manufacturing," *Rev. Econ. Stat.*, Feb. 1965, *47*, 1-12.
4. RONALD SOLIGO, "The Short-Run Relationship Between Employment and Output," *Yale Econ. Essays,* Spring 1966, 160-215.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Introduction to the Analysis and Institutions of Money and Banking. By WILLIAM J. FRAZER, JR. and WILLIAM P. YOHE. Princeton: Van Nostrand, 1966. Pp. xi, 682. $10.75.

An undergraduate text in money and banking, this book is unusual both in the depth of its coverage and in the sophistication of its approach. The exposition proceeds via a succession of linear algebraic models and well-focused institutional interludes, culminating in lucid analyses of (1) the principles of economic policy (à la Tinbergen) and (2) complications raised by the existence of lags in monetary policy.

Structurally, Frazer and Yohe cover much the same topics as Chandler's Fourth Edition and in roughly the same order: money and interest; commercial banking and its history in the United States; central banking; macroeconomic theory; international finance; U.S. monetary policy in recent years. But, though the frame is the same, the authors' preoccupation with algebraic models and quantification produces a rather different sort of book.

The strongest chapters are those on U. S. financial institutions, international finance, and the theory of economic policy. These chapters are both well motivated and well conceived. The chapter on the derivation of the Member-Bank Reserve Equation and consolidated Money-Supply Accounts (Ch. 9) is particularly good and deserves a place on graduate reading lists everywhere.

On the other hand, the chapters on monetary and national-income theory are somewhat less successful. The difficulty is primarily one of vocabulary and focus. For an undergraduate textbook, there is to my mind excessive use of hard-to-explain professional jargon and excessive concern with subsidiary behavioral detail. This latter concern blurs the overall flow of the theory and gives the book an unnecessarily ponderous and unwieldy character. In particular, extensive chapters on investment and consumption theory develop a great many points that are not specifically employed in subsequent chapters, while the LM-IS model (which serves to integrate 150 pages of prior theory) is presented and dismissed in five closely written pages.

Some redress is provided by a series of excellent chapter summaries, but the book is by no means a self-teacher. Still, for those willing to take on the risks and extra work of supplementary assignments and guidance, Frazer and Yohe can promise a livelier and more relevant course.

EDWARD J. KANE

Boston College

Monetary Policy in a Developing Economy—A Study of the Policies of the Reserve Bank of India and Their Effect on the Operations of the Banking System, 1956-65. By C. K. JOHRI. Calcutta: The World Press Private Ltd., 1965. Pp. 283. Rs 20.

India's First Five Year Plan, which ended in March 1956, was modest in size and imposed little strain on the economy. In fact, price levels actually declined during the period and the nation's foreign exchange position remained

comfortable. The Second Five Year Plan was far more ambitious. Its goal was no less than that of raising real national output by 25 per cent and of creating eight to ten million new jobs. To this end, total planned outlays during the Plan period were to be 72 billion rupees, of which 48 billion were to be spent in the public sector and 24 billion in the private sector. The rate of net investment was to be raised from 7.3 per cent of national income at the beginning of the Plan to 10-11 per cent at the end. It was recognized from the beginning that some monetary expansion would be required to reach expenditure targets. Only 24 billion of the public sector outlays were to be met from the government's current revenues and foreign assistance and another 12 billion by borrowing from the public. This left about 12 billion to be covered through expansion by the Reserve Bank and the commercial banks. It also became clear that an expansion of bank credit would be required to meet expenditure targets in the private sectors.

Such was the environment in which the Reserve Bank operated during the five years following March 1956. The Bank was assigned the dual role of assuring that achievement of Plan objectives would not be hampered by a lack of money and of simultaneously preventing an excessive rise of prices. Moreover, it was expected to prevent any decrease of prices or increase of yields on government securities. The policy was called one of "controlled expansion" but "largely passive expansion" would be more apt. The outcome should surprise no one. The money supply rose 22 per cent and price levels about 30 per cent.

Johri describes in detail the policies followed by the Reserve Bank and traces out their effects on the scheduled banks, which hold about 90 per cent of total deposits in the organized banking sector. He does not attempt to analyze effects on rates of expenditures or on price behavior. Open market policies dominated all other instruments of general monetary management. To prevent their prices from falling, the Reserve Bank purchased passively all the government securities that others were not willing to hold at the selected pattern of prices and yields. Some actions were taken towards the end of the period to raise interest rates on credit to the private sector, but these were probably not very effective in slowing the rate of increase, partly because the actions were not very vigorous and partly because of the dominance of open market policy. Bank rate was raised only from 3 to 4 per cent. The imposition of borrowing quotas on banks and levying of penalty rates on borrowings in excess of quotas were of little effect, largely because banks could replenish their reserves by selling government securities. In March 1960 the Reserve Bank imposed marginal reserve requirements, obligating banks to hold increased reserves equal to 25 per cent of the increase of their liabilities after that date. In May these were raised to 50 per cent. These actions appeared to be slightly restrictive, but they were rescinded ten months later because they were restricting "the availability of credit for the legitimate needs of industry and trade."

Selective controls, embodying margin requirements and quantity limitations, were imposed on loans based on food grains, oil seeds, sugar, shares, and certain textiles and fibers. Their purpose was primarily to curb speculation in

these commodities and to limit increases in their prices. Johri concludes that they probably did influence the distribution of credit but that they did little to limit the total quantity of credit or price increases on the selected commodities.

In Appendix I Johri discusses briefly monetary policies during the first three years of the Third Five Year Plan, 1961-1964. During this period the Reserve Bank did allow some increase in interest rates. In general, however, it continued its policy of passive purchases of government securities. The Treasury bill rate actually declined while yields on consols were allowed to rise by about one-half of 1 per cent. These policies were accompanied by a 31 per cent further increase in the money supply, a percentage increase nearly equal to that during the preceding five years.

Johri's detailed study of Indian monetary policies during the Second Five Year Plan is a useful addition to the literature on the role of monetary policy in economic development. It does not even attempt to make new theoretical contributions, but it is a competent and judicious case study. It would be of wider interest if he had concentrated less on effects on the banking system and had ventured more into an analysis of the effects of these policies on the development program. Unlike some writers, he does not contend that inflation occurred because the central bank was powerless to prevent it. Rather, inflation was the inevitable consequence of the Reserve Bank's policy of providing almost passively the rising quantities of credit demanded by the government and the private sectors. On the effects of the degree of inflation actually experienced on the development effort he offers only tentative conclusions: "While nothing can be said with complete certainty, it is probably a safe conjecture that the disadvantages have outweighed the beneficial effects of inflation" (p. 34). On the positive side, inflation probably benefited farmers and business and thus improved incentives to produce and assume risks, at least in the short run. On the negative side, it may have contributed to the decline of the incremental ratio of saving to national income from 19 per cent in the three years preceding 1955-56 to 14 per cent in the following three years. It weakened India's competitive position in world markets and contributed to the stagnation of her exports. It encouraged gold smuggling and clandestine capital exports. And, by reducing real incomes of fixed income groups, it militated against welfare objectives of the Plan. Johri recognizes that his study does not provide the basis for firm answers to these questions.

LESTER V. CHANDLER

Princeton University

Money, Banking, and Economic Activity. By GEORGE G. SAUSE. Boston: D. C. Heath, 1966. Pp. xiv, 507. $8.75.

The basic theme of the book is how the use of money affects the economic welfare of human beings. The author develops this idea by examining the factors which determine the quantity and turnover of money and the methods available to the authorities to control these factors. He shows how the money system affects the economic well-being of the nation through its impact on the

level of GNP, degree of unemployment, rate of economic growth, and level of prices. He then explains how the authorities have attempted to influence the monetary system in order to achieve economic objectives. In a final section, he examines the role of money in international economic relations.

The author's discussion of commercial banking covers both the deposit mechanism and bank portfolio policy. The exposition of the deposit mechanism is lucid but marred by the use of standard terminology (external and internal drains) in a nonstandard way. It is confusing to read (pp. 92 and 95) that a cash withdrawal by domestic depositors (which most writers would term an internal drain)[1] is an external drain and that an internal drain is a cash withdrawal by other banks. The discussion of bank portfolio policy and the bank management problem (of reconciling profitability, solvency, and liquidity) is very brief. In keeping with his emphasis on monetary economics, the author is more interested in showing how bank operating policy affects the public welfare, but this part of the discussion is also brief and may leave some students with the impression that the allocation of resources is affected by bank lending but not by bank purchases of securities (cf. pp. 114-16).

The section on central banking covers the usual material on the structure and service functions of the Federal Reserve and on the instruments of monetary policy. There is a reference to the 1965 law that eliminated the gold certificate requirement for deposits in order to increase the supply of free gold. In spite of the change, deposit expansion continues to be indirectly constrained by the gold reserve requirement on notes for, as the author points out, the demand for Federal Reserve notes is related to the volume of demand deposits. It might also be noted that, given the continuing drain on the gold stock, the gold reserve requirements will ultimately have to be modified or dropped (either by Congress or by the Federal Reserve exercising its emergency powers). This is not only because a gold drain could hamper domestic monetary expansion but also because a continuing gold drain (without excess gold reserves) would put the Federal Reserve in the anomalous position of having to contract its Federal Reserve note liabilities without having any direct means for doing so.

In reviewing the principles that govern the United States monetary system, the author correctly stresses that "we rely upon a combination of competition, government regulation, and Federal Reserve influence to direct commercial bank activities in the public interest" (p. 190). He adheres to this view in alloting space for the discussion of the Federal Reserve and (to a lesser extent) of government regulation but not of competition. There is no discussion of the market structure of commercial banking, the nature of market rivalry in banking markets, or the competitive implications of the large number of mergers and entrants in recent years. One of the very few observations about bank competition—and it is incidental to a discussion of deposit mechanics—is that an American bank competes with approximately 13,750 other commercial banks (p. 94). This statement probably does no damage to the discussion

[1] For example, cf. Lester V. Chandler, *The Economics of Money and Banking*, rev. ed., New York 1953, p. 217, or W. H. Steiner, Eli Shapiro, and Ezra Solomon, *Money and Banking*, 4th ed., New York 1958, p. 161.

: the deposit mechanism, but it plants in the student's mind a very mislead-g idea (which the text does not elsewhere correct) about bank competition.

The monetary theory section begins with a clear presentation of the quanti-' theory in its Fisherian and Cambridge cash balance formulations, and then :oceeds to modern income theory, including national income accounting and ıe determinants of consumption, saving, and investment. Ira Scott's four-part agram is utilized to assemble the component parts of the Keynesian system ıd to show the relation between interest rate and income determination. The ıthor considers the similarities and differences in the quantity approach and .e income and expenditure approach to monetary theory and finds "the main gnificance of the two approaches in the different attitudes toward social and ›litical problems exhibited by their respective adherents" (p. 323).

While the author accepts the Gurley-Shaw position that commercial banks 'e not unique, he would not agree that they are as much like other financial stitutions as the critics of the conventional view have suggested. However, : does believe that other financial institutions ought to be considered in a udy of monetary economics and he has included a section to discuss their le, types, and relation to monetary theory and policy. He covers various ›vernment credit agencies as well as the major private nonbank financial in-itutions, and (without much elaboration) attributes the government's inter-:ntion in the financial market to "a feeling that without some form of gov-nment guidance the operation of financial institutions leads to an unsatisfac-ry allocation of resources" (p. 355). The discussion of the Gurley-Shaw ew of financial institutions is useful as far as it goes, but it is brief and per-ıps too cryptic on some points. For example, it would have been desirable if e author had elaborated on the statement, not otherwise explained, that ›pen-market operations do affect these [other than commercial bank] insti-tions" (p. 377).

This is a money and banking text in the traditional mold and, all in all, an ceptable one. Although there are some lapses in the exposition, the author ıs generally written clearly and with a readable style, and the integration of e material into an organic whole will help the student grasp the big picture well as the detail. Moreover, in spite of the topics (including some newer :velopments) that have been omitted or treated too briefly, the coverage is :nerally adequate, especially for a one-semester course.

DAVID A. ALHADEFF

niversity of California, Berkeley

:terest Rates and the Cost of Capital in Israel, 1950-1962. By HAIM BEN-SHAHAR. Tubingen: J. C. B. Mohr (Paul Siebeck), 1965. Pp. 136. DM 22.50; paper, DM 18.50.

In its contribution to the stock of a nation's basic economic data, this study ıblished by the List Institute, parallels the achievements of some of the onographs produced in the last generation at the National Bureau of Eco-›mic Research. It shows meticulous development of a wealth of commercial ınk and government source material which few investigators can achieve. part from the information it provides about Israel, the book will be of value

as a guide for those developing series for effective interest rates in other countries where problems of tieing of loans to the foreign exchange rate or the cost of living, changes in the terms of loans already outstanding, hidden charges on bank loans, etc., are met.

The author resolves the problem of the significance of interest rate series covering periods of markedly different rates of price increase through (in most cases) the working assumption of perfect foresight; this permits him to deflate each year's effective interest rate by the rise in the price index for investment goods during the year. (The assumption is not so drastic as it may sound, for much of the additional interest burden which raised the effective interest rate above the nominal rate was due to linking the value of the portions of loans still outstanding to the cost-of-living index or the price of the dollar, or both.) The assumption of perfect foresight had to be supplemented by forecasts in estimating the effective interest rates on the portions of loans that were to be still outstanding after the end of the period of observation (1962). While these forecasts (4 per cent annual price increases and a 50 per cent rise in the price of the dollar in 1966) have proved inaccurate, they may have been good estimates of what the parties actually foresaw at the time the loans were contracted; if so, the forecasts led to good estimates of the effective rates—and even better estimates of the real rates—which actually determined the behavior of the market.

In addition to the obvious series, the interest rate series derived include: the cost of the government's borrowing from abroad (the most recent real rate about 12 per cent); the real rates paid by several private sectors for government loans, the chief source of finance for private fixed investment (recently 1½ per cent to 8½ per cent); those for short-term borrowings on the large black (later grey) market for commercial paper (a maximum of 15 per cent but recently 9 per cent). The private sector's required rate of return on capital was found to be best represented by the 17 per cent net-of-tax real annual rate of profit realized on listed stocks held for several years. As in other developing countries, usury laws have kept the real rates charged by banks at extremely low levels—often negative and never over 8 per cent—so that rationing had to be used to equate demand with supply in most years.

While most of the book is devoted to the derivation and description of the various time series, as well as some of the institutional background, the final series of real interest rates are employed to test the Israeli government's policy of subsidizing private investment by re-lending its high-cost foreign borrowings to industry and agriculture at the low interest rates described above. It is argued that the economy should invest enough each year so that the marginal rate of return on investment is no higher than the country's real cost of capital (the 12 per cent on the foreign borrowings described earlier). Offering the government's funds to entrepreneurs at that rate would be unjustified, however, because it would depress the rate of return receivable by private savers —the yield on stocks in particular—and thereby reduce private domestic saving. The alternative of inducing the entrepreneurs to invest in projects that had yields below what the private sector required (through offering them part

of the funds needed for such projects at very low, subsidized interest rates) would avoid this deterrent to domestic savings because it would permit the private investor to continue to earn his customary high yields; in fact, this alternative would increase the rate of private saving, for it implies increased demand for private savings and hence an increasing rate of return on them. While the data were too crude and incomplete to permit a full test of this model, it was found that the low interest rates charged on government loans financing portions of the subsidized investment projects would have permitted the entrepreneur to earn roughly his required 17 per cent rate of return on the *non*government portion of the funds if the project as a whole was yielding the desired minimum rate of 12 per cent. The existence of a rational basis for the Israeli government's investment subsidy program thus tended to be confirmed.

A number of defects justify doubt that this test was usable even in the loose form in which it was applied. The marginal cost of capital to the economy is represented by the 12 per cent average interest cost on the chief foreign loan issues (issues offered continuously to the public in the United States); but these issues' marginal costs seem clearly much above the average cost, since the very high cost of Israel's marketing efforts apparently has added three percentage points to the nominal 5 per cent interest rate paid. This understatement of the cost of capital to the economy may have been compensated, however, by an overstatement of the required rate of return on that capital: the associated additional returns to labor and land (and the tax collector) were, in general, excluded from consideration when the minimum acceptable return on investment (12 per cent) was specified. The admittedly shaky estimate of the required rate of return on private investment (17 per cent) may remain unacceptable despite the device of averaging over several years, for the perverse short-run effects cited—rises in required rate of return (or declining profits) cut stock prices and thereby lead to a *reduced* ex post figure for the required return—can still be present if they appear at the end of the period, and their occurrence gradually over the course of the period in a country undergoing rapid economic and financial development cannot be assumed away. Finally, the real rate of return on the equity funds invested in subsidized projects must have been substantially above the figures estimated because, first (contrary to the author's assumption) the nongovernment funds included some nonequity funds costing much less than the equity portion's required 17 per cent; and, second, many of the subsidized projects must have yielded more than the assumed 12 per cent.

William H. White

International Monetary Fund

The Liquidity Structure of Firms and Monetary Economics. By William J. Frazer, Jr. Gainesville, Florida: University of Florida Press, 1965. Pp. 91. $2.00.

Professor Frazer's monograph is another addition to the stock of empirical investigations of the demand for money and near moneys, a stock which of late seems to grow at a much faster rate than the relevant population of mone-

tary economists. The study presents results of a cross-section analysis of firms in the manufacturing sector, with data obtained from the FTC-SEC *Quarterly Financial Report for Manufacturing Corporations.*

His major finding is that cash as a percentage of assets declines as firms increase in size, which he interprets as "empirical support for the proposition that the precautionary demand [for money] rises less than proportionally with wealth (assets)" (p. 86), and, since he finds assets and income (sales) almost perfectly correlated, this further suggests that "ratio of sales to money balances must be rising, at least over some major portion of the domain over which asset sizes actually vary" (p. 86). The decline in the money-sales or money-assets ratio as scale increases coincides with a *rise* in the index of "corporate liquidity" (cash plus near moneys divided by current liabilities) as income or assets increase; as assets (sales) rise, firms do choose greater liquidity, but rely more on "near moneys" (including a reduction of a "negative" near money—bank loans) to meet liquidity needs (at least beyond some minimum size). Frazer's results, then, are generally consistent wtih the Baumol-Tobin hypothesis of economies of scale in the demand for money, and in general inconsistent with the findings of Meltzer (unit elasticity with respect to income or sales) and Friedman (substantial diseconomies of scale with respect to the demand for money).

Frazer does not explore the basis for these differences between the results of his study and those of Meltzer and Friedman. Indeed, he does not explicitly use the data to test a demand function for money by manufacturing firms but rests content with regressions using the ratio of money to assets as the dependent variable and assets as the independent variable, which yield him negative slope coefficients for quarterly cross-sections covering the period 1958-63, on which he bases the major finding noted.

Several comments about his results may be made. First, Frazer's interpretation of his findings is open to question. He refers throughout to the implications of his results for the "precautionary demand" for money by business firms. However, there is no attempt to separate business holdings of money balances into motivational components, if indeed that would be possible, and we are left to wonder how we can be sure that the economies of scale reflect economies in precautionary balances, rather than in transactions or speculative balances. One could argue, as has been done recently, that in an environment where near moneys dominate money, the only demand for money is for transactions purposes; all precautionary and speculative balances will be held in time deposits, bills, or other near moneys. If this is the case, then Frazer's results are relevant to the transactions demand, not the precautionary demand. Second, too little attention is given to definitional questions. Frazer apparently uses the FTC-SEC definition of "cash" throughout, but nowhere that I could find is the reader told whether this includes or excludes business holdings of bank time deposits or savings shares, a point of some importance in comparing income or wealth elasticities of the demand for money (particularly since his data run through 1963, well into the era of importance of certificates of deposit). Third, there is no analysis of shifts in the estimating parameters of the equations for the cash-assets ratio (or the parameters of the

implicit money demand equation) over time. Intercept values are not given at all, and there is some evidence of changes in the slope parameters from quarter to quarter (p. 21). This might have provided useful information on the significance of interest rates and/or business cycles on the business demand for money. Finally, it is most disappointing to find no attempt by Frazer to explain the difference in his findings from those of Meltzer and Friedman-Schwartz (or their critics). Pay your money and take your choice—economies, diseconomies, or proportionality.

Frazer discusses more than the demand for money in the volume. He relates his findings about the financial structure of firms by asset size to the commercial loan doctrine, the effects of trade credit on monetary policy, bank loans as a source of funds for inventories, and the role of business holdings of governments in tax payments and, as a liquidity source. The discussion of these subjects is informative and interesting.

DAVID J. OTT

Southern Methodist University

Commercial Banking and Regional Development in the United States, 1950-1960. By GEORGE MACESICH. Florida State University Studies, No. 45. Tallahassee: Florida State University, 1965. Pp. xvii, 160. $5.50.

In this small book, Professor Macesich attempts to tie together some hypotheses regarding Southern economic development and the banking structure of the South. The book is a curious mélange of development theory, assorted thoughts on the competitive structure of banking, and some statistical observations which combined do not substantiate Macesich's conclusions. The book's fatal weakness is that the author's objectives are obscure. The reader's confusion is not abated by a generally pedantic style and some grammatical weaknesses.

The five introductory chapters develop the hypotheses to be tested. It is Macesich's concept that generalized propositions explaining the existence of backward regions (in this case, the South) can be measured through statistical analysis of the performance of the Southern banking system. Chapter 2 is almost in the nature of an aside dealing with the monetary development of the United States; the chapter argues that past errors by various monetary authorities stretching back to before Jackson's war with the Second Bank have taken a peculiar toll of the Southern region. Chapter 3 deals with the background of Federal Reserve regionalism and includes a two-page discussion covering the fact that member banks do not own their District Banks—this insert like many others does not relate to the hypotheses, the statistical observations, nor the conclusions. The author's similar insertions, *a priori* comments, *obiter dicta*, etc., are the basis for the reviewer's comment that the author's purpose is not always clear.

Banking as a business is taken up in Chapter 4. Chapter 5, the best in the book, discusses the legal and structural peculiarities of Southern banking. Macesich deals heavily with the phenomenon of non-par banking and nonmembership in the Fed, and correctly demonstrates that both are symptoms of backwardness. But, since over 80 per cent of total deposits in the Sixth

District are in member banks, the total economic impact of these marginal banks is probably not too serious. (In 1966 the Georgia Legislature finally required insurance of the state's fifty, small, nonchartered banks.)

The book's central section deals with various statistical tests to determine whether economic backwardness of an area shows up in banking performance. Chapter 7 demonstrates that member banks located on the periphery of Southern economic centers do not have significantly different earning ratios from those institutions located in metropolitan centers. Chapter 8 demonstrates that Southern banks are neither underloaned, overinvested in government securities, nor nonprogressive in their lending attitudes. Chapter 9, using analysis of variance techniques, demonstrates that Southern member banks as compared to other districts have a higher rate of return on loans, similar return on U.S. securities (why should they be different?), and that there are local differences in loan markets. Using T. W. Schultz's hypothesis that industrial areas have a higher degree of economic efficiency than rural, less-developed economies, Macesich posits that this would be shown if Southern banks had higher returns than banks in other areas. This is demonstrated in Chapter 10, although the various bank correlation statistical tests have mixed significance level results. He finds that Southern member banks hold more cash and more real estate mortgages and less government securities than non-Southern member banks. In addition, banks in less developed areas earn proportionally more from their loans as compared to total income than do other Federal Reserve District member banks.

Chapter 12 contains a lead sentence so typical of Macesich's style that it is worth quoting: "The evidence and analyses summarized in this chapter suggest that the principal source of member bank earnings is derived from loans" (p. 140). Chapter 13, the last analytical one, shows that member banks in the less developed areas tend to have a lower capital coverage of the higher risk loans than those in the more highly developed areas.

The conclusion reached by Macesich is that monetary and banking forces have helped to create a situation "favorable to economic stagnation." Furthermore, the higher proportion of cash and real estate loans "simply immobilizes needed resources for local development" (p. 156). Macesich holds that many of the views advanced by Schultz, E. Dunn, C. Clark, S. L. McDonald, and B. Ratchford are useful in understanding certain Southern financial phenomena, but that W. H. Nicholls' concept of "Southern Tradition" as being the basis of stagnation is not supported by the evidence.

Because Macesich holds that the legal environment of banking in general tends to encourage stagnation, he ends the book with a few comments on Mr. Saxon, Mr. Patman, and the Fed. While Macesich's remarks do not appear to be germane to the issue, he apparently does support Patman's desire to reform the Federal Reserve System.

To argue that an efficient banking system aids economic development and lowers the risk premium in the saving-lending process hardly seems an important contribution to monetary economics. That interest rate differentials and the resulting incomes of lenders are higher in one area than another is as natural as the economic fact that transportation costs cause geographical

differentials in the prices of goods. All in all, Macesich has not added significantly to the total of knowledge concerning the role of financial institutions in economic development.

ROBERT R. DINCE

University of Georgia

Consumer Credit Finance Charges. By WALLACE P. MORS. NBER Studies in Consumer Instalment Finance No. 12. New York: Columbia University Press for the National Bureau of Economic Research, 1965. Pp. xvii, 133. $5.00.

Consumer Instalment Credit and Public Policy. By PAUL W. MCCRACKEN, JAMES C. T. MAO, and CEDRIC V. FRICKE. Michigan Business Studies Vol. XVII, No. 1. Ann Arbor: Bureau of Business Research, University of Michigan, 1965. Pp. x, 250. $6.00.

Consumer credit continues to be the subject of lively political and economic controversy. Despite some limitations and faults, these two works are useful contributions to the literature on the subject.

Wallace P. Mors' monograph is part of a National Bureau study of consumer credit that has produced several earlier works and promises more. It describes the methods U.S. financing agencies and sellers utilize in computing and quoting charges to consumers, explores the uses of the various types of information available, considers problems encountered in converting finance charge information to any given form, and reviews evidence regarding consumer knowledge of these charges, both as dollar amounts and as effective rates.

The variety of methods used to compute and quote finance charges can be credited to the usury laws that set maximum interest rates; to legislative sanction of specific procedures when legislatures have exempted some activities from such laws (instalment cash lending) and applied them to other activities not originally subject to the laws (retail instalment financing); to man's ingenuity in striving to lower calculation and other costs; and, perhaps, to the desire to sugarcoat the price of this credit. The three general methods of computing charges include the "add-on method" (the amount owed by the borrower is the amount received plus the finance charge), the "discount method" (the amount received by the borrower is the amount owed less the finance charge), and the "per cent per month of the outstanding balance method" (a procedure that is clear from the name). There are many variants in use such as "monthly add-on," "annual add-on," "monthly add-on plus," etc., though annual add-on, discount, discount plus, and per cent per month are dominant.

Almost all states add to the variety of practice and the problem of assessing actual rates by imposing graduated ceilings as part of small loan laws. Massachusetts, for example, imposes graduated ceilings of 2½ per cent, 2 per cent, 1¾ per cent, and ¾ per cent per month respectively on the parts of the loan under $200, from $200 to $600, from $600 to $1000, and from $1000 to $3000.

Mors presents this picture plus much related information and considers

problems in effecting a desirable standardization. Appendix B provides a table for converting various add-on and discount rates to alternative types of effective rates. The book contains an almost indispensable glossary and a helpful index.

A few qualifications to a generally favorable recommendation are called for. The organization and exposition require that the reader pay careful attention to terms and cross-check some material discussed partly in one section and partly in another. Some of the analysis and discussion is vaguely disquieting. For example, the consumer cannot and does not borrow, say, the full cost of a durable with repayment over the lifetime of the durable with no effect on the rate. How do changes in the various dimensions of the loans available affect his behavior? Investment theory has been moving away recently from the assumption of a perfect market and identical borrowing and investing rates, and has always contained *ad hoc* treatments of the more various market phenomena. Mors' discussion, which builds on an earlier work by Juster and Shay, is not actually unsatisfactory, but the statement (p. 47) that "the traditional approach" assumes that a change in interest rates influences demand if and only if it affects monthly payments is true only for a limited, highly tentative literature. The author provides no explanation, only references to other sources, of some important concepts—for example, "constant ratio formula," etc. (p. 62)—which will be familiar to those acquainted with the finance rate literature but not to many readers, including some who have given considerable attention to other aspects of consumer credit. And some who turn to the book, with the "Truth-in-Lending" Bill before Congress in mind, will regret the absence of any policy recommendations.

Policy recommendations, though not on the matter of finance charge information, do appear in the work by Paul W. McCracken, James C. T. Mao, and Cedric V. Fricke which provides the authors' answer to the question: "Should there be direct federal control of the terms on which instalment credit is made available to consumers?" An appendix presents a sixty-page case study by Thomas R. Dyckman of the influence instalment credit had on the 1955 automobile sales year in this country.

After a brief introduction and overview of the instalment credit industry, the authors consider in order: "Do movements in consumer instalment borrowing make the economy significantly more unstable?" "Is direct regulation of instalment borrowing essential for the effective operation of general monetary and credit policy?" "Would government regulation of instalment borrowing enhance the welfare of consumers themselves by curbing their propensity to buy on time?" "Do the lessons from our experience of controls suggest a need for their continuation?" "What are the prospects for the further growth of instalment credit?" Their examination of these questions leads them to the conclusion that peacetime regulation is not desirable.

The book presents estimates of instalment debt outstanding in 1970, information on repossession ratios by length of employment, etc., and much else. A large part of the data was gathered from a variety of other publications including, of course, the lengthy Federal Reserve study in the late 1950's,

though additional material required for a systematic survey of the many aspects of the subject was acquired from "a large sales finance company," individual banks, etc.

The writing style is straightforward but not very lively, the index skimpy. One cannot say that the book offers any startlingly new data or analysis, and it is unlikely to change many economists' views on the desirability of consumer credit controls in peacetime, views which tend to reflect differing propensities to interfere with private markets and differing assessments of the problem of enforcing controls. But the work can certainly serve usefully as a systematic account to which one can refer students, public policy study groups, and all others interested in the subject.

H. LAURENCE MILLER, JR.

Carleton College

Public Finance; Fiscal Policy

Public Economics. By LEIF JOHANSEN. Amsterdam: North-Holland Publishing Co.; Chicago: Rand McNally and Company, 1965. Pp. ix, 367.

Once in a decade one can hope to find a text that is intelligible to the undergraduate and an education to their professors. Johansen's *Public Economics* not only meets that prescription, it also stands as a complement to established classics in the field such as R. A. Musgrave's *The Theory of Public Finance*, H. Theil's *Optimal Decision Rules for Government and Industry*, and Bent Hansen's *The Theory of Fiscal Policy*. *Public Economics* explicates the field of public finance for the serious student at a level that demands a limited knowledge of calculus; unnecessary notation and needless complexity are skillfully avoided. Explanations are concise, appropriately qualified, and illustrated by a limited number of examples, primarily from the experience of Norway and other European countries.

Johansen has collected and simplified the presentation of a number of problems in public finance which previously could only be derived from extremely scattered and advanced argument in the literature. Thus his treatment of demand for public goods and resulting conflicts over the apportionment of costs gives insight into the pioneering studies of Lindahl, Samuelson, and Arrow at a level that is easily mastered by an undergraduate.[1] His careful explication of fiscal policy in an economy with changing prices cuts through the ambiguous controvery over money illusion that represents the last major exchange on this subject in the literature.[2] His treatment of the computation of incidence is far clearer than its predecessors: he makes clear in a simple presentation that distributional effects of taxation can only be studied within the context of a

[1] See E. Lindahl, "Just Taxation—A Positive Solution" in R. A. Musgrave and A. T. Peacock, ed., *Classics in the Theory of Public Finance*, New York 1958; P. A. Samuelson "The Pure Theory of Public Expenditures," *Rev. Econ. Stat.*, Nov. 1954, *36*, 387-89; other articles by P. A. Samuelson in *Rev. Econ. Stat.*, 1958, *40*, 332-38 and 1955, *37*, 350-56; and K. Arrow, *Social Choice and Individual Values*, New York 1951.

[2] See R. A. Musgrave, *The Theory of Public Finance*, New York 1958, 447-56 and A. Morag, *On Taxes and Inflation*, New York 1965, Ch. 3.

general equilibrium model of the economy. Assumptions that are required to provide an approximate incidence computation are carefully discussed and the author indicates a sophisticated awareness of their limitations.

Public Economics represents the first intermediate textbook in public finance that attempts the level of sophistication that characterizes current intermediate price and national income theory texts. Both the material presented and the method of presentation make it a text that outranks its competitors. The book's quality makes it impossible to be genuinely critical. Instead the book invites suggestions for the development of additional pedagogical material in public finance, and the remainder of this review is devoted to such suggestions.

In the first three chapters of *Public Economics* Johansen discusses problems of stabilization policy with a simple and well manipulated national income model. Chapters 4 and 5, which deal with the public debt and the functions of public budgeting, are the weakest section of the book. Neither the monetary implications of debt nor the recent controversy over the Buchanan view of welfare consequences of debt finance are discussed.[3] The discussion of public budgeting entirely neglects the use of the budget as a vehicle for allocating resources within the public sector. Program budgeting, cost effectiveness, and cost benefit analysis are not mentioned. Functional areas of public expenditure and the problems associated with expenditure in particular areas—such as defense, education, and welfare—are superficially treated.

Chapter 6 is an excellent presentation of classic welfare arguments for government intervention in the allocation of resources. The only weakness of this discussion lies in its failure to discuss the paradox of government intervention in decreasing-cost industries—that is, subsidies may provide optimal current use of a facility once it has been built, but operation with a subsidy makes it impossible to use the price system to determine whether demand is sufficient to warrant construction of the facility in the first place; and subsidy creates difficult problems in determining distribution of the tax burden when some members of the population are non-users or when benefits of the public enterprise are local in character.[4]

Chapter 7 is a reasonably comprehensive discussion of taxation. It begins with a quick survey of alternative tax bases, and continues with a detailed discussion of income taxation and consumption taxes. The discussion of the problems of income-splitting, exemptions, capital gains, and depreciation is outstanding. The mathematical treatment of consumption taxes makes the price effects of turnover, value-added, and retail sales taxation clear and intelligible to any reader. Two criticisms can be made of Johansen's subsequent treatment of the effects of taxation First, and most serious, he neglects the effects of changes in the allocation of effort between work and leisure in his discussion

[3] See J. M. Buchanan, *Public Principles of the Public Debt*, Homewood, 1958; W. G. Bowen, R. G. Davis and D. H. Kopf, "The Public Debt: A Burden on Future Generations?" *Am. Econ. Rev.*, Sept. 1960, *50*, 701-6; and P. A. Diamond, "National Debt in a Neoclassical Growth Model," *Am. Econ. Rev.*, Dec. 1965, *55*, 1126-50.

[4] See A. M. Henderson, "The Pricing of Public Utility Undertakings," *Manchester School*, Sept. 1947, *15*, 223-50.

of incidence.[5] Second, Johansen tends to adopt the misleading partial equilibrium usage of the term tax-shifting rather than the more appropriate shifting concept that is tied to a general equilibrium analysis of tax burden.[6]

Chapter 8 is an interesting discussion of the problems of federal finance, based largely on Norwegian experience. Unfortunately, models of the choice of optimum scale for government functions are not developed.[7] Nor are grants-in-aid given the treatment they deserve in a book of this level of sophistication.[8]

Largely because the book is so well organized and clearly written, one wishes that Johansen had set his hand to the discussion of several additional topics. A natural subject that would fit within his general framework is the effect of government fiscal and debt policy on investment and economic growth. Additional material related to current questions of tax harmonization would also fit neatly into Johansen's treatment both at the macro level, where a good discussion of foreign trade and stabilization is already developed, and at the micro level, where Johansen aptly develops a partial equilibrium analysis of customs duties. Additional material contrasting price regulation through tax-subsidy arrangements and direct controls would be a useful adjunct to Chapter 6. One would like to see some mention of the psychological impact of taxation in addition to the more traditional models of the incentive impact of taxation.[9]

One cannot forgive the omission of a bibliography from a book of this caliber. American users of the text will find it necessary to supplement the text with a volume of illustrative material concerning the public sector in the United States, such as is now available in several easily accessible sources.[10]

MARTIN DAVID

University of Wisconsin

[5] Discussed in detail in Musgrave, *The Theory of Public Finance, op. cit.*, 217-23. Subsequent analysis of survey data by J. N. Morgan, R. Barlow, and H. Brazer suggests that this effect will not be extremely important (*Economic Behavior of the Affluent*, Washington 1966).

[6] See Musgrave, *op. cit.*, pp. 230-31.

[7] See, for example, Charles Tiebout, "An Economic Theory of Fiscal Decentralization" in the National Bureau conference volume *Public Finances*.

[8] See R. A. Musgrave's excellent mathematical presentation, "A Multi-Level Theory of Public Expenditures," *Proceedings of the National Tax Association*, 1959, *52*, 266-78.

[9] See Günther Schmolders, *Das Irrationale in der Offenthchen Finanzwirtschaft*, Hamburg 1960.

[10] Especially the Brookings Institution Studies in Government Finance and F. C. Mosher and O. F. Polzand, *The Costs of American Government*, New York 1964.

Economics of the Property Tax. By DICK NETZER. Washington: Brookings Institution, 1966. Pp. xviii, 326. $6.75.

Professor Netzer attempts in this, the first major book on property taxation in the United States since Jensen's 1931 classic, to use "the vastly enriched supply of economic data available in recent years" to determine the impact and incidence of the property tax in the aggregate, the significance of the benefit components of the tax, and the effects the tax and geographic differentials in taxation have upon investment, output, industrial location, and

urban development (p. 16). Explicitly excluded from analysis are the theory of tax incidence and capitalization and the history and administration of the tax. "This volume," as Netzer notes, "is *not* a latter-day version of Jensen" (p. ix). Only a brief description of the book's contents and its main strengths and weaknesses is given in this review.

Netzer's analysis is perhaps at its best in the discussion of the economic effects of non-neutral property taxation. In Chapter II he uses a special Census Bureau compilation to estimate the initial impact of property taxes. His estimates reveal that in the aggregate these taxes discriminate both between sectors of the economy and within any given sector. Moreover, there are substantial geographic differentials in property taxes.

In a provocative theoretical discussion of conditions in various industries, Netzer concludes that inter-regional differences in property taxes probably have little effect on industrial location, but that intraregional differences, especially within large metropolitan areas, may seriously distort patterns of economic activity. Similarly, in Chapter IV he argues that differential taxation of different types of property within cities may affect future urban development, particularly urban renewal.

In Chapter III the incidence of the property tax by income groups is estimated using assumptions about each of its component's incidence among producer and consumer groups. The tax is found to be "markedly regressive" in the lowest income levels, but only mildly regressive or proportional above that. However, the benefits financed by the tax are estimated to be "distinctively regressive" throughout. Thus Netzer concludes that on balance the property tax in the aggregate contributes significantly to income equalization, although substantial horizontal inequities exist.

In his general appraisal of property taxation in Chapter VII Netzer argues that "the land component aside, general taxes on consumption or income and a general value added tax seem clearly preferable alternatives on economic grounds to both the housing and business property taxes." He therefore views the relatively high (about unitary) income elasticity of the economic base of the property tax as somewhat of a mixed blessing, since it portends continued heavy reliance on the tax by local governments.

In the eighth and final chapter Netzer discusses four alternative forms of property taxation, but finds none entirely satisfactory. He rejects a tax on annual rather than capital values as distorting resource allocation and being administratively impractical for the United States. Relatively heavier reliance on taxation of site values would lessen misallocation of resources, but would create serious inequities. Finally, both the taxation of incremental land values and "a family of user charges," while attractive on efficiency grounds, would allow only slightly reduced use of the presently defective property tax.

One of this reviewer's chief complaints about this book is that, although the impact and incidence of property taxes in the nation as a whole is largely irrelevant for the evaluation of the property taxes in any one state, geographic differentials in taxation and their economic implications are not discussed until Chapter V. And even in that chapter some of the most important interstate differences in property taxation are left buried in a category which "is a

tchall but very largely consists of non-farm business holdings" (p. 96), ough Chapter VI sheds some light on the personality components of this category. Estimating the sectoral impact and the incidence of each state's proper- taxes is admittedly a Herculean task. Yet this study's aspirations might ld one to expect such estimates.

Further, Netzer has not enjoyed equal success in answering the questions he t for himself. Subject to the above qualifications, his analysis of the impact d incidence of the property tax is informative and his discussion of geoaphic differentials in taxation and their effects quite provocative. But his aluations of the benefit components and the investment and output effects of operty taxes are less satisfactory.

For example, Netzer states rather categorically that the property tax iances benefits primarily for individuals and that there are no significant nefits associated with most of its business components. He does not consider e benefits a firm receives from expenditures providing services to its employs. This omission reflects the inadequacy of the theoretical literature in this ea. Since this conceptual question has never been answered, the assessment the benefit component of business taxes financing such expenditures is omed to less than complete success.

In his discussion of the investment and output effects of property taxes, etzer theorizes about tax-induced substitutions of labor for capital, of iman for physical capital, of nondurable for durable consumer goods and using, and between goods. But these hypotheses are not subjected to gnificant empirical tests. Thus it can hardly be agreed that Netzer has deterined the effects of the tax upon investment and output, though many of the ta he has developed would be useful in such a determination.

This book is valuable primarily because Netzer has brought together and nthesized a wealth of material, largely empirical, on property taxation. In so ing he has increased our understanding of property taxes and has facilitated ture research on this important tax. One can only regret that Netzer did not rect his efforts more toward theoretical analysis. Quite often one finds hints novel theoretical insights which unfortunately usually remain implicit.

CHARLES E. MCLURE, JR.

Rice University

International Economics

easuring Transactions Between World Areas. By HERBERT B. WOOLLEY. New York: National Bureau of Economic Research; Columbia Univ. Press, distrib. 1966. Pp. xviii, 157. $7.50.

Progress in the analysis of world trade and payments is severely hampered the lack of geographically disaggregated trade and payments data. Our owledge of trade flows among major world regions was considerably adnced by the publication, in 1942, of Hilgert's pioneering League of Nations idy, *The Network of World Trade*. Since about 1950, merchandise trade ta by country of origin and destination have been regularly published by e statistical offices of international organizations. However, corresponding

information for service payments and capital flows has not been available. The publication of Herbert B. Woolley's book, the third in the series Studies in International Economic Relations issued by the National Bureau of Economic Research, consitutes a further step forward. In addition to refinements in the world merchandise trade matrix, Woolley presents for the first time regional matrices of service transactions, unilateral transfers, and capital movements.

The geographic classification distinguishes eight major world regions: United States, Canada, United Kingdom, Rest of Sterling Area, Continental OEEC countries, Continental Overseas Territories, Latin America, and Other countries; a supplementary category, International Institutions, is added. (From the viewpoint of today's requirements, one would have preferred it if Japan were shown as a separate analytical region.) Transactions matrices are prepared for eleven classes of balance-of-payments items: merchandise valued f.o.b., five types of service payments, private and official unilateral transfers, capital, gold, and the residual (net multilateral settlements and error). Merchandise and service transactions are shown gross and net; the remaining categories are given on a net basis. The data are annual for the period 1950-54. These transactions matrices were distilled from a regional elaboration of balance-of-payments statements for 78 countries, some of which could be taken more or less directly from the *Balance of Payments Yearbook* of the International Monetary Fund, while others had to be modified or newly constructed. Merchandise import data, often available only on a c.i.f. basis, were re-estimated on a f.o.b. basis. Special studies were undertaken of world petroleum trade and transportation payments involving the fleets flying the flags of Panama, Honduras, and Liberia ("flags of convenience").

The goods and services matrices are "two-valued," i.e., two entries are made in each cell, one referring to the transactions as reported by paying countries and the other to the same transactions as reported by receiving countries. This dual approach afforded an essential check in the preparation of the material and suggested a number of problems, which could then be studied more intensively. While the remaining discrepancies are in some instances still large, on the whole the pairs of corresponding records compiled from statements of different reporting countries agree surprisingly well.

Of the three objectives with which Woolley embarked on this project, (1) presentation of the statistical record, (2) description of trade and payments flows in the period covered, and (3) analysis of transactions and testing of hypotheses, the present volume confines itself chiefly to the first, and deals only to a minor extent with the second. The methodological problems encountered in compiling the record and, in particular, the sources of discrepancy between the corresponding entries in the two-valued matrices account for the bulk of the text of this volume. The choice of emphasis was probably wise; the period 1950-54 is now chiefly of historical interest, and a detailed description and analysis of the transactions of that period would not hold the attention of readers chiefly interested in current trade and payments problems. It is to be hoped, however, that this volume will generate sufficient interest in an extension of the record to stimulate Mr. Woolley or other research workers

into continuing this project. It would be particularly valuable for current work on world trade in goods and sevices to have the respective transactions matrices brought up to date, so as to furnish a sufficient number of observations for statistical time series analysis. Separate records of unilateral transfers, capital movements, gold transactions, and multilateral settlements and error must undoubtedly remain somewhat less reliable than those for goods and services, even with as painstaking an effort as that underlying this volume. Nevertheless, a regional transactions matrix of capital movements, brought up to date, would be a most valuable tool in connection with current discussions of balance-of-payments disequilibria and the adjustment process. All those interested in these questions owe a debt of gratitude to Mr. Woolley and to the National Bureau of Economic Research for having initiated this project.

RUDOLPH R. RHOMBERG

International Monetary Fund

Exchange-Rate Devaluation in a Semi-Industrialized Country: The Experience of Argentina, 1955-61. By CARLOS F. DIAZ ALEJANDRO. Cambridge, Mass.: The M.I.T. Press, 1965. Pp. xiv, 210. $7.50.

Professor Díaz has tackled a difficult and frustrating task. He has attempted to trace the impact of currency depreciation on a country which, except for brief intervals, has been plagued by acute inflation for a generation or more. While concentrating on the period 1955-61, he has devoted most of his attention to the effects of the unsuccessful stabilization measures announced by President Frondizi in December 1958. These included the complete removal of quantitative restrictions on imports and the introduction of a single exchange rate which was to be permitted to fluctuate without any government commitment to a pegged value.

Under this program, the Argentine peso immediately depreciated sharply, the balance of payments greatly improved, the price level almost flattened out (albeit at a much higher level) and, for a time, all appeared to be going well. But by 1961, the end of the period covered by Díaz, the gains achieved in 1959 and 1960 rapidly began to melt away. The wholesale price level, which had been virtually stationary in 1960, zoomed upward; the balance of payments, which in 1959 and 1960 had achieved a strong surplus position, relapsed into deficit; and the exchange rate, which had been steady since the second quarter of 1959, began a new decline.

Díaz has made a valiant effort to distill what can be learned from this discouraging experience. His first two chapters are a statement, with new ideas of his own, of the theory of devaluation; the next three describe the main characteristics of the Argentine economy that are relevant to his analysis; and the two final chapters review in detail the recent Argentine experience with currency depreciation in an effort to test the theory developed earlier in the volume.

Díaz's exposition of devaluation theory reflects wide acquaintance with the literature and much penetrating thought. His observations about price elastic-

ities, income effects, and the "absorption" approach should impel many a reader to review his own position on these matters. On numerous points, I find myself in hearty agreement. To save space, I shall not dwell on these, but instead shall concentrate on what Díaz clearly regards as his main theoretical contribution—namely, his treatment of the role played in devaluation by changes in the distribution of income.

The author correctly indicates that, for devaluation to improve the balance of payments, it must lead to a reduction in "absorption"—absorption being equivalent to net national product plus the deficit on current international account. The necessary reduction in absorption can be promptly achieved through a cut in domestic expenditure on imports and on "exportables," provided the relevant price elasticities are sufficiently favorable. But here the author encounters a difficulty. He states (p. 10) that "elasticity optimism does not necessarily lead to devaluation optimism," since the initial improvement in the trade balance will lead to an increase in the level of (money) income, thereby leading to increased domestic expenditure on imports and exportables and, consequently, to a new deterioration in the trade balance which may offset much or all of the earlier improvement.

But this, the author assures us, is not the whole story. As a potentially favorable aspect of devaluation, he places great emphasis on what he terms its "redistributive effect." The reasoning, as I understand it, runs as follows: In terms of domestic currency, devaluation raises the cost of living by raising the price level of international goods (imports and exportables). If the pressure to raise money-wage rates can be tightly restrained, there will be shift of real income away from wage earners, whose propensity to save is low, in favor of "capitalists," whose propensity to save is high. By a Keynesian route, this shift in income toward the "saving class" can induce a fall in real output, a consequent fall in absorption, and thus an improvement in the balance of payments. As evidence of such an effect, the author cites a 5 per cent drop in Argentine real output following the sharp depreciation of the peso late in 1958.

While the redistributive effect may in certain cases be a significant (though, in my view, needlessly painful) part of the adjustment process, I am disturbed by the clear implication in Díaz's exposition that a drop in real output is needed, following devaluation, to assure the necessary reduction in absorption. This is surely wrong. What is required is not a drop in real output—this need not and should not occur—but, rather, a drop in real expenditure by the domestic sector. The latter objective will all but certainly be realized, provided the level of money income is prevented from rising by as high a proportion as the devaluation-induced rise in the price level. Subject to this proviso, the real income available to the domestic sector will fall; the domestic demand curves for imports and exportables, however price inelastic, will shift to the left (the degree of shift depending on income elasticity); and a reduction in domestic expenditure on international goods—in other words, a reduction in absorption—will thus be assured. It should be noted that the mechanism here described makes it possible to reverse Díaz's statement about elasticity optimism, and to say instead that "elasticity pessimism does not necessarily lead to devaluation pessimism."

For the mechanism to work, however, devaluation must be accompanied by vigorous anti-inflationary measures to restrain the rise in the level of money income. Díaz's apparent failure to see the need for such measures may account for his decidely relaxed attitude toward monetary policy. He states (p. 158) that "for the sake of simplicity" he did not deal explicitly with monetary influences in his development of devaluation theory. In view of his puzzling observations about Argentine monetary policy, such an omission would appear to be a serious deficiency. Notwithstanding a 43 per cent increase in the money supply in 1959, the author regards the influence of Argentine monetary policy in that year as "neutral" (p. 159), because the increase was of the same order of magnitude as his estimate of the devaluation-induced rise in the Argentine price level. And despite a 29 per cent increase in the money supply in 1963, he attributes the poor economic performance in that year to what he calls (p.188) a "most restrictive monetary policy"! With such a philosophy, it is difficult to see how the Argentine inflation could ever come to an end or how currency depreciation could offer more than the most temporary relief.

But I do not wish to end this review on a negative note. On many matters, Díaz's treatment impresses me as eminently competent. I would particularly like to record my emphatic agreement with his subordination of the role played by terms of trade in the post-devaluation adjustment process.

RANDALL HINSHAW

Johns Hopkins Bologna Center, and
Claremont Graduate School

The British Balance of Payments. Edited by D. J. ROBERTSON and L. C. HUNTER. Edinburgh and London: Oliver and Boyd, 1966. Pp. vii, 187. 30s; $4.20.

Seven papers by British academic economists which appeared in 1966 as a symposium in the *Scottish Journal of Political Economy* comprise this volume. These papers are not concerned directly with the recent British balance-of-payments "problem," the resulting pound-sterling crises, or the means available to combat the problem. Rather, these papers may be thought of as providing background information for economists interested in the problems of sterling in the postwar period. For those well versed in the British balance of payments, little new and nothing very exciting will be found here. For those who have only a general notion of the existing U.K. difficulties, this volume will provide an efficient means of deepening and widening their knowledge. Both groups will find a host of previous research conveniently digested in one place.

Individual papers on the broad categories in the balance of payments—exports, imports, invisibles, long-term capital movements, and short-term capital or monetary movements—are included, and two additional papers—one on fiscal policy and the balance of payments, and one on trade with Europe—round out the contents. With one exception (the paper on fiscal policy), the method by which the authors attack their respective topics is essentially the same: a description of the historical movements in the data intertwined with a synthesis of various explanations for those movements. The explanations are

largely those found in existing literature, but sometimes these are buttressed with additional observations and analysis by the authors. While a fairly thorough grounding is thus given in the historical performance of the various components of the balance of payments, the chronic balance of payments disequilibrium which Great Britain has experienced in the past decade is nowhere explicitly attacked per se.

Four papers deal essentially with the current account items in the balance of payments. Postwar changes in the amount and composition of exports, imports, and invisible payments and receipts are reviewed in the first three papers. U.K. exports, it is found, have been affected adversely by the geographical pattern of trade, weak sales efforts, and relative price rises in Britain. Imports have risen especially rapidly in periods of full capacity utilization and excess demand, and have failed to fall in the subsequent "stop" period.

Receipts from and payments for services and unilateral transfers are traced from the late 1800's. Adverse factors for the overall balance of payments have been the decline in British shipping receipts and the increase in government transfers; favorable factors have been rising receipts from income on foreign investment and relatively stable balances on travel and "other" accounts. In no case do these three papers attempt to assess the relative contribution of their items to the overall British balance-of-payments problem, nor do they attempt to analyze changes in the structure and relative efficiency of British industry, the impact of economic policy, or other potential causes of the phenomena which they describe.

The fourth paper, on trade with Europe, unexplainably placed at the end, traces postwar developments in the magnitude and composition of exports and imports. Included is an interesting discussion on matters such as the impact of the struggle toward convertibility in Europe and the relaxation of wartime import and payments controls on U.K. trade with European countries in the earlier postwar period. Perhaps most interesting is the treatment of the division of Europe into two trade blocs and the impact of this on British trade. The author concludes that the establishment of a common tariff by the E.E.C. has had relatively little impact, while changes in demand have been much more important in causing changes in the U.K. trade pattern with that group.

A. D. Bain's paper on fiscal policy and the balance of payments deals with a potentially very interesting subject but fails to fulfill completely that potential. The analysis is concerned with the impact of tax structure on export and import competing industries; taxation of income on foreign investments and its effects on capital movements; and the influence of government expenditures on exports and imports. The most intellectually interesting part analyzes domestic taxes as trade distorting devices. The relevant theory is succinctly reviewed, but the evaluation of the U.K. system in light of the theory, is both skimpy and unconvincing, since the international comparisons needed to make the conclusions valid are absent.

The remaining two papers cover developments in the capital accounts. A. G. Kemp's paper on long-term capital movements contains two rewarding discussions, in addition to the summary of historical movements. The first attempts to evaluate the available evidence on the *net* balance-of-payments effect of

U.K. foreign investment. While concluding that foreign investment probably improves the U.K. trade balance, Kemp prudently leaves unanswered the magnitude of this improvement relative to the original capital outflow. The second provides a convenient capsule summary of the recent government actions to suppress capital outflow. With the growing number and complexity of such devices employed by the United Kingdom in recent years, this is a valuable service. Kemp's paper does, however, generate some minor annoyances. For example, he repeatedly writes as if the capital account should be made to adjust to the current-account balance, and ignores the conflict of this view with the traditional transfer theory. He also supposes that one can tell whether foreign investment has been made in lieu of domestic investment or in addition to domestic investment. Unfortunately, his criteria yield question-begging results.

P. M. Oppenheimer's paper on monetary movements and the international status of sterling is the best and the longest. The quality of this paper does not result from a difference in approach from the others, but from a difference of thoroughness. This paper ranges widely over the short-term capital horizon. It touches such topics as the reserve currency status of sterling and how it has changed; the effectiveness of bank-rate and forward-exchange-market policy in altering international short-term money movements; sterling crises and how they occur; and the question of what the impact of the reserve currency status and the sterling area has been on the British balance of payments and British economic policy. Oppenheimer thus comes the closest of any of the writers to treating the "problem" aspect of the British balance of payments. Indeed, he concludes that the international position of sterling has made little difference to the U.K. balance of payments, but has "discouraged policy-makers from going for sensible solutions" presumably implying that a pound devaluation would not cause a world catastrophe.

These essays are prefaced by a brief introduction by D. J. Robertson. Since the various items in the balance of payments are intricately interconnected, the repetition and incomplete integration encountered was scarcely unexpected. The quite unequal length, quality, and sophistication of analysis of the papers, however, may represent a more serious hindrance.

ROBERT G. HAWKINS

New York University

Business Finance; Investment and Security Markets; Insurance

The Stock Market and Economic Efficiency. By WILLIAM J. BAUMOL. The Millar Lectures, Number 6. New York: Fordham University Press, 1965. Pp. xiii, 95. $3.00.

In these four lectures the author, in his own words, has "tried to explore a wide range of subjects, each of which it was hoped would help us to understand the function of the stock market as an allocator of the economy's capital resources. In the process I have sought to integrate the analysis of the work-

ings of the stock market more closely into the framework of standard economic theory and to incorporate into the discussion a number of segments of the theory of corporation finance."

Specifically, the book consists of stimulating discussions of: (a) the role of the specialist, (b) the determinants of stock prices, and (c) the efficiency of the capital allocation process, prefaced by an introduction, and with a brief appendix discussion of the effect of stock options on the decisions of management and their sense of identification with stockholders. These are all timely and important topics. Moreover, especially as regards (b) and (c) there has been a recent proliferation of both analytical and empirical work, so it is particularly appropriate to assess how far we have come, and what remains to be done. Add to this Professor Baumol's unusual ability to see the broad sweep of economic problems and his expository skill, and it is no surprise that this is a stimulating volume that will have a large audience.

The lectures have an underlying similarity of structure, and a lesson in common. Each starts with an examination of what economic theory has to say about the problem. And in every case it turns out that theory has a great deal to say, but nothing conclusive. In theory, at least, there seem to be countervailing arguments about equally convincing, but leading to very different conclusions for economic behavior and policy. The resolution of this difficulty, therefore, rests with empirical research. This is Baumol's major point which his lectures drive home dramatically and powerfully. He demonstrates convincingly and with wit and wisdom that in fact we know too little about the stock market and associated activities to evaluate the efficiency of the process by which stock prices are determined and capital is allocated among the business enterprises that compete for its use. And thus his call for further research is pointed and meaningful rather than simply the perfunctory amen of the economist's litany. Indeed Baumol goes much further than simple encouragement. He provides a number of interesting specific research suggestions. On this level—an analysis of the issues, a review of what is known, and a statement of what has to be found out—these lectures are eminently successful.

But the author attempts more than this; he feels a responsibility to tell the reader where he stands in the welter of conflicting theory and "fact," and the reasons for his position. Given the state of the field, many such judgments are necessarily matters of taste. To argue about differences in taste is unprofitable. But, in a number of instances, in reaching his judgment, the author relies on a specified set of assumptions, a "model," a view of the world. And these assumptions are not consistent from chapter to chapter. It is in this connection that I find the lectures less satisfactory.

For example, much is made in the lecture on stock price determination of the Miller-Modigliani theorem which proves that "*given the firm's planned investments and future earnings and dividend payments*, it should make no difference to the shareholder whether he receives any dividends or, instead, the company retains these earnings to finance its investments," and a particular elaboration of this proposition—the "true value will out theorem"—which "shows that in the long run, even if a stock offers a relatively low payout rate

and is consistently undervalued by the market, it will yield a full measure of returns to the holder of the security." Indeed these theorems are offered as one of the bases for the "strong reason to suspect that in a rough and ready way, security prices do follow closely the developments in company prospects," despite the fact that because "the long-run competitive mechanism, as ordinarily conceived, functions on the stock exchange only very imperfectly if at all," in this market we cannot expect that the forces summarized under supply and demand would bring about a structure of prices that incorporated this correspondence.

The theorem which proves that dividend policy is irrelevant, is, of course, derived from a set of assumptions and is valid only under these specified conditions. The proof depends essentially on the assumptions associated with perfect markets, including the assumption that there are no taxes.[1]

In the next lecture, concerned with the efficiency of the capital allocation process, these assumptions are eschewed, and a more "realistic" set is adopted. Specifically, Baumol notes that the irrelevance of the rate of dividend pay-out "holds only in a perfect world. As imperfections creep in, the scales become unbalanced" and dividend policy is not a matter of indifference. After pointing to a number of these imperfections—major ones being transaction costs and taxes, particularly the differential between the rate on regular income (dividends) and the lower rate on capital gains—he concludes that there are good reasons to hold "that the bulk of business enterprise should finance its investments insofar as possible entirely out of retained earnings *because that is, characteristically, the cheapest way to raise additional funds*."

What is the reader to make of all this? In one chapter an important reason for the position that the author takes is a theorem based on assumptions of market perfection, among them no taxes. In the next chapter doubt is cast on the realism and relevance of these assumptions and the arguments are based on an imperfect world with taxes. Given their particular assumptions, both the retention argument and the earlier irrelevance theorem are logically correct. But in any particular world they cannot both be valid. This is not primarily a matter of the realism of assumptions, but of consistency in their use. Either the world is perfect or imperfect. Either we have taxes or we don't. Either dividend policy is irrelevant, as in Chapter 3, or consequential as in Chapter 4. To go from one set of assumptions to another at will is to have the ability to prove everything and, hence, nothing.

To end on a critical note would be unfair and misleading. For these lectures accomplish their main purpose in stimulating and instructive fashion. Any one interested in economic theory, financial markets, or business finance should read this book. He will find in it a provocative summary of the state of the

[1] Baumol notes that Miller and Modigliani demonstrate that their theorem can be shown to hold in the face of a number of imperfections if investors behave with "symmetric market rationality," but this does not affect my simple point. Parenthetically, the proof also assumes that the rate at which a dividend stream is discounted is invariant with respect to the time pattern of that stream. Gordon considers that this amounts to "assuming away the fundamental problem." (See Myron J. Gordon, *The Investment, Financing and Valuation of the Corporation*, Homewood 1962, p. 61.)

theory, a useful, albeit necessarily far from complete, account of recent empirical work, and many imaginative suggestions for further research.

DANIEL M. HOLLAND

Massachusetts Institute of Technology

Industrial Organization; Government and Business; Industry Studies

Crude Oil Prices in the Middle East: A Study in Oligopolistic Price Behavior. By HELMUT J. FRANK. New York: Frederick A. Praeger, 1966. Pp. xi, 209. $15.00.

A Financial Analysis of Middle Eastern Oil Concessions: 1901-1965. By ZUHAYR MIKDASHI. New York: Frederick A. Praeger, 1966. Pp. xviii, 340. $17.50.

Professor Helmut J. Frank's study is divided into two parts. In the first part, he reviews with admirable clarity and detail the evolvement of the pricing system. Up to the beginning of World War II, a single basing point system of pricing crude oil prevailed throughout the world. Because of the war conditions, this system gave way to a dual basing point system whereby the supplier of the Middle East oil continued to quote the prevailing prices in the U.S. Gulf of Mexico but charged the actual cost of shipping from the Middle East. The postwar increase in demand for oil and the diminishing importance of the Western Hemisphere as the main supplier of oil provided the impetus for the industry's tremendous expansion in the Middle East. In order to secure new outlets for his oil, the Middle East supplier began in 1950 to post his "own" prices which were lower than those in the U.S. Gulf. This price differential continued to widen during the last decade because of entry, emergence of excess capacity, and the imposition of import restrictions in the United States. Frank concludes that because of these and other developments, the few oligopolists lost control over prices. In the second part, Frank attempts an assessment of the effectiveness of pricing policies with respect to intra-industry and inter-industry allocation of resources.

Although Frank provides us with a good description of the forces which have an impact on pricing policies, the reader is left on his own to judge whether his criteria of effectiveness have been met or not. Moreover, some of his assertions and conclusions cannot be supported either analytically or empirically. Thus we are told that the kink in the oligopolist's demand curve requires the seller to abstain from raising his price. However, since prices were actually raised, we cannot, therefore, say that a kinked demand curve has prevailed (p.141). Frank is also guilty of a major inconsistency in his assessment of the industry's market structure. His conclusion that the oil industry in the Middle East is no longer a tight oligopoly is contradicted by his own assertion that the importance of entry in the Middle East oil industry should not be exaggerated since the newcomers controlled in 1960 only 3.6 per cent of the out-

put (p. 134). Oligopoly is still the best desciption of a market structure where only eight producers jointly control 96.4 per cent of the output of a standardized commodity.

Professor Zuhayr Mikdashi's volume will impress those who are interested in the economics of Middle East oil as an excellent, straightforward, and comprehensive account of the financial developments which have taken place since the turn of the century. His purpose is to study the determinants of the methods and rates of payment to the host governments of the Middle East and the financial performance of the oil companies. To this end, he painstakingly and diligently plowed through an enormous bibliography of books, official publications, Congressional hearings, company reports, and minutes of negotiations. Also of great interest is the wealth of data which Mikdashi's forty-five tables contain.

The book is divided into three parts to correspond with the three distinct methods of payment. The first part covers the period 1901-32 when payment was based on the integrated profits of the producing company. The second part covers the period 1925-51 when payment was a fixed sum per unit of output. The third and the most important part of the book covers the period 1951-65. During this period the principle of profit sharing was introduced. According to this principle the host governments of Iran, Iraq, Kuwait, and Saudi Arabia receive 50 per cent of the profits realized on the production of crude oil.

A major criticism can be leveled against the importance which Mikdashi attaches to the discounted cash flow method as a measure of the rate of return. Mikdashi apparently fails to distinguish between the use of the DCF method as a tool for capital budgeting and its use as a measure of profitability. Few people will disagree that in its first role the DCF method is superior to the conventional accounting rate of return. Even Mikdashi finds himself forced to abandon his own "scientific" DCF method as a measure of profitability throughout the text except for brief mention on four occasions. Furthermore, his comparison of the DCF rates of return with the accounting rates of return and the conclusions drawn from the comparison are misleading. Thus we are told that the DCF rate of return for Iraq Petroleum Company is only 15 per cent as compared with an accounting rate of return of 56.6 per cent. But Mikdashi is guilty of using two different time periods for his calculations. It is very interesting to note, however, that the two methods produce strikingly similar results when the same time period is used. Thus Mikdashi calculated for the Iranian Consortium for the period 1954-64 the DCF rate of return to be "close to 70 per cent," while the accounting rate of return for the period 1955-64 is calculated to be 69.3 per cent. Had he used, as in this case, the same time period, a meaningful comparison would have been possible.

These criticisms should not, however, detract from the fact that Mikdashi's book is essential reading for any student of the economics of Middle East oil.

Abbas Alnasrawi

University of Vermont

Domestic Transportation—Practice, Theory and Policy. By ROY J. SAMPSON and MARTIN T. FARRIS. Boston: Houghton Mifflin Company, 1966. Pp. 464. $7.95.

In its general plan, organization, and emphasis, this addition to a relatively small but growing list of up-to-date textbooks on transport economics owes an obvious and acknowledged debt to the work of the late Stuart Daggett. Sampson and Farris attempt to treat all modes of inland transport. Their book emphasizes the movement of freight (rather than passengers), and it is written for students of both economics and traffic management.

The book is divided into eight parts. Parts I-III are essentially descriptive. They discuss the importance of transportation, its historical background and evolution in the United States, and the performance characteristics of the different modes viewed primarily from the standpoint of the user. Several chapters provide a detailed description of not only line-haul services but also other-than-line-haul services offered particularly by common carriers.

Parts IV-VI are more largely concerned with analytical problems. They deal with policy questions of current interest and introduce the reader to some important economic issues. Part IV begins with a brief account of the economic and legal basis of rates followed by a simple but useful analysis of the role of demand and costs in the pricing of transport services. This is followed by a concise description of tariffs and freight rate structures. Part IV concludes its discussion of transport rates with two chapters which: (1) emphasize the significance of the relation between transport costs and spatial economic activity, and (2) provide a brief introduction to location theory.

Part V treats the complex history of transport regulation by organizing its discussion around two themes: (1) an initial phase of regulated monopoly which attempted to force the regulated monopolists to compete, and (2) a second phase in which policy was designed to regulate transport competition. A descriptive chapter which summarizes some of the major federal and state regulatory agencies concludes this survey of transport regulation. Part VI considers goals of national transportation policy and emphasizes difficulties created both for definition and realization of these by conflicting interest groups. Part VII, entitled "Transportation Aspects of Physical Distribution Administration," covers topics of particular interest to shippers and traffic managers. A very brief concluding section, Part VIII, looks ahead by emphasizing the significance of further technological change.

The particular mix of analysis, institutional, and descriptive material contained in this text has at least one important drawback. Analysis of transport policy issues is likely to be distorted if passenger transport is neglected since it risks overlooking major determinants of investment in transport facilities. The joint nature of the product of transport facilities, involving both freight and passenger service, should not be ignored in an evaluation of transport policy. Much of the current federal interest in transport policy questions involves problems of investment policy. Pressure for the formation of a Department of Transportation reflects this interest. Also, recent federal government activities involving the planning of integrated passenger transport facilities have been

forced to take the interdependence of freight and passenger services into account, especially in connection with proposed restructuring of railroad networks.

The text is somewhat marred by a tendency of the authors to indulge in obiter dicta that will either antagonize or puzzle sophisticated readers. But, in general, the style of writing is commendable in that the authors' relatively informal exposition and their willingness to make clear and provocative statements should contribute to their goal of stimulating the interest of beginning students in problems of transportation. The plan of the text and its execution suffer to some extent from the attempt to serve sets of readers who have different interests. This leads occasionally to undue brevity in the treatment of details which may be important to the traffic manager, as well as to oversimplification of analysis of broader policy issues. Nevertheless this text has definite areas of strength. It renders its readers a service in stressing the nature of the current regulatory problem which it properly characterizes as one of regulating competition. It also is sound in stressing the significance of the relationship between economic development, transport costs, and freight rates.

In its sometimes overbrief but often judicious discussion of institutional details, the text suggests some of the important and confusing factors affecting the transport industry. This text fails to produce a very clear perspective on the major economic issues of transport economics. But this failure cannot be blamed primarily on the authors. It reflects a general failure to date on the part of economists to produce an analytical framework within which a satisfactory evaluation of modern transport policy issues can be made. Transport economics at best is a very complicated subject. Its complexity is heightened in a pluralistic society, such as in the United States, which gives great scope to the influence of interest groups in the formation of public policies. And rationalization of transport affairs is further complicated by the continuing rapid change in transport technology. This text calls the attention of the reader to many of these problem areas. In this way it is a helpful introduction to a very complex and important area of economics.

Howard W. Nicholson

National Science Foundation

Land Economics; Agricultural Economics; Economic Geography; Housing

Water Resource Investment and the Public Interest. By Robert H. Haveman. Nashville: Vanderbilt University Press, 1965. Pp. xiii, 199. $6.00.

Water Resources Development. By E. Kuiper. Washington: Butterworth, 1965. Pp. xi, 483. $24.50.

These books are concerned with a significant public expenditure category and one which is likely to continue to be of considerable importance in the United States for some years to come. As Haveman notes in his volume, total

federal investment in water resource projects had reached the sum of $23.5 billion by 1963 with more than half of this amount appropriated since 1950. Furthermore, many more billions of dollars will be sought—and no doubt appropriated—in the near future for water development programs. Hence, any guidelines, criteria, evaluations, etc., that will contribute to a more effective water development program will be most welcomed. Both of these volumes attempt to make such a contribution but in vastly different ways.

Kuiper's book deals with the problem of developing a river system to the greatest benefit of the people who live in the river's drainage system. The emphasis is on the engineering aspects of preliminary project design that will enable sound basin development to proceed from reasonable technical and economic analysis of the alternative project schemes.

In contrast, Haveman's study undertakes to analyze empirically and somewhat theoretically the relationship between the two primary determinants of economic welfare, viz., economic efficiency and income distribution, and incorporates both of them into a multidimensional welfare function as applied to water resource development. "In particular, it attempts to analyze these forces in both their *ex ante* roles as self-contained goals which determine the allocation of water resource appropriations and their *ex post* roles as effecters of changes in social economic welfare" (pp. 9-10).

Kuiper's volume is an excellent treatment of the integrated development of the water resources for an entire basin. The subjects included are water resources planning, hydrology, hydraulics, river morphology, hydraulic structures, flood control, water power, irrigation, navigation, water supply, and economic analysis. Throughout the book the author sets the stage with a broad discussion that puts each subject into proper perspective with respect to the whole work. The treatment of basic principles is necessarily brief but is appropriately well founded on physical reasoning so that it is useful to the engineer who needs a reminder about those principles upon which sound practical analysis rests and it should provide an insight into engineering analysis to the nonengineering reader. The engineering analysis is practical. The limitations and imprecision of all the analytical techniques used for component design and project synthesis are brought to light; however, the author, while not pretending a precision that is not attainable, gives reasonable design criteria and guides so that well-balanced preliminary project designs can be developed.

Kuiper states that it has been his objective to provide sufficient information: (1) to understand the underlying principles and interrelationship of the main elements of river development: (2) to make possible the preliminary design and cost estimates of the required engineering works; (3) to provide guide lines for the selection of the most desirable water plan.

The author has achieved his objective for instructional use with the exception of making possible cost estimates of the required engineering works. There are no adequate data or even guide lines on which to make preliminary cost estimates. This information has to be developed from outside sources. Information is available from which to make cost analyses once project costs have been reliably estimated. The book by itself does not provide adequate

information for preliminary engineering design but it serves as an excellent framework for project planning and preliminary design if it is supplemented with references from its excellent bibliography (35 pages, briefly annotated) and adequate practical engineering background, skill, and resources.

The explanation of engineering analysis and its relationship to economic analysis of water resource development should make this a useful reference to the economist who is interested in understanding the viewpoint and role of the civil engineer in the effective technical and social development of our natural resources.

In his book, Haveman expresses the hope that he is adding to the stream of progress in the area of water resource development begun in the middle 1950's by the studies of Otto Eckstein, John V. Krutilla, and Roland McKean (economists) and Arthur Maass (political scientist). I would also include in this highly selected list of basic studies the three-volume report of the President's Water Resources Policy Commission (1950). The author states that his study is derived from these analyses, and what he has "added is perhaps most simply described as an empirical combination of the two primary determinants of that variable which economists call economic welfare" (p. ix). He then notes that while both the size of the national income and its distribution have been recognized in economic welfare, economists have generally assumed that welfare is a single-valued function of only the size of the national income. He feels that the distinctiveness of his study is the empirical investigation of the relationship between these two welfare determinants and their incorporation into a multidimensional welfare function.

The Haveman study is regional in nature and deals empirically with federal appropriations to the U. S. Army Corps of Engineers for water projects in ten southern states in the years since World War II. In this period, 1951 to 1962, the Corps received some 60 per cent (over $8 billion) of total federal appropriations for water resource projects.

The rather compact volume consists of six chapters, an epilogue, and three appendixes. In the first section, Chapters 1-3, the nature of the past Corps of Engineers program is described and evaluated. The stated goals of the program are outlined, and an empirical verification of the operation of these motivations is set forth. Chapter 3 considers three key questions pertaining to economic efficiency and income redistribution through water resource projects. (1) Is there evidence that economic efficiency has been an effective Congressional criteria of choice? (2) Can it be inferred that Congress has appropriated a relatively greater share of funds to low income (per capita) states, and does this imply a desire to make use of the water development program as an aid to low income areas? (3) Does the evidence imply that such an attempted redistribution has actually led to less disparity in per capita regional income?

In the second and major section of the study, Chapters 4-6, the two problems of income distribution and economic efficiency (or lack of it) are empirically analyzed in separate chapters and then combined in an attempt to demonstrate the effect of the application of economic efficiency criteria on regional income distribution. The conclusion of this demonstration is that, given the

goal of maximizing the size of the national income, the construction of such projects is an inefficient means of securing a redistribution of income to favored groups.

In the final section, the Epilogue, a rather unorthodox attempt is made to combine both of these primary determinants of changes in economic welfare into a single multidimensional approach. It is concluded that by substituting a multidimensional welfare function for the single-value national income function, it can be demonstrated that the redistribution of income through the construction of some economically inefficient projects may well be a desirable technique.

The three appendixes contain (1) an estimate of the state incidence of federal taxes, (2) a discussion of risk, uncertainty and water resource investment decisions, and (3) data on southern Corps projects.

After careful analysis of the Corps projects in the ten southern states in terms of income redistribution and economic efficiency (Chapters 1-6), he reaches the conclusion that nearly half of the projects, as measured by federal funds committed to the projects, should not have been undertaken as the construction of these projects has led to a misallocation of national resources and economic waste (pp. 117, 123). However, he feels that in terms of economic welfare this conclusion is open to question. "That is to say, conclusions on changes in national or regional welfare have been identified with conclusions on changes in national income only by assuming the existence of a naive welfare function which weights increments of income to each and every recipient equally, i.e., by assuming that the marginal utility of income is equal and constant for every individual" (p. 128). He feels that some new effort must be made to secure a superior technique for evaluating changes in economic welfare.

Haveman tentatively accepts a suggestion made by Eckstein to permit the economist to accept the judgments of policy-makers in assigning unequal weights to the dollar gains and losses to different individuals. "Because such an approach appears to be not only a solution to the economist's ethical problem but also empirically feasible in that it allows real weights to be attached to increments of income to different individuals, it is tentatively accepted as being a superior approach and used in a series of experiments in welfare evaluation" (p. 132). On the basis of different sets of weights he performs two experiments in welfare evaluation and concludes that by substituting a multidimensional welfare function for the single-valued national income function, the construction of some economically inefficient projects may well be desirable.

I certainly agree with Haveman that something more is needed than project evaluation on the basis of economic efficiency and income distribution only. My own view is that financial feasibility is not the same as economic feasibility. While financial costs and returns should be considered most carefully, I believe that they alone should not determine the desirability of a project. A more tenable position, it seems to me, would be for the federal government to seek to conserve and develop the nation's natural resources in such a manner as to provide the broadest and strongest possible foundation for all sorts of

economic activities and to provide the greatest possible scope for all sorts of private investment.

Haveman's study certainly fulfills his expressed hope that it "adds to the stream of progress in this area." Any serious attempt to develop better criteria for the guidance of multibillion dollar public investment in water resources is a most welcome addition to the literature. This is such a study and deserves careful consideration.

JEWELL J. RASMUSSEN*

University of Utah

* Since Kuiper's book deals largely with the engineering aspects of river development, assistance was received from Professor Alan G. Fletcher of the Department of Civil Engineering in reviewing this volume.

The Common Wealth in Ocean Fisheries. By FRANCIS T. CHRISTY, JR., and ANTHONY SCOTT. Baltimore: Johns Hopkins University Press, for Resources for the Future, 1965. Pp. xiii, 281. $6.00.

Economists who are theorists and/or sport fishermen will find this book a useful introduction to the technology and analysis of commercial ocean fishery exploitation. The essence of a common property resource is the existence of certain social costs of exploitation which are not reflected in the private costs of exploitation. Consequently, a social optimum does not necessarily follow from private maximization. For fisheries, some sort of controls on catch rates and/or entry are required, but such controls are difficult if not impossible to enforce because we are dealing with a resource which is not subject to the complete authority of one national government.

Chapter 3 on the demand for fish shows clearly how the problem of national exploitation of fisheries is complicated by the diverse pattern of demand for fish products. Agreement on mesh size is difficult if in one country the demand is for large members of a species while in another the small members are in demand. Similarly, a control is complicated if in one country the demand is for a predator of a species demanded in another country.

Chapters 4 through 7 provide a competent and thorough discussion of the many aspects of supply—the ecological basis of fish production, the geographical distribution of fish populations, limitations on the potential productivity of the seas (the myth of boundless potential is effectively destroyed), aspects of extinction and depletion of fish populations, the technology of locating, catching, transporting and processing fish, and trends in output of different species by principal countries. Chapter 8 directs the book's treatment of supply and demand to the problem of meeting future demand. The remainder of the book, Chapters 9-13, deals with the problem of control in an international setting.

I find Chapter 2, dealing with the theory of the common-property natural resource as it applies to ocean fishing, the only disappointing part of a good book. It is marred by confusions concerning the relationship between the fish population and the catch under "sustainable yield," i.e., stationary conditions. The graphs reflect this confusion as well as contradictory assumptions which make the graphs inapplicable for comparing the exploitation of a fishery under

sole ownership with that of a fishery under decentralized ownership. Considering the importance of Chapter 2 for economists, it would seem that this review will best serve the authors and readers by devoting the remaining space to a brief restatement of the theory of fishery exploitation.

The technological basis of the theory, in its simplest form (i.e., the analysis of a single fish population without explicit regard for its interaction with other populations), is expressed in the differential equation $\dot{X} = f(X) - P$, where X = fish population in pounds, $\dot{X}$ = time rate of change in population, P = product catch rate, $f(X)$ = "recruitment" rate net of natural mortality. The sustainable yield is defined by the condition that $\dot{X} = 0$, or $P = f(X)$. The ecosystem described by $f(X)$ has the property that when population is low, recruitment net of natural mortality is low; as X increases net recruitment rises to a maximum then declines as natural mortality increases faster than gross recruitment (pp. 7-8). Hence, $f(X)$ has an inverted "U" shape: $f'(X^o) = 0$, $f(0) = f(\bar{X}) = 0$, $f''(X) < 0$, $f(X) > 0$, $0 < X < \bar{X}$, where $\bar{X}$ is the maximum self-sustaining population, and X^o is the population producing the largest recruitment net of natural mortality (sustainable yield).

Now consider that the fishery is exploited by N homogeneous fishermen with identical operating cost functions, and that the fishery's total output is not large enough to influence price, p. These are the assumptions employed by the authors (p. 10). The most natural hypothesis about operating cost requires it to be an increasing function of the individual fisherman's catch rate x, and a decreasing function of population i.e., $\emptyset = \emptyset(x, X)$. Here, we will assume with the authors that the cost function is independent of X, i.e., $\emptyset = \emptyset(x)$. We shall also assume explicitly that a minimum profit (or wage) rate, $\hat{\pi}$, is required to hold a fisherman in the industry. Long-run total cost is then $C = \hat{\pi} + \emptyset(x)$. Also we have $P \equiv Nx$.

In the case of sole ownership, the relevant profit function is $\Pi = pNx - NC(x)$, to be maximized over the decision variables x, N, and X, subject to $f(X) - Nx = \dot{X}$, with $\dot{X} = 0$ if we restrict ourselves to "sustainable yield" analysis. A sole owner has control over X as well as x, and must also decide how many fishermen (boats) will be employed under stationary conditions. The Lagrangian to be maximized is $L = pNx - NC(x) + \lambda[f(X) - Nx]$, giving the conditions: (1) $\lambda = p - C'(x^0)$, (2) $\lambda = p - C(x^0)/x^0$, (3) $\lambda f'(X^0) = 0$. Hence, if $\lambda > 0$, the catch rate of each boat is determined from (1) and (2), i.e., $C'(x^0) = C(x^0)/x^0$, marginal cost = average cost — the usual internal efficiency condition for the multifacility firm with a variable number of facilities. The equilibrium population is determined from (3) giving $f'(X^0) = 0$, i.e., internal efficiency requires the sustainable yield to be maximized. Finally, N is adjusted until Nx, the total catch, is equal to the sustainable yield $f(X^0)$.

The authors attempt to carry through this analysis by postulating aggregate profit, revenue, and cost functions of the form $\Pi(N) = R(N) - C(N)$. We may derive their functions by assuming suboptimization on X^0, and substituting $x = f(X^0)/N$ into the profit function, i.e., $\Pi(N) = pNx -$

$NC(x) = pf(X^o) - NC\,[f(X^o)/N]$, which is not consistent with the authors concave revenue and linear cost functions, in the Figures 1, 3, and 4. Also contrary to the above, they conclude that a maximizing sole owner would draw the natural population down to some $X < X^o$.

Applying the same assumptions to the case of decentralized competitive ownership, the profit function for each fisherman is $\pi = px - C(x)$. Profit maximization requires $p = C'(x^*)$. If $p = C'(x^*) > C(x^*)/x^*$, pure profit above $\hat{\pi}$ is enjoyed and additional fishermen enter the fishery. However, under the authors' assumption that price is constant, the increase in N does not lower p, and the situation is represented by the conditions: (4) $p = C'(x^*) > C(x^*)/x^*$, (5) $f(X) - Nx^* = \dot{X}$. But with $\dot{N} > 0$ (firms entering), sooner or later we have $\dot{X} < 0$, until the fishery is exhausted. That is, given the assumption of constant p, there is no escaping the conclusion that the fishery will eventually be exhausted. (There is a trivial exception to this if we have price below average cost. Then $\dot{N} < 0$, and the *industry* is exhausted as $X \rightarrow \bar{X}$, to the everlasting delight of sport fishermen.)

These criticisms do not, however, alter the validity of the authors' principal conclusion: Under conditions of competitive ownership the exploitation of the fishery is economically inefficient. The reason of course is the externality represented by the population variable, X, which affects the conditions of production for all firms, but does not enter as a decision variable in the individual firm's criterion function. The conclusion holds under the more general cost assumptions mentioned above, where $C = C(x, X)$. In this case long-run firm equilibrium is possible even if p is constant since the entry of firms reduces X and may increase cost until profits fall to $\hat{\pi}$. But a complete analysis of this interesting case, and explicit dynamic treatment of both industry size, N, and the fish population, X, is more appropriate for a research paper than a brief review.

VERNON L. SMITH

Purdue University

Soil Conservation in Perspective. By R. BURNELL HELD AND MARION CLAWSON. Baltimore: The Johns Hopkins Press, for Resources for the Future, Inc., 1965. Pp. xiii, 44. $7.50.

One aim of the Soil Conservation Service is to help farmers take action to avoid losses in soil productivity that are difficult to reverse after top soil has been eroded away. Though not large in relation to other federal activities, the expenditures are substantial enough (almost $100 million a year or $1.5 billion over the past thirty years) that one would have expected a serious evaluation of this effort by now.

In reviewing the history of soil conservation and bringing together the results of numerous previous studies, Held and Clawson give the most complete assessment yet available. The book begins with a rather standard exposition of the economics of soil conservation, followed by a review of the events leading to establishment of the Soil Conservation Service and of changes in the content of the agency program since it was established. Early emphasis was on

soil conservation as usually defined (construction of terraces and contour plowing to control erosion). Before long, the agency added farm planning that substantially changed the enterprise combinations of a farm, as part of the planning assistance offered to farmers. Primarily this type of planning increases present income to farmers. Over the years, still other Department of Agriculture activities were begun that, confusingly, have been labelled soil conservation. These include flood control, payments for using ferilizer, and payments for idling land to control production. The book discusses these activities but appropriately does not give them major attention. Expenditures on them are many times as great as the $100 million annual expenditures on soil conservation as it is more usually defined. This book is probably the best single source to find out what program content has been, what has been and has not been called soil conservation and why, and what is known about the economic and physical effects of soil conservation activities.

One of the purposes is to juxtapose soil conservation efforts and current agricultural adjustments. The conflicts are emphasized between output-increasing activities and progress to control surplus agricultural production. Yet, the possibility is overlooked that the soil conservation agency may have had a desirable income distributional impact within agriculture. Through farm planning, technical assistance has been carried to farmers who might not have been served by other agencies. The contribution to increasing overall output has been minor compared to other activities including agricultural research and price support programs when prices have been supported without controlling production.

In the middle of the book, on "Performance and Evaluation," the large amount of data available on physical soil conditions is reviewed. The authors stress the still existing lack of useful information, without however offering a serious research agenda for improving the situation. I wish a try had been made to reach more definite conclusions from the summary of studies of returns and costs of soil conservation in various areas. In addition, since the previous studies have lacked a common framework, it would have been useful to spell out the kind of effort that would be required to estimate the returns from soil conservation on a common basis in different areas of the country. The cost of this effort is probably modest. While more research is needed on the effects of erosion on crop yields, it might be concluded that enough is known to obtain fairly reliable results on economic returns without waiting for years of controlled experiments.

The last two chapters concern the future and feature the interplay between the declining political power of agriculture, appeal of the conservation idea relative to other goals, and the need for adaptation by local soil conservation organizations. The tone is that accommodating changes rather than drastic departures will occur, and the recommendations are in this mild vein. The five pages devoted to outlining an ideal program stress the desirability of having more clearly defined soil conservation objectives, better information on the physical effects of conservation, consistency with other programs, more analysis of the economics of conservation and questioning of subsidies. In realistic recognition that the Soil Conservation Service will not limit itself to soil con-

servation as defined narrowly, it is implied that the agency should foster needed agricultural adjustments in addition to promoting soil conservation.

Too frequently throughout the book, the analysis stops at the point where general questions are raised. Instead, in my opinion, definite hypotheses should have been advanced leading to more pointed recommendations. A disappointingly vague conclusion about the condition of the soil is that much has been accomplished but much remains to be done. Would it not be better to recommend a benefit-cost analysis for measures on all farms aided by the Soil Conservation Service, as an operational way of trying to undertake only worthwhile conservation. Pending precise results, public debate would gain from a professional judgment that terracing tends to be too expensive to justify. Contouring often pays but at interest rates too low to be profitable to farmers. The case here from society's point of view is debatable enough so that contouring might be subsidized as best in view of uncertainties.

Many observers have suggested that technical assistance be better geared to individual career capabilities and to farm and nonfarm employment possibilities. In response to exhortations along these lines, there have been pilot efforts by the Soil Conservation Service to promote economic development in local areas. An encouraging result is that some full-time assistance appears effective in catalyzing local group action. However, much of the energy has been devoted to traditional activities of the Soil Conservation Service. In short, to foster needed adjusments requires more than a call for a change of direction, both in doing economic research and in achieving reorientations of Congressmen and persons working in agencies.

G. S. TOLLEY

University of Chicago

Capital Formation in Indian Agriculture. By TARA SHUKLA. Bombay: Vora & Co., 1965. Pp. xiv, 261. $6.00.

This study brings together in highly useful form information which shows for India as a whole, and for eight selected regions, the growth of stock or inventory of five major categories of durable assets used in farming between 1920 and 1960. In addition to the estimates of growth for the respective inventories of farm land, buildings, irrigation facilities, work animals and implements, the study makes analytical use of parallel data covering the growth of farm labor and output. The entire work has benefited from the carefully prepared analytical frame of reference that appears in Part I and which has guided the author at every crucial stage of the inquiry.

Compilation of the various series which show capital growth over four decades in some detail and on a regional basis, was a formidable undertaking, in view of the well-known problems and frustrations which beset compilers of comparable data over so long a period. In a chapter devoted to sources, and to shortcomings of data and their adjustment, the author reveals the nature and scope of the statistical problems which confronted her when she attempted to measure capital formation. That this task of assembling and adjusting original data was carefully and skillfully done will, I think, be widely recognized. Until fuller basic information is available in the reports of government agen-

cies and other fact-compiling bodies it seems unlikely that more trustworthy measures of capital growth in Indian agriculture will be produced. For this accomplishment alone the study deserves acclaim.

The growth of inventories portrayed in this study makes it clear that some net investment occurred in Indian agriculture during the 40-year span which ended in 1960, but the increases in durable capital assets which reflect it averaged only about one per cent per annum. This was barely enough to provide even the traditional, relatively inefficient implements to the ever expanding supply of farm labor, and it barely kept the capital-labor ratio from declining. Certainly it provided little with which Indian agriculture could mount a technological advance!

Small though the increase in farm assets was, it may come as a surprise that, except for funds which government had invested in irrigation and land reclamation projects (which before 1950 amounted to less than three per cent of total gross agricultural capital formation), the savings which made possible the growth of agricultural capital came mainly from the income of Indian farmers themselves. That capital from this source was obtained only as a result of considerable privation will hardly be doubted. The surprisingly strong motivation to save seems to have been associated with the inexorable growth of farm labor, and with the imperative need of farmers to equip the additional workers with at least traditional rude forms of capital. So, the relentless growth of the labor supply, and the low substitutability of labor for capital, have been the forces, more than any others, which determined the rate of capital formation in Indian agriculture. Moreover, the forms which the new capital assets have taken were greatly influenced by the source of the savings. Plagued by very low incomes and a relentless need to equip additional workers, farmers generally have been reluctant to invest their meager savings in anything but the traditional types of capital. So much is this so that Dr. Shukla reports evidence of "excess capacity" for some of the traditional forms of capital. There is, however, also evidence that farmers in India may relax a little this ultracautious approach to investment in periods of rising incomes, or when prospects for prices or crops are especially bright. Thus traditional types of capital like bullocks and wooden plows continue heavily to dominate the Indian farmer's holdings, despite recent impressive percentage gains of nontraditional types. Such high percentage gains—687 for tractors and 939 for iron plows—made in the quarter century before 1960, have had small influence on the composition of farm assets so far because they were measured from very small bases.

Still, these high percentage increases of nontraditional types of capital herald the beginnings of improvement in methods of cultivation. Further evidence that some slight improvement has occurred since World War II is found in the comparison of the share of gross investment in farm implements which was directed to the nontraditional, or technology-improving, types. In a five-year period ending in 1946 nontraditional implements (iron plows, sugar cane crushers, electric pumps, oil engines and tractors) accounted for twelve per cent of the gross investment in implements. Fifteen years later, in a similar period, the nontraditional types accounted for thirty-nine per cent. But it must

be reiterated that despite these encouraging indications of change, the great mass of capital formed in recent decades has continued to be along traditional lines—bullocks, wooden plows and carts. It is significant that the capital-product ratio began to decline only after 1950.

One of India's basic needs is for substantial and continued improvement in farm technology. A mild improvement occurred during the 1950's in connection with efforts to plan the economy, to alter the structure of agriculture by land reforms, and to reform credit institutions and market organization. Above all farm technology was improved by the establishment of extension services and community development projects. This stirring on many fronts to improve the methods of production was accompanied by capital formation more active than at any other period within the 40 year survey.

India's experience during the 1950's seems to indicate that, in an underdeveloped country, technological improvement, broadly defined, is a more potent activator of the saving-investment process than high incomes. Dr. Shukla suggests that "probably one explanation of the low rate of investment is the lack of technological improvement over much of the period we studied." Cause and effect become somewhat blurred here. There is much in this study which rightly suggests that poverty of ideas and lack of technical knowledge are not the least among the deficiencies which account for low productivity in underdeveloped countries.

ALVIN S. TOSTLEBE

College of Wooster

Labor Economics

Manpower Planning in a Free Society. By RICHARD A. LESTER. Princeton: Princeton University Press, 1966. Pp. xiv, 227. $5.00.

The Economics of Manpower Planning. Edited by M. R. SINHA. Bombay: Indian Institute of Asian Studies, 1965. Pp. xv, 194. $4.00.

Is it conceivable and technically feasible to "plan" allocation and pricing of resources in a given sector while the whole system remains predominantly a "free" (unplanned) economy? This is one of the fundamental questions that arise from *Manpower Planning in a Free Society,* a "think piece" as its author calls it. This reader's reaction to the question is that "planning" in the context of the book is needlessly provocative. "Labor market policy" would be a neutral and more fitting expression for the kind of public measures proposed by the author. The need for "manpower planning," in this book, arises from widespread and entrenched imperfections in labor markets that result in wasteful redundancies of certain human resources and acute shortages of others.

If "planning" is only a sales word, Professor Lester's analysis of labor-market imperfections and of the lack of fit between the formal system of education and the economy's occupational requirements is extremely illuminating. Of particular importance is his analysis of much neglected imperfections on the

demand side of the labor market (Ch. 2 and 5). Many firms seem to entertain economically irrational desires and values. They tend to prize formal education for its own sake with little regard to other equally important indicators of skill and ability. The obsession with formal education has led to an escalation of hiring standards far beyond the basic requirements of job performance and to personnel policies that tend to dissociate the employees from labor markets. Seniority rights, fringe benefits, and welfare facilities further reduce inter-firm labor mobility. In addition, some firms discriminate against disadvantaged workers and members of minority groups for no valid economic reasons. Under the circumstances, employment does not respond to wages in the manner expected from the theoretical wage-employment relationship in which wages serve as inducements to mobility.

Imperfections on the supply side of the labor market are already well known in the literature and major findings are summarized in Chapter 6. Taken as a whole, labor market imperfections described and analyzed in this book do appear formidable and generate a sense of crisis for the urgency of a rational labor market policy. The author's references to the successes of various measures in Western Europe also make it clear that much of the waste of human resources resulting from market imperfections in the United States is wholly unnecessary. Lester thus proposes an Employment Service as a central instrument of his manpower "planning," though it is to be an institution restructured on a radically different basis from the existing institution (Ch. 3).

In other chapters, the author pleads for more research in the benefit-cost analysis of various manpower measures than has hitherto been undertaken (Ch. 7) and evaluates the contributions of research results toward effective manpower utilization (Ch. 8). His policy proposals, summarized in the concluding chapter, include a reorganized Employment Service, improvements in career counselling in high schools, a vocational training network, employment opportunities for disadvantaged workers, governmental guidelines for labor mobility, and benefit-cost analysis of manpower measures (Ch. 9). An appendix reproducing the "Manpower Development and Training Act of 1962" and an index complete the book.

Economics of Manpower Planning is a collection of seven articles that previously appeared in the *International Labor Review*. The common theme of these papers is how to fill the skill requirements of rapid economic development in underdeveloped countries. Thus, "manpower planning" takes on a different, and quite specific, meaning in this context. Manpower planning means planned increase in the supply of skills that require long periods of training whose absence create bottlenecks of planned economic development.

The most concise statement of the operational principle of manpower planning in underdeveloped countries is made by Michael Debeauvais in his opening essay, "Manpower Planning in Developing Countries." He feels that until more sophisticated calculations can be made of skill requirements, "manpower planning is restricted to the more modest aim of estimating the minimum, rather than optimum, resources to be directed to training purposes in order to safeguard economic planning targets." This means, according to Debeauvais, "deducing manpower requirements at the different levels of skill in the light of

production aims, and then drawing up the corresponding training programs. In this way an attempt is made to harmonize or, if possible, integrate economic planning, manpower planning, and educational planning." Debeauvais and others add some insights and variations to this basic principle.

The other six articles are: "The Interdependence of High Level Manpower Planning and Economic Planning" by F. Paukert, "The Labour Market and the Manpower Forecaster: Some Problems" by John Vaizey, "The Economics of Manpower Forecasting" by R. G. Hollister, "Long Term Planning of Employment in the Hungarian People's Republic" by Janos Timar, "Manpower Planning in the Restructuring of Education" by Anver Hovne, and "Manpower Policy in Japan" by Saburo Okita.

Hollister's article is a world apart from the others. Much to the delight of academic economists, it is theoretical and rigorous. Hollister explores the relationship between skill mix and product mix at different levels of national income in order to evaluate the success or failure of forecasting future skill requirements. Success is assured only under stringent conditions, i.e., constant skill coefficients and accurate knowledge of future product mix. This conclusion is enriched by considerations of major variations in the elasticity of substitution between different skills or between skill and capital. However, empirical testing of various hypotheses that follow from Hollister's analysis is fraught with great difficulties. Even the best empirical studies of changes in labor input that existed at the time of Hollister's writing (studies by Chenery, Fabricant, Komiya, and some others) fell far short of the requirements of his model.

In view of the complexity of factors that shape the course of skill mix in economic development as analyzed by Hollister, the practical propositions and strategies made by other authors in this book do sound unbelievably audacious. But this is the difference between the world of administrators and that of academic economists. Administrators often have to make decisions on the basis of insufficient evidence and analysis. If their propositions and actions broadly agree with academic analysis as exemplified by the articles in this book, they are justified for administrative efficiency.

Koji Taira

Stanford University

The Urbanization of Japanese Labor, 1868-1955. By Thomas O. Wilkinson. Amherst: University of Massachusetts Press, 1965. Pp. xvi, 243. $6.00.

Mr. Wilkinson, who teaches sociology at the University of Massachusetts, has written an uneven book. When his analysis concentrates on Japanese urban demography (Chapters 5-10) the results are most interesting and informative. But when he attempts to place the demographic developments in historical, economic, and comparative perspective, the outcome is far less satisfactory.

The book deals with three broad questions: (1) How urban has Japan been throughout her modern period and what have been the rates of change of urbanization? (2) What are the demographic and ecological traits of Japan's urbanizing population and specifically her employed population? (3) What

are the significant parallels and contrasts between Japanese urbanization and that of the West? Questions (1) and (2) are well handled in a set of chapters on demographic characteristics of the urban population, a functional classification of Japanese cities, urban growth and demographic differentials by functional classification, and a particularly valuable chapter on metropolitan development in Japan. Wilkinson concludes that "In terms of the universals of the urbanization process, Japanese experience conforms to the theoretical model" (p. 205) which stresses the shift from agriculture to industry, more extensive division of labor, and the development of metropolitan complexes. He also underlines some unique elements in the Japanese urban-industrial transition: the role of government, "the penetration of agricultural activities into the administratively defined urban population's labor force [showing] the narrowness of the line between urban and rural labor force differentiation" (p. 207), and the continuing social and economic role of the traditional Japanese familial unit in the city.

Throughout the book, but especially in Chapter 2 on preindustrial urbanization, there are allusions to Japanese economic history. Many of these are inaccurate or vague. For example, Wilkinson speaks of "The relative stability of Japan's total population . . . during the Tokugawa Period. . . ." (p. 19). This period lasted from 1603 to 1867, and most informed scholars believe that the period of stability lasted only from the 1720's to perhaps the 1840's.[1] On the same page we read that ". . . the Kanto Plain, containing Edo (Tokyo) is by far the largest and most productive . . ." agricultural area in Japan during the rule of the *shogunate*. This must be false on two counts. First, the Tosan region produced a larger total agricultural output.[2] However, if by productive we are meant to infer something concerning land productivity, it is also well known that the yields in the Kanto Plain were below those of the southwest.[3] Nor is it correct to say that "the practice of *sankin kotai* or alternate residence instituted by the *shogun* required that the lord spend a specified period of each year in residence in Edo" (p. 21). The period of residence varied, but generally it was alternate years. Readers may also be surprised by the statement that "landlords were little concerned with agricultural productivity" (p. 58) when applied to the Meiji Period.

Equally questionable are some of the more economic statements and conclusions. Two examples will suffice. Take the following about the period 1908-20: "Industrialization based largely upon a narrow export production, i.e. textiles, and the exploitation of limited mineral resources had almost reached a plateau" (p. 47). Yet, in 1913-17, the export-output ratios for cotton fabrics were 24.2 per cent, and for cotton yarn 30.1 per cent, at a time when these industries were growing rapidly.[4] Total population was about 55 million, and Japanese industrialization certainly had a more solid base than narrow export production. In another place (p. 199) the author distinguishes two basically

[1] E.g. Irene B. Taeuber, *The Population of Japan*, Princeton 1958, pp. 20 ff.

[2] T. C. Smith, *The Agrarian Origins of Modern Japan*, Stanford 1959, p. 72.

[3] Cf. T. Ogura, ed., *Agricultural Development in Modern Japan*, Tokyo, Japan FAO Association, 1963, Ch. 24.

[4] M. Shinohara, *Growth and Cycles in the Japanese Economy*, Tokyo 1962, p. 48.

dissimilar productive systems in Japan currently and historically: the strategic industries—"shipbuilding, chemicals, armaments, and the like"—having since their beginning utilized advanced industrial technology, and production of goods for home consumption which is largely in the hands of household-handicraft units. Many scholars have used two-sector models in approaching the Japanese economy, but this version makes little sense. If we talk about modern industries, how can they be confined to "strategic" manufacturing? And don't the modern industries cater to the home market? Aren't the household-handicraft units engaged in exporting? Wilkinson even has his household-handicraft sector engaging in *subsistence* production, and that is pretty hard to imagine except in most unusual circumstances.

Finally, a word or two about comparative analysis, as used by the author. At many places in the book comparisons are made with "the West," but just exactly what this concept means or why it is the proper comparative standard for Japan is not explained. Frequently the West seems to mean Great Britain and France, and this creates problems in the form of artificial contrasts. A meaningful comparison of Japanese and European urbanization must, it seems to me, recognize the distinction between pioneer industrializers and follower countries. Only in this fashion can we make intelligent analogies. By way of illustration, let us look at the following statement: "The Industrial Revolution came to Western Europe at the culmination of a long period of revolutionary pressure from below, from an urban-based merchant class clamoring for destruction of the old social order and creation of a new one in which new institutions generated in an urban, commercial context could flourish" (p. 32). Wilkinson points out that it was not this way in Japan, and I agree. Even if this description fits Britain and France—and I have my doubts—one could still ask: so what? The industrial revolution did not come to Germany and Russia after a long period of revolutionary pressure from below, and that may be much more relevant in trying to generalize Japan's experience.

All in all, I would sum up this way: very valuable information on Japanese urbanization between 1868 and 1955 with, however, an interpretation of the results not up to the level of the demographic work.

HENRY ROSOVSKY

Harvard University

Strategies for the Displaced Worker. By GEORGE P. SHULTZ AND ARNOLD R. WEBER. New York: Harper & Row, 1966. Pp. ix, 221. $4.95.

Automation and its concomitant evil, the displacement of labor, are the problems to which this work is directed. Endeavors to find similar employment for the labor so displaced, or, alternatively, to retrain and place such labor elsewhere are the main topics under discussion. The book also deals with the problem of financial hardship endured by a worker during the transitional period, as well as the services rendered to displaced workers by private and public agencies.

This is a lucid, detailed, straightforward, and well-written interim report of a novel attempt in the meatpacking industry by Armour & Company and two labor unions, the Amalgamated Meat Cutters and Butcher Workmen of North

America, and the United Packinghouse Food and Allied Workers, to solve the problem of large-scale labor displacement due to mechanization. The nature of the problem, and the manner in which labor and management dealt with it should be of interest to everyone, but most of all to those who are likely to be exposed to a similarly challenging situation. This work is an indispensable guide to such individuals insofar as it reveals the problems and pitfalls which are sure to attend any attempt to solve the problem of labor displacement.

In the fifteen years ending in mid-1965, twenty-one Armour plants employing about 14,000 workers had borne witness to the pervasive reorganization of the meatpacking industry (p. 6). Armour & Company agreed with the unions (which negotiate jointly) to the formation on September 1, 1959 of an Automation Fund to be administered by a committee of nine. Four representatives were from the company; four were from the unions. The impartial chairman was Professor Clark Kerr who wrote the *Foreword* to this report. In 1961, George P. Shultz, dean of the Graduate School of Business, University of Chicago, and co-author of this book, was made co-chairman. Professor Arnold R. Weber, also of the University of Chicago and co-author, worked in behalf of the committee. The functions of the committee as described in the 1961 Agreement (pp. 203-4) are: "to utilize the fund for the purpose of studying the problems resulting from the modernization program and making recommendations for their solution. . . ."

Armour & Company contributed $500,000 to the Fund, $140,000 of which still remained by the summer of 1965. The work of the committee was mainly concerned with labor displacement after the formation of the AFC and particularly with the closure of plants in Oklahoma City, Oklahoma; Fort Worth, Texas; Sioux City, Iowa; and Kansas City, Missouri. The problem facing the committee was further complicated by the fact that the skills of workers in the meatpacking industry are not transferable to other industries, that the median age of the workers ranged from 45 to 48, that most of them did not go beyond eighth grade in school and that their number included many Negroes and Latin Americans.

Chapter 2 contains a detailed and comprehensive account of the various measures for dealing with the effects of automation on the labor force which may be contained in a collective bargaining agreement. The next chapter discusses the sharing of job opportunities through a system of interplant transfers. Chapter 4 discusses at some length the role of financial cushions, especially severance pay and retirement income. Statistical tables are provided here. Chapter 5 discusses the role of the AFC in the placement campaigns. Of significance to future joint committees is the endeavor of the committee to expand the range of its placement activities by visiting prospective employers. "The response to these visits ranged from cordiality to outright hostility . . ." (p. 111). The longest and most informative part of the book is the sixth chapter which deals with experiments in retraining.

It is chimerical to hope that the incubus of unemployment due to automation will disappear without conscious attempts to provide for such unavoidable and even desirable eventualities. Chapter 7 is, therefore, devoted to a discussion of the aid which may be given by private agencies and by the federal

government under various Acts to those communities which are endeavoring to cope with a major displacement of labor. The last chapter of this edifying work gives a brief appraisal of the committee's endeavors and concludes with some valuable recommendations for labor market administrators. For those interested in pursuing this study in even greater detail, Appendix C contains a list of the publications arising out of the work of the AFC.

The workers assisted by the Automation Fund Committee, in this novel and highly laudable experiment, were small in number. Nevertheless, this report highlights many problems of certain future recurrence in such situations and so provides a basis for future policy. In this small project, the qualifications of the displaced workers were such as to render their re-employment exceedingly difficult. One wonders, therefore, if labor displaced in the future by the introduction of the techniques of "operations research" and "heuristic programming" will be absorbed with less difficulty. This study emphasizes the need for continuous cooperation between the communities involved and private and public agencies; it also foretells the increasing influence of the federal government in these affairs. Although not in every sense a complete success, the AFC, nevertheless—to use one of the authors' bromides—could not be expected to make a "silk purse out of a sow's ear."

JOHN C. O'BRIEN

Fresno State College

Labor Relations and Collective Bargaining. By BEVARS D. MABRY. New York: Ronald Press, 1966. Pp. vii, 475. $7.50; Cases, $3.00.

This is a relatively small, well-written textbook prepared for a one-semester course in collective bargaining. It assumes that the student has already had some training in labor economics. Moreover, since a large part of the book requires a good grasp of microeconomic and macroeconomic theory, the use of this text in its entirety is limited to graduate courses in economics or undergraduate students who have had work in price theory and aggregate economics.

The volume is divided into four parts. Part I, which consists of seven chapters, comprises nearly two-fifths of the book. It deals with the nature of labor relations and the roles of the participants. The author develops the idea that efforts by management and labor to achieve their respective goals sometimes leads to industrial conflict. The roles of workers, unions, and management are looked at from an interdisciplinary approach, particular emphasis being placed on sociological and psychological elements. The worker is considered within the framework of class structure and in terms of his attempt to achieve job security. The author suggests that it is logical to include professional and occupational associations (e.g., doctors, teachers) among labor organizations. The union is examined in connection with its structural and leadership problems. The relationship of the union to its membership, management, and the government is discussed. Then the changing role of management during the past few decades is indicated. Three chapters are devoted to a straightforward and factual presentation of federal and state legislation bearing on labor relations. The material relating to the states, however, is excessively detailed.

Part II, which has five chapters, represents the major raison d'être for the text. The theory of collective bargaining is explored with the basic tools of economic analysis. After expounding the customary theory of collective bargaining—a case of bilateral monopoly and the consequent indeterminacy of the wage settlement—the author sets forth his own complex theory based on concepts of the net gains of the parties. The less developed bargaining models worked out by Neil W. Chamberlain, Jan Pen, and Carl Stevens are discussed and compared. Two chapters are devoted to the economic effects of bargaining. The theoretical implications of the effects of increases in wages on the position of the firm and the economy are delineated. Empirical research and data in these areas are carefully reviewed to determine their relevance and consistency. The attempt to show the linkage between theory and empirical studies is particularly valuable for students.

The last two sections of the book are primarily descriptive. Part III contains three chapters on the techniques of collective bargaining. These include a chapter on the strategy and tactics of negotiations that might serve as a manual for participants in the process, another on the content of the contract, and the third on administering the agreement. Part IV on labor disputes includes four chapters. The first, dealing with the weapons available to the disputants, needs strengthening. The relative effectiveness of the weapons is not indicated, nor are they all included (e.g., whipsawing). The next two chapters cover the procedures used in emergency disputes by the state and federal governments and mediation processes. The final chapter is an unduly lengthy and general discussion of ethics, containing only a few remarks relevant to labor relations.

Each chapter ends with a conclusion. This section ordinarily summarizes the material in the chapter, although regrettably in a number of cases new facts and ideas are introduced for the first time. This is especially true in those chapters dealing with state laws, such as 7 and 17. Every chapter is followed by a group of approximately eight discussion questions and a short selected bibliography of books and government documents.

The book is written at several distinct levels. Five chapters (8 through 12) can best be understood by students well versed in economic theory; eight chapters (5-7 and 14-18) are basically descriptive; five chapters (1-4 and 19) use an interdisciplinary analysis; and one chapter (13) seems more suited as a set of reference materials for negotiators than for students using a text in collective bargaining. For those students not well trained in economics, only the first four chapters of the book are likely to be appropriate; the material in the remainder is either too difficult or more fully discussed in many of the basic texts on labor economics.

The book will be useful in many advanced courses in labor economics or collective bargaining. Nevertheless, it requires some supplementation to cover the field of labor relations and collective bargaining. Despite its 1966 publication date, the statistical material rarely goes beyond 1963. A variety of relevant and important subjects are omitted or inadequately discussed, including the special problems involved in collective bargaining for the very large group of government employees, the impact of Title VII of the Civil Rights Act of

1964 (Equal Employment Opportunity Act) as it affects admission practices of unions, and the meaning and importance of the bargaining unit and its relation to the union. Recent and current bargaining issues between management and unions, such as tripartite bargaining and growth in fringe benefits of different kinds, are not included.

The author has also written a brief companion book to the text, consisting of twenty-four actual cases involving a variety of labor problems. Each case is followed by a series of discussion questions.

ROBERT D. LEITER

City College of the City University of New York

The Measurement and Interpretation of Job Vacancies. NBER Conference Report. New York and London: Nat. Bur. of Econ. Research, 1966. Pp. ix, 593. $12.50.

The papers appearing in this NBER volume were given at a conference designed to present a "state of the arts" picture of a statistic which has evoked considerable interest in recent years—job vacancies. This interest derives from the fact that a measure of unfilled jobs is necessary in obtaining a complete estimate of labor demand. The material is organized around four basic themes. Part I considers some of the theoretical and operational uses to which job vacancy data may be put. The experiences with this information in several foreign countries are treated in Part II. In Part III aspects of several recent surveys (some still in process) undertaken in the United States are evaluated, and in Part IV three possible alternatives to job vacancy data are evaluated.

While the central theme of this volume is a single statistic, in treating it many of the papers deal in considerable detail with the overall functioning of labor markets. Thus, as has been the case with several recent NBER conference volumes, this publication will be of interest and use to all students of labor economics—both theoretical and institutional. For example: Dunlop outlines a theory of labor hiring which places considerable emphasis on the "internal labor market," that is, movement up and down the skill ladder within the firm. A prevalence of this kind of market limits the usefulness of job vacancy information since, in the extreme, there is only one "port of entry" for the unemployed—at the low skill job. It is interesting to note the similarity between the theoretical scheme of John Dunlop and the Nenkoh system (lifetime commitment) in Japan, as described in the paper by G. Somers and M. Tsuda. The latter suggest that under the pressure of very tight labor-market conditions, the increasing premium placed on labor mobility has tended to dilute the Nenkoh system. The Japanese experience suggests that Dunlop's model should not be adopted as a general description of the U. S. labor market. The fine paper by C. Holt and M. David presents an exploratory general demand-supply model of the labor market which yields a wealth of empirical implications, and should provide many students food for thought and research. Of particular interest will be the way in which the "Phillips hypothesis" falls out of their model. In much of the published empirical work on the wage-unemployment relationship, the theoretical basis has been either omitted or presented without much rigor.

In addition to the many theoretical insights contained in these conference proceedings, several institutional aspects of the labor market are treated competently. Among these are the private employment agency and the temporary help industry. Although these two institutions seem to offer little in the way of substitute information on job vacancies, the authors have provided a useful service in simply describing and analyzing some aspects of these infrequently treated areas.

On the major questions of the usefulness and feasibility of collecting job vacancy statistics, a reading of this volume raises several troublesome issues. It is not at all clear that the same set of job vacancy statistics can be utilized optimally for both employment service activity and aggregate economic analysis. The former purpose requires a fairly detailed breakdown in terms of job content and location, while the information necessary to implement the Holt-David type model need not be nearly so detailed. A more important distinction for the two purposes is in the definition of a job vacancy itself. Information on anticipated vacancies would seem to be highly relevant to the employment service officer concerned with educational policies and advising. Should temporary jobs be included in the vacancy list? The answer would be yes as far as employment service needs are concerned. From the point of view of aggregative analysis, however, it appears that the notion of a job vacancy is every bit as troublesome as that of its counterpart on the supply side, the unemployed worker.

Even if satisfactory definitions for both purposes could be agreed upon, there would still remain the problem of obtaining the information from employers. The general tone of the papers presented in Part III—"Job Vacancy Surveys in the United States"—is cautiously optimistic on this point. However, several discussants do not share this optimism. (The outspoken dissent of some of the discussants enlivens this volume a good bit.) Differences in employer hiring policies, hoarding of skilled workers, employer pessimism or optimism concerning future availability of labor are some of the factors mentioned in casting doubt on the ability to gather meaningful estimates of job vacancies. The fact that wage rates are not attached to the vacancy estimates thus far obtained presents an additional problem of interpretation.

The general conclusions obtained in Part II, dealing with foreign experiences, cause this reviewer, also, to view somewhat skeptically the reliability and usefulness of job vacancies as a major statistic in our present manpower program. An economy operating with unemployment close to 4 per cent is not apt to create the pressing need for detailed job vacancy information, the kind of information which would be so useful under conditions of very tight labor markets. It is not just coincidence that the demand for job vacancy data arose during the recent structuralist-aggregate demand controversy. Since this controversy is subsiding somewhat, it would be well to reconsider the need for an additional aggregate statistic—particularly since the opportunity cost of obtaining this information is greater than zero.

DAVID E. KAUN

University of California, Santa Cruz

Population; Welfare Programs; Consumer Economics

Poverty in America. Edited by Louis A. Ferman, Joyce L. Kornbluh, and Alan Haber. Ann Arbor: University of Michigan Press, 1965. Pp. xxv, 532. $9.00.

Poverty in America. Edited by Margaret S. Gordon. Proceedings of a National Conference held at the University of California, Berkeley, February 26-28, 1965. San Francisco, California: Chandler Publishing Company, 1965. Pp. xxiv, 465. $5.00; paper $2.50.

So many books on poverty have appeared in the last couple of years that the publishers have run out of titles. Here are two substantial compendia with the same title, *Poverty in America.*

The Ferman book is an anthology, containing more than forty recent contributions to the literature of poverty, culled from books, magazines, and scholarly journals. It includes the now-classic *New Yorker* article by Dwight Macdonald, "Our Invisible Poor," Mollie Orshansky's statistical tour de force, "Counting the Poor," a fascinating article by Hylan Lewis, "Child Rearing in Low-Income Families," the pronouncements of the Ad Hoc Committee on the Triple Revolution, and a potpourri of other items. There is a brief introduction by Michael Harrington, as well as several bits of commentary from the editors.

This reviewer found the editorial comments far more irritating than enlightening. Ferman and his co-editors apparently hold a conspiracy view of poverty (sick society, power elite, the rich conspiring to profit from the misery of the poor, and so forth). They take an alarmist view of automation, and have no fear of the undocumented assertion. Their selection of articles, however, is excellent—balanced, comprehensive, and interesting.

The tone of the Gordon book is far more restrained. The only editorial comment is a lucid foreword by Lloyd Ulman which points out, among other things, that most economists view technological change as "a boon rather than a bugbear." The book consists of thirty papers presented at a conference on poverty held at Berkeley in 1965. It was a better-than-average conference and most of the contributions are worth reading. The professional economist will find more to chew on here than in the Ferman book (especially in Hyman P. Minsky's article on "The Role of Employment Policy," and Fritz Machlup's "Strategies in the War on Poverty"). The general reader, however, might get more ideas from the Ferman collection.

The spectrum of views represented in the two collections is actually quite similar and some of the poverty regulars (Robert Lampman, Sar Levitan, Frank Reissman, Michael Harrington) are represented in both. Taken together, these two books provide a good survey of what America academicians and government officials are thinking and writing about poverty and how to eliminate it. What does it all add up to? Is there a consensus—either on cause or cure?

We have clearly amassed a great many statistics about the poor. We have

detailed counts of people falling below a "poverty line" defined in terms of income and family size. We know how many of the poor are black and white, old and young, rural and urban, employed and unemployed. We know something about how the numbers and characteristics of the poor change over time given various methods of redefining the poverty line. However, judging from the evidence presented here, we know almost nothing about specific individuals and families who fall into poverty or move out of it. How many of those who were poor five years ago are still poor and what happened to the rest? Our statistics do not tell us. The persistent moan of the social statistician—too many cross-sections, no longitudinal studies—echoes through both collections.

Perhaps surprisingly, the books reflect little disagreement on public policy to reduce poverty, only differences of priority or emphasis. Minsky and others would put fiscal policy to ensure full employment at the top of their list. Frederick Harbison and others emphasize training and human investment programs. But everybody really wants both. Even the prophets of the Triple Revolution—for all their startling predictions—are in favor of education, public works, and other less-than-startling public policies.

The reader of these volumes is also impressed by the number and variety of public programs to combat poverty which are already on the books. He is provided with some descriptions of these programs and with a few poignant examples of their failures. The best-written article in the Ferman book is Mary Wright's, "The Dusty Outskirts of Hope," which describes the experiences of a proud, shy man seeking help in a busy, over-organized welfare office. It is followed by Charles Lebeaux's "Life on ADC: Budgets of Despair." There are no descriptions of successes, no analyses of programs that have been effective in moving people out of poverty. We know that poverty is declining, albeit slowly. We have a variety of public programs to hasten that decline. But we lack any real analysis of the effectiveness of these programs on which to base decisions about future emphasis in public policy. These books reflect this lack of analysis.

ALICE M. RIVLIN

Department of Health, Education, and Welfare

Poverty Amid Affluence. Edited by LEO FISHMAN. New Haven and London: Yale University Press, 1966. Pp. xii, 246. $6.00; paper, $1.75.

Poverty Amid Affluence. By OSCAR ORNATI, with J. S. SWEET. New York: Twentieth Century Fund, 1966. Pp. xiii, 208. Paper, $3.25.

Two books with precisely the same title published at the very same time are likely to be much alike. These, however, could have been very different, for one presents the results of an integrated series of research projects five years in the doing, while the other is a collection of conference papers, most of which were written, it would seem, in no more than five days. The two are, in fact, very much alike: both are comprehensive, systematic, and technically unpretentious. Their comparison yields another surprise: apparently a skillful conference convener can, undoubtedly with much luck, assemble a group of

experts who, with only slight duplication, can more competently survey an issue than a single author.

Ornati's book, as he reminds the reader whenever some utterly mundane point is to be made (which is often), reports the results of a continuing research project at the New School. The report is in four parts. The three chapters of the first Part define the poor and estimate their numbers now and in the past. In the nine chapters of Part Two, the socioeconomic attributes which characterize the poor are catalogued. Part Three is devoted to examining actual and proposed antipoverty programs. These twenty-two chapters, many of which are too long, average just under six pages each. Finally the basic data and the methodology of the study are presented in thirty-five Appendices.

The Fishman volume consists of a set of invited papers presented at West Virginia University in May, 1965. It, too, is in four parts the first of which is devoted to definitions. Selected psychological and sociological consequences of being poor are taken up in the second Part. Part Three examines three poverty related attributes of people and areas, while the concluding section is devoted to policy.

The first two chapters are far and away the best of Ornati's book. Their essence is an examination over five decades of the elevating definitions of poverty that can be derived from an enormous time series of worker family budgets. Having learned from this exercise that poverty is relative in time and place and that, by the definitions of each period, poverty in the past will afflict fewer people (and just about the same proportion of people in each period too) than if any later definition is employed persistently through time, the message is promptly forgotten for the rest of the book. All of the second and third sections proceed from the belief that poverty today is different from poverty in the past, because it is now not a general affliction but is confined to particular groups. The crucial issue to Ornati is "whether a self-professing 'open society' can tolerate, not the existence of income differentials, but the existence of income differentials on the basis of social and demographic 'classes'" (p. 124). The issue, posed this way, is as phony as an Army General opposing socialized medicine. For if poverty is always defined by contemporaries to embody a virtually fixed proportion of the lower income tail, then surely it has embraced virtually the same group (except for the immigrants) since at least 1860, although their proportions vary with the age and race profile of the nation and the business cycle. This point comes through very clearly in Oscar Handlin's useful essay in the Fishman volume. The literature on poverty is widening but not deepening and perhaps this excessive taxonomy of the poor is the reason, for as Harry Johnson put it, "preoccupation with the categories of poverty can obscure or prevent understanding of the broader economics of the poverty problem and of the economic strategy required for the war on poverty" (Fishman, p. 183).

Only a short comment on the eleven papers of the Fishman volume is feasible. They are mainly descriptive; by and large each author relied heavily on his prior publications, and since they were written to be read, they are clear and pointed. Two of the papers are especially good for their genre and both

are by Robert Lampman. When a man has written so often about "Population Change and Poverty Reduction, 1947-1975" and "Ends and Means in the War Against Poverty," the probability of a flat, stale rehash is high, but in these two essays Lampman is really in tip-top form.

Putting things in better perspective, both books have some things to recommend them, but both are highly uneven. Ornati has a good discussion of the measurement problem, and many data appendices that will be useful for undergraduate term papers. The Fishman volume has good coverage with superior articles by Handlin and Lampman, and interesting articles by Alexander and Dorothea Leighton, Theodore Schultz and Harry Johnson, the main ideas of which have already been published elsewhere. For general classroom use, most instructors will prefer one or another of the many collections of readings which are available.

EUGENE SMOLENSKY

University of Chicago

Private Pensions and Individual Saving. By GEORGE KATONA. Ann Arbor: University of Michigan Survey Research Center, 1965. Pp. v, 114. $1.50.

This monograph may well prove to be one of the most significant research reports in many years.

Armed with the facilities of the Survey Research Center at the University of Michigan and a grant from the Social Security Administration, Dr. Katona has undertaken to test the widely held premise that the spread of collective retirement security arrangements must adversely influence individual incentive to save. He felt justified in the challenge by two theories pointing in the opposite direction: "Aspirations may grow with achievement" and "Effort is intensified the closer one is to one's goal." During 1962 and 1963 his surveyors interviewed about 5,000 family units carefully selected to represent (in age distribution, income distribution, location, occupation, and education) the entire labor force. He decided to exclude the responses where the family head was already retired or a housewife or a student or under 35 or single, or where the family income was under $3,000, on the ground that such people's retirement expectations, if any, would be atypical. There remained a crucial group of 1,853 respondents who were found to be representative, in the above mentioned respects, of about half the labor force.

Direct questions disclosed that members of this group were overwhelmingly optimistic about retirement. They were well informed about what Social Security benefits they could expect, and the great majority were confident that their retirement incomes would be adequate to meet their needs. Many expected to continue earning some income by part-time employment or self employment after retirement. Slightly fewer than one-half were participants in private pension plans, and relatively few of these were clearly aware of any impact of their pension plan participation on their spending and saving behavior.

Recognizing that direct questions often yield inaccurate information in such sensitive areas as saving behavior and expectations, the surveyors used the in-

direct method by asking questions with no apparent bearing on the central issue which produced answers that could by analysis be made to yield the desired information with a high degree of reliability. From this information there were formulated four independent variables (participation in private pension plans, expected retirement income high in relation to current income, expected retirement income greater than expected need, expected retirement income "enough") and three dependent variables (proportion of income saved in the past year greater than 5 per cent of current income, saving "behavior," and saving "mindedness"). The complex relations between these variables were examined through cross-tabulation and through multivariate analysis by digital computer calculation of regression equations. Both methods revealed that participants in private pension plans were *greater* savers—with respect to both past saving and expected future saving—than nonparticipants.

Further analysis indicated that this conclusion was unaffected by other variables such as differences of liquid asset holding, of expenditures on consumer durables, of education, and of home ownership. In addition, analysis of the relationships between the variables by a new computer technique called the Automatic Interaction Detector Program confirmed the conclusion.

The neatness and orderliness of the project are slightly flawed by some vagueness about the concept of saving. Respondents were asked, not to subtract their consumption expenditures from their incomes, but to indicate their "discretionary saving" which was defined as "net change in various forms of deposits with banks and savings institutions as well as stocks and bonds"; that is, saving was conceived as the *application* of saved funds to increase of financial asset holdings. The saving component of life insurance premiums, however, and the repayment of debt (even on homes) were excluded on the ground that they were "contractual" and not "spontaneous"; and the definition wandered over to the *source* side in announcing that "we neglect incurrence of debt because it is usually motivated by expenditure considerations (e.g., the desire to purchase a house or durable goods) rather than by saving considerations." These imperfections do not seriously compromise the study, of course, and certainly it would be unreasonable to expect a very sophisticated concept of saving in a survey of this type.

A more fundamental reservation concerns the likelihood of change in the outlook of middle-aged people generally. At present their retirement expectations are based mainly on promises. Later on, if they actually see large numbers of their older relatives and friends enjoying comfortable and worry-free retirement, their attitudes might become significantly different. That Katona is not unaware of such possibilities is indicated by several notes of caution about drawing inferences from his central conclusion. He says at the very outset, "It will be several years before a sufficient number of people covered by private pension plans will have retired so that there will be a pronounced improvement in the standard of living of the average retired person." At another point he reminds the reader that the effectiveness of the principle that achievement raises aspirations depends on prevailing conditions. "Aspirations for higher income and for an improved standard of living are raised when people are optimistic and confident rather than pessimistic or uncertain in their view

of the future." Finally, "Circumstances may change, and habituation to pension plans may change and therefore saving behavior. The studies reported in this monograph need to be repeated later."

This reviewer would add that the present report constitutes an excellent foundation for the later studies.

ROBERT W. MAYER

University of Illinois

Population in History: Essays in Historical Demography. Edited by D. V. GLASS AND D. E. C. EVERSLEY. Chicago: Aldine, 1965. Pp. xii, 692. $17.50.

Historical demography, the field represented by this volume, is of interest to economists working on population, labor, and economic history, and also, currently, to growth economists for the light it may shed on demographic aspects of historical economic development. In the past two decades work in historical demography has enjoyed something of a revival. The present volume, edited by David V. Glass, a demographer-sociologist, and D. E. C. Eversley, an economic historian, provides a valuable picture of the recent evolution of this field and a cross-section of representative research approaches and results. Of the 27 contributions to the volume, seven are unabridged reprints of earlier papers; the remaining ones are either entirely new or represent revisions of work published elsewhere. The contributors are divided almost evenly between the fields represented by the two editors; there is no one who holds an economics appointment.

The title of the volume somewhat misleadingly suggests an encyclopedic scope. Actually, the primary focus in space and time, presumably reflecting the emphasis of recent research, is selected populations of Western Europe in the 18th century. Studies of small local areas are especially favored. In content the papers range from questions of sources and methodology to substantive observations on demographic experience and attempts to assess the causes and, to a very limited extent, effects of demographic change. A truly outstanding contribution is a previously unpublished piece by J. Hajnal, placing Western European marriage patterns since the Middle Ages in broad spatial and temporal perspective and speculating on their interrelations with demographic and economic phenomena. In addition to an introductory chapter by each editor, other valuable contributions include papers relating to all or parts of France by Louis Henry, Pierre Goubert, J. Bourgeois-Pichat, and J. Meuvret; Scandinavia by G. Utterström; Finland by Eino Jutikkala; Germany by W. Koellmann; and Flanders by P. Deprez. Much of this represents reports on research not previously published or available in English. Economists working in the field of economic history will recognize well-known pieces (though in a few cases slightly revised) by K. F. Helleiner, H. J. Habakkuk, K. H. Connell, J. D. Chambers, D. E. C. Eversley and T. H. Marshall, and may profit from the skeptical view taken by the editors of the recent emphasis in some of this literature on fertility increase as the main factor in increased British population growth in the 18th century. Also noteworthy, though available elsewhere, are studies of the demography of British ducal families by T. H. Hollings-

worth and of Europe's ruling families by S. Peller, and one on medical evidence related to English population changes in the 18th century by Thomas McKeown and R. G. Brown.

With regard to substantive results, the primary yield is on facts of demographic experience; attempts to relate this experience to economic and other determinants are, on the whole, much less satisfying. But the facts are tantalizing and provocative ones. Although the evidence is fragmentary, the papers rather consistently point to the conclusion that by the end of the 18th century, Western European demographic conditions were quite distinctive. As Hajnal points out, "all that is pre-industrial, including eighteenth-century [Western] Europe, is often lumped together in generalizations about 'agricultural' or 'peasant' or 'underdeveloped' societies" (p. 131). Yet it appears that compared with contemporary Eastern Europe or pre-World War II underdeveloped economies, populations in Western Europe married noticeably later and a larger percentage remained single, life expectancy was noticeably longer, and fertility lower. Was there a time in Western Europe when "peasant society" demographic conditions equivalent to those in the other areas prevailed? Perhaps the fifteenth century with regard to marriage pattern; perhaps the seventeenth with regard to mortality; the picture for fertility is obscure. In any event, it would seem that in most of this area pronounced (if uneven) changes in demographic conditions occurred well before the periods commonly associated with modern economic growth. People were living longer, with more years of adulthood free from family and childcare responsibilities. Why? What bearing did this have on the current and subsequent process of economic development? These are some of the fascinating and perplexing questions raised by these essays in historical demography, questions to which economics along with the disciplines represented in the volume should be able to make significant contributions.

RICHARD A. EASTERLIN

University of Pennsylvania

TITLES OF NEW BOOKS

General Economics; Methodology

ANDERSON, J. E. Politics and the economy. Boston and Toronto: Little, Brown and Co., 1966. Pp. xi, 386. Paper, $2.95.

BJERVE, P. J. Teknisk revolusjon i økonomisk analyse og politikk? (Technical revolution in economic analysis and policy?) With Engl. summary. Artikler no. 18. Oslo: Central Bur. of Stat., 1966. Pp. 23. NKr. 4.

CLARK, C. An incomes policy. Aims of industry study no. 12. London: Aims of Industry, 1966. Pp. 8. 2s. 6d.

HADAR, J. Elementary theory of economic behavior. Reading, Mass. and London: Addison-Wesley, 1966. Pp. ix, 332. $8.50.

HAILSTONES, T. J., MARTIN, B. L. AND WING, G. A. Contemporary economic problems and issues. Cincinnati: Southwestern Pub. Co., 1966. Pp. iv, 412.

HAVENS, R. M., HENDERSON, J. S. AND CRAMER, D. L. Workbook and study guide to accompany *Economics: principles of income, prices, and growth.* New York: Macmillan; London: Collier-Macmillan, 1966. Pp. vii, 173. $3.25.

HEILBRONER, R. L. The limits of American capitalism. New York: Harper & Row, 1965. Pp. 148. $4.95.

KRUPP, S. R., ed. The structure of economic science—essays on methodology. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. vi, 282. $7.95.

RENNER, R., SACHS, R. AND FOSBERRY, J. German-English economic terminology—systematic and alphabetical vocabulary with translation exercises. Munich: Max Hueber; New York: St. Martin's Press, distrib., 1965. Pp. 556. $12.50.

SCHULMANN, M. M. Clouds over Wall Street. New York: Exposition Press, 1966. Pp 242. $4.

SCHULTZ, G. P. AND ALIBER, R. Z., eds. Guidelines, informal controls, and the market place—policy choices in a full employment economy. Chicago and London: Univ. of Chicago Press, 1966. Pp. xi, 357. $5; paper, $1.95.

UHR, C. G. Economics in brief. New York: Random House, 1966. Pp. vii, 149. Paper, $1.95.

WEBER, W. Einheit und Vielfalt in den Sozialwissenschaften. Festschrift für Alexander Mahr. New York and Vienna: Springer, 1966. Pp. vi, 382. $9.05.

Anticipating the nation's needs for economic knowledge. 46th annual report, June 1966. New York: Nat. Bur. Econ. Research, 1966. Pp. x, 92. Single copies free.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

ABRAHAM, C. AND THOMAS, A. Microéconomie—décisions optimales dans l'entreprise et dans la nation. Stat. et programmes écon. vol. 11. Paris: Dunod, 1966. Pp. xx, 460. F 68.

BROCKHOFF, K. Unternehmenswachstum und Sortimentsänderungen. Schriften des Inst. für Gesellschafts- und Wirtschaftswissenschaften der Univ. Bonn vol. 5. Cologne: Westdeutscher, 1966. Pp. 173. DM 29.50.

CHAND, M Elasticity in economics. Univ. of Allahabad stud. 1965. Allahabad, India: Univ. of Allahabad, Econ. Section, 1965. Pp. 61.

DANØ, S. Industrial production models—a theoretical study. New York and Vienna: Springer, 1966. Pp. viii, 220. $9.75.

DUE, J. F. AND CLOWER, R. W. Intermediate economic analysis—resource allocation, factor pricing, and welfare. 5th ed. Homewood, Ill.: Irwin, 1966. Pp. xiii, 481. $8. First pub. in 1947.

FAU, J. Le processus productif national—essai sur la nature du phénomène économique de la production. Paris: Cujas, 1966. Pp. viii, 207.

FERGUSON, C. E. Microeconomic theory. Homewood, Ill.: Irwin, 1966. Pp. xiv, 439. $8.

FUSFELD, D. R. The age of the economist—the development of modern economic thought. Glenview, Ill.: Scott, Foresman and Co., 1966. Pp. 147. $3.50.

GISSER, M. Introduction to price theory. Scranton, Penn.: Internat. Textbook Co., 1966. Pp. ix, 325. $7.50.

HAVEMAN, R. H. AND KNOPF, K. A. The market system. New York and London: Wiley, 1966. Pp. xiv, 223. Paper, $1.95.

HEINEMANN, K. Externe Effekte der Produktion und ihre Bedeutung für die Wirtschaftspolitik. Volkswirtschaftliche Schriften no. 99. Berlin: Duncker & Humblot, 1966. Pp. 149. DM 28.

JAMES, P., ed. The travel diaries of Thomas Robert Malthus. New York and London: Cambridge Univ. Press, 1966. Pp. xvi, 316. $8.50.

JOSSA, B. Analisi economica del progresso tecnico—primi elementi di una teoria neoclassica. Milan: A. Giuffrè, 1966. Pp. iv, 392. L. 3.200.

KLEIN, L. R. The Keynesian revolution. 2nd ed. New York: Macmillan: London: Collier-Macmillan, 1966. Pp. xi, 288. Paper, $2.95. First pub. in 1947.

LYONS, I. L. AND ZYMELMAN, M. Economic analysis of the firm—theory and practice. New York and London: Pitman, 1966. Pp. xvi, 422. $8.75.

MAMALAKIS, M. La teoría de los choques entre sectores. Pub. no. 83. Santiago: Inst. de Econ., Universidad de Chile, 1966. Pp. 62.

MONTEIL, J. Les théories des surplus. Sér. analyse écon. no. 6. Paris: Gauthier-Villars, 1966. Pp. 178. F 32.

SAMUELS, W. J. The classical theory of economic policy. Cleveland: World Pub. Co., 1966. Pp. xvii, 341.

TRUCHET, J. Politique de Bossuet. Paris: Armand Colin, 1966. Pp. 317.

VON BÖVENTER, E. Théorie de l'équilibre en économie spatiale. Sér. analyse écon. no. 7. Paris: Gauthier-Villars, 1966. Pp. xv, 190. F 45.

VON THÜNEN, J. H. Von Thünen's isolated state. Transl. from German by C. M. Wartenberg. New York and Oxford. Pergamon Press, 1966. Pp. lv, 304. $15.

Economic History; Economic Development; National Economies

ALEXANDER, Y. International technical assistance experts. A case study of the U.N. experience. New York and London: Praeger, 1966. Pp. xx, 223. $15.

ALLOCATI, A., ed. Tipiche operazioni del Banco della Pietà in alcuni atti notarili dei secoli XVI-XIX. Stud. no. 10. Naples: Edizioni Scientifiche Italiane, 1966. Pp. 221, L. 2.500.

ANTONIASDIS-BIBCOU, H. Etudes d'histoire maritime de Byzance—a propos du "thème des Caravisiens." Paris: S.E.V.P.E.N., 1966. Pp. 220.

BALDWIN, R. E. Economic development and export growth—a study of Northern Rhodesia, 1920-1960. Berkeley: Univ. of California Press, 1966. Pp. xviii, 254. $6.

BHATT, V. V. AND KAHN, N. A. A framework for planned economic development of Lower Mekong Basin countries. Bangkok: Asian Inst. Econ. Develop. and Planning, 1966. Pp. v, 43.

BIRMINGHAM, W., NEUSTADT, I. AND OMABOE, E. N., eds. A study of contemporary Ghana—Vol. I, The economy of Ghana. Evanston, Ill.: Northwestern Univ. Press, 1966. Pp. 472. $12.50.

BRAND, D. D. Mexico—land of sunshine and shadow. New York and London: Van Nostrand, 1966. Pp. 159. Paper, $1.75.

CARR, R. Spain 1808-1939. London: Oxford Univ. Press, 1966. Pp. xxix, 766. $12.50.

CLARK, J. G. The grain trade in the old Northwest. Urbana and London: Univ. of Illinois Press, 1966. Pp. xi, 324, $7.50.

CLARK, P. G. Development planning in East Africa. East African stud. no. 21. Nairobi: East African Pub. House, 1965. Pp. 154. $3.

CLARKE, W. M. Private enterprise in developing countries. New York and London: Pergamon Press, 1966. Pp. vii, 59. Paper, $1.50.

DEMARCO, D. La Calabria: economia e società. Reggio dinanzi alla peste del 1743. Gli uomini e la terra dopo l'Unità. Stud. no. 9. Naples: Edizioni Scientifiche Italiane, 1966. Pp. 105. L. 1.200.

DONALDSON, L. Development planning in Ireland. New York and London: Praeger, 1966. Pp. 156. $12.50.

DROHAN, N. T. AND DAY, J. H., eds. Readings in Australia economics. London: Cassell, 1965. Pp. xvii, 328.

DRUMMOND, I. M. The Canadian economy—organization and development. Homewood, Ill.: Irwin, 1966. Pp. ix, 146. $2.50.

FLINN, M. W. The origins of the Industrial Revolution. New York: Barnes & Noble, 1966. Pp. x, 114. Paper, $2.

FUÀ, G. Notes on Italian economic growth, 1861-1964. Milan: Giuffrè, 1966. Pp. 89.

FURTADO, C. Développement et sous-développement. Paris: Presses Univ. de France, 1966. Pp. xi, 226. F 15.

HACKETT, J. AND A.-M. L'économie britannique—problèmes et perspectives. Cahiers de la Fondation Nat. des Sci. Pol. no. 147. Paris: Librairie Armand Colin, 1966. Pp. x, 219. F 32.

HARRIS, R. C. The seigneurial system in early Canada—a geographical study. Madison: Univ. of Wisconsin Press; Quebec: Presses de l'Univ. Laval, 1966. Pp. xi, 247. $10.

HARRISON, B. South-east Asia—a short history. 3rd ed. New York: St. Martin's Press; London: Collier-Macmillan, 1966. Pp. xi, 278. $4.25. First pub. in 1963.

HAYES, S. P., JR. Evaluating development projects—a manual for the use of field workers. 2nd ed. New York UNESCO, 1966. Pp. 116. $2.50. First pub. in 1959.

HENDERSON, P. D., ed. Economic growth in Britain. New York: Humanities Press, 1966. Pp. 296. $6.75.

Contributions by W. Beckerman, D. C. Foster, R. G. Opie, M. F. G. Scott, P. Streeten and J. Vaizey.

HENDERSON, W. O. Britain and industrial Europe 1750-1870—studies in British influence on the Industrial Revolution in Western Europe. 2nd ed. New York: Humanities Press, 1965. Pp. vi, 267. $6. First pub. in 1954 by Liverpool Univ. Press.

JACOBS, N. The sociology of development—Iran as an Asian case study. New York and London: Praeger, 1966. Pp. viii, 541. $17.50.

KOH, S.-J. Stages of industrial development in Asia—a comparative history of the cotton industry in Japan, India, China, and Korea. Philadelphia: Univ. of Pennsylvania Press 1966. Pp. 461.

KÖBER, K.-O. Ordnungspolitische Probleme der spanischen Wirtschaftspolitik. FIW-Schriftenreihe no. 28. Cologne: Carl Heymanns, 1965. Pp. 166. DM 16.50.

LE GRIS, C. L'entrée du Canada sur la scène internationale (1919-1927). Paris: Presses Univ. de France, 1966. Pp. xi, 93. F 6.

MCGANN, T. F. Argentina—the divided land. New York and London: Van Nostrand, 1966. Pp. 127. Paper, $1.45.

NYSTROM, J. W. AND HAVERSTOCK, N. A. The Alliance for Progress—key to Latin America's development. New York and London: Van Nostrand, 1966. Pp. 126. Paper, $1.45.

POPESCU, O. Periodization in the history of economic thought. *Internat. Soc. Sci. Jour.*, vol. 27, Paris: N.p., 1965. Pp. 607-34.

RAYNER, A. C. AND LITTLE, I. M. D. Higgledy piggledy growth again: an investigation of the predictability of company earnings and dividends in the U. K. 1951-1961. Oxford: Blackwell, 1966. Pp. vii, 111.

SILVA, J. G. DA. En Espagne: developpement économique, subsistance, déclin. Paris: Mouton, 1965. Pp. 219.

SLICHTER, S. H. Economic growth in the United States— its history, problems and prospects. New York: Free Press; London: Collier-Macmillan, 1966. Pp. 189. Paper, $1.95. First pub. in 1961 by Louisiana State Univ. Press. Reviewed in 1962 by M. Bronfenbrenner.

VANDEWALLE, G. De conjuncturele evolutie in Kongo en Ruanda-Urundi van 1920 tot 1939 en van 1949 tot 1958. (The course of economic activity in the Congo and Ruanda-Urundi, 1920-1939 and 1949-1958.) Antwerp: Standaard Wetenschappelijke Uitgeverij, 1966. Pp. xii, 329. Bfr. 395.

WILSON, G. W., BERGMANN, B. R., HIRSCH, L. V. AND KLEIN, M. S. The impact of highway investment on development. Washington: Brookings Inst., 1966. Pp. xii, 226. $6.

Annali . . . Istituto di Storia Economica e Sociale no. 5. Naples: Edizioni Scientifiche Italiane, 1964. Pp. 489. L. 5.500.

Contributions by E. Coornaert, G. Mira, C. Trasselli, P. Giniewski, P. Guichonnet, D. Demarco and J. Zawadzki.

Choice of technologies in the Latin American textile industry. U.N. Econ. Comm. for Latin Am. New York: United Nations Econ. and Soc. Council, 1966. Pp. 69.

Design for a worldwide study of regional development: a report to the United Nations on a proposed research-training program. Resources for the Future staff stud. Baltimore: Johns Hopkins Press, 1966. Pp. x, 82. $1.50

The economic development of Morocco. Baltimore: John Hopkins Press for Internat. Bank for Reconstruction and Develop., 1966. Pp. xiv, 356. $8.50.

Economic report on 1965. H.M. Treasury. London: H.M. Stationery Office, 1966. Pp. 38. Paper, $1.

The Lothians regional survey and plan—Vol. I, Economic and social aspects; Vol. II, Physical planning aspects. Edinburgh: H.M. Stationery Office, 1966. Pp. xvi, 289; xxii, 359. 75s. each.

National directory of Latin Americanists—biobibliographies of 1,884 specialists in the social sciences & humanities. Hispanic Foundation bibliog. ser. no. 10. Washington: Supt. Docs., 1966. Pp. iii, 351. $2.

The process of industrialization in Latin America. Stat. annex submitted by Secretariat of the Econ. Comm. for Latin America. New York: United Nations, 1966. Pp. 189.

Le prospettive dell'industria italiana nel quadriennio 1966-69. Collana di studi e doc. no. 13. Rome: Confederazione Generale dell'Industria Italiana, 1966. Pp. 637. L. 5.000.

La tradition et le développement économique dans l'Asie du Sud-est. Brussels: Univ. Libre de Bruxelles Inst. de Soc., 1964. Pp. 208.

Statistical Methods; Econometrics; Social Accounting

ALLEN, R. G. D. Mathematical analysis for economists. New York: St. Martin's Press, 1966. Pp. xi, 548. $3.50. First ed. 1938.

BACKMAN, J. AND GAINSBRUCH, M. R. Inflation and the price indexes—materials submitted to the Subcom. on Econ. stat., Joint Econ. Com., 89th Congress, 2nd Sess. Washington: Supt. Docs., 1966. Pp. 129.

BRODERSOHN, M. A multiregional input-output analysis of the Argentine economy. Buenos Aires: Inst. Torcuato di Tella, 1965. Pp. 179. Limited distribution.

BURLEY, H. T. Growth rate tables. Occas. paper no. 10. Cambridge: Cambridge Univ. Press, 1966. Pp. 60. $1.75.

DAVID, F. N., ed. Research papers in statistics. Fesschrift for J. Neyman. New York and London: Wiley, 1966. Pp. 468.

LAURIE, E. J. Computers and computer languages. 2nd ed. Cincinnati: Southwestern Pub. Co., 1966. Pp. vii, 725. $8.75.

Meyers, C. H. Elementary business and economic statistics. Belmont, Calif.: Wadsworth, 1966. Pp. xii, 496. $11.95.

Neter, J. and Wasserman, W. Fundamental statistics for business and economics. 3rd ed. Boston: Allyn and Bacon, 1966. Pp. xii, 738. $9.95. First pub. in 1954.

Simonnard, M. Linear programming. Transl. by W. S. Jewell. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xxiv, 430. $9.

Stockton, J. R. Introduction to business and economic statistics. 3rd ed. Cincinnati: South-western Pub. Co., 1966. Pp. xi, 632.

Stone, R. Mathematics in the social sciences and other essays. Cambridge: M.I.T. Press, 1966, Pp. xiii, 291. $12.

Stuart, F. Introductory computer programming. New York and London: Wiley, 1966. Pp. xi, 155.

Economic Systems; Planning and Reform; Cooperation

Bernard, P. J. Planning in the Soviet Union. Transl. by I. Nove. New York and London: Pergamon Press, 1966. Pp. xxv, 309. $9.50. First pub. in 1963 by Ouvrières.

Chambre, H. L'économie planifiée. Que sais-je? Le point des connaissances actuelles no. 329. Paris: Presses Univ. de France, 1966. Pp. 126.

Güsten, R. Problems of economic growth and planning: the Sudan example—some aspects and implications of the current Ten Year Plan. Afrika-stud. no. 9. New York and Berlin: Springer, 1966. Pp. xii, 61. DM 13.50.

Hayward, J. E. S. Private interests and public policy—the experience of the French Economic and Social Council. New York: Barnes & Noble, 1966. Pp. 115.

Pejovich, S. The market-planned economy of Yugoslavia. Minneapolis: Univ. of Minnesota Press, 1966. Pp. xii, 160. $5.75.

Rakowski, M., ed. Efficiency of investment in a socialist economy. Transl. from the Polish by E. Lepa. New York and London: Pergamon Press, 1966. Pp. xi, 528. $12. First pub. in 1963 in Warsaw.

Romig, F. Theorie der wirtschaftlichen Zusammenarbeit. Berlin: Duncker & Humblot, 1966. Pp. 312. DM 48.

Sharpe, M. E., ed. Planning, profit and incentives in the USSR—Vol. I, The Liberman discussion—a new phase in Soviet economic thought. White Plains, N.Y.: Internat. Arts & Sci. Press, 1966. Pp. xiii, 314. $10.

Teich, G., comp. Der Rat für Gegenseitige Wirtschaftshilfe 1949-1963. Fünfzehn Jahre wirtschaftliche Integration im Ostblock. Kieler Schrifttumskunden zu Wirtschaft und Gesellschaft no. 14. Kiel: Inst. für Weltwirtschaft, Univ. Kiel, 1966. Pp. v, 445. DM 30.

Vellisz, S. The economies of the Soviet Bloc—a study of decision making and resource allocation. New York and London: McGraw-Hill, 1966. Pp. vi, 245. Paper, $2.45. First pub. in 1964.

Volfson, M. A reappraisal of Marxian economics. New York and London: Columbia Univ. Press, 1966. Pp. xii, 220. $6.75.

Business Fluctuations

Reichard, R. S. Practical techniques of sales forecasting. New York and London: McGraw-Hill, 1966. Pp. xii, 272. $8.50.

Scott, R. H. Problems in national income analysis and forecasting. Glenview, Ill.: Scott, Foresman and Co., 1966 Pp. 87. $1.75.

Managing a full employment economy. A CED symposium, Los Angeles, May 1966. New York: Com. for Econ. Develop., 1966. Pp. 62. $1.

Quarterly predictions of national income and expenditures no. 8. Wellington, N.Z.: New Zealand Inst. Econ. Research, 1966. Pp. 20.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

ADLER, R. W. AND MIKESELL, R. F. Public external financing of development banks in developing countries. Eugene: Bur. Bus. and Econ. Research, Univ. of Oregon, 1966. Pp. 89.

BAXTER, N. D. The commercial paper market. Boston: Bankers Pub. Co., 1966. Pp. xv, 153. $10.

BRANGER, J. Traité d'économie bancaire. Vol. 2, Les opérations de banque. Paris: Presses Univ. de France, 1966. Pp. 486.

BROTHERS, D. S. AND SOLÍS, M. L. Mexican financial development. Austin and London: Univ. of Texas Press, 1966. Pp. xi, 236. $7.50.

BROWN, C. V. The Nigerian banking system. Evanston, Ill.: Northwestern Univ. Press, 1966. Pp. 214. $6.

COHEN, K. J. AND HAMMER, F. S., eds. Analytical methods in banking. Homewood, Ill.: Irwin, 1966. Pp xv, 599, $12.50.

GRAEBNER, W Direkte Kontrollen als Mittel der Geldpolitik. Cologne: Westdeutscher, 1966. Pp. 86. DM 29.90.

HAINES, W. W. Money, prices and policy. 2nd ed. New York and London. McGraw-Hill, 1966. Pp. xx, 854. $9.75. First pub. in 1961. Reviewed in 1961 by H. S. Gordman.

JARCHOW, H.-J. Theoretische Studien zum Liquiditätsproblem. Kieler stud. no. 75. Tübingen: J.C.B. Mohr (Paul Siebeck), 1966. Pp. v, 191. DM 36.

KHAZZOOM, J. D. The currency ratio in developing countries. New York and London: Praeger, 1966. Pp. xv, 122. $13.50.

MAURI, A. Il mercato del credito nei paesi sottosviluppati. Ser. 4, no. 19. Milan: Giuffrè, 1966. L. 1.200.

MIYASHITA, T. The currency and financial system of mainland China. Tokyo: Inst. Asian Econ. Affairs, 1966. Pp. viii, 278. $6.

MORRISON, G. R. Liquidity preferences of commercial banks. Chicago and London: Univ. of Chicago Press, 1966. Pp. xi, 163. $7.50.

QUINTERO-RAMOS, A. M. A history of money and banking in Argentina. Rio Piedras: Univ. of Puerto Rico, 1965. Pp. 346.

SEN, S. N. Co-operative movement in West Bengal. Calcutta: Bookland Private, 1965. Pp. 63. Rs. 5.

URI, P. WITH KALDOR, N., RUGGLES, R. AND TRIFFIN, R. Una política monetaria para América Latina. Mexico: Centro de Estudios Monetarios Latinoamericanos, 1966. Pp. 172.

WILSON, J. S. G. Monetary policy and the development of money markets . . . collected papers. London: Allen & Unwin, 1966. Pp 354. 45s.

Bank of Greece—Report—1965. Athens: Bank of Greece, 1966. Pp. 111.

Le financement du logement en Frances et à l'étranger. Centre d'Information et d'Etude du Crédit ser. Paris: Presses Univ. de France, 1966. Pp. xviii, 267. F 20.

Public Finance; Fiscal Policy

DATTA, A. Inter-governmental grants in metropolitan Calcutta. Calcutta research stud. ser. no. 10. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. xiv, 50. $3.35.

GHAI, D. P. Taxation for development—a case study of Uganda. East African stud. no. 23. Nairobi: East African Pub. House, 1966. Pp. 132. $4.

KRZYZANIAK, M., ed. Effects of corporation income tax—papers presented at the symposium on bus. tax. Detroit: Wayne State Univ. Press, 1966. Pp. 268. $8.95.
Contributions by M. Krzyzaniak, A. C. Harberger, R. A. Musgrave, R. E. Slitor and R. Goode.

Misra, B. Fiscal policy in the context of planning—an analysis of Keynesian economics in relation to economic development. Calcutta: World Press Private, 1966. Pp. x, 347. $5.

Okner, B. A. Income distribution and the federal income tax. Michigan govt. stud. monogr. no. 47. Ann Arbor: Univ. of Michigan Inst. Public Admin., 1966. Pp. xiii, 148. $3.

Pechman, J. A. Federal tax policy. Washington: Brookings Inst., 1966. Pp. xxi, 321. $2.45.

Shoup, C. S. Federal estate and gift taxes. Washington: Brookings Inst., 1966. Pp. xvi, 253. Paper, $2.50.

Sommerfield, R. M. Tax reform and the Alliance for Progress. Austin and London: Univ. of Texas Press for Inst. Latin Am. Stud., 1966. Pp. xiv, 217. $5.

Estudio comparativo de sistemas tributarios de los paises centroamericanos. Ser. econ. y fin. no. 416. San José, Costa Rica: Escuela Superior de Admin. Pública, América Central, 1966. Pp. 31.

Modernizing local government to secure a balanced federalism—a statement on national policy by CED Research and Policy Com., July 1966. New York: Com. for Econ. Develop., 1966. Pp. 77, $1.

Statistics of income . . . 1964: preliminary individual income tax returns. Treasury Dept., Internal Rev. Svce. pub. no. 198 (6-66). Washington: Supt. Docs., 1966. Pp. 31. 25¢.

International Economics

Aharoni, Y. The foreign investment decision process. Boston: Grad. School Bus. Admin., Harvard Univ., 1966. Pp. xvii, 362. $6.

Blough, R. International business: environment and adaptation. New York: McGraw-Hill, 1966. Pp. 373.

Chopra, V. P. India's industrialization and mineral exports. New York: Asia Pub. House; Taplinger, distrib., 1965. Pp. viii, 95. $6.50.

Conan, A. R. The problem of sterling. New York: St. Martin's Press; London: Macmillan, 1966. Pp. v, 122. $5.

Dasgupta, M. International liquidity—a study of new plans for international trade. Calcutta: Sri Sadhan Dasgupta, 1965. Pp. iii, 182.

Douglas, P. H. America in the market place—trade, tariffs and the balance of payments. New York: Holt, Rinehart and Winston, 1966. Pp. ix, 381. $7.95.

Eckstein, A. Communist China's economic growth and foreign trade—implications for U.S. policy. New York and London: McGraw-Hill for Council on Foreign Relations, 1966. Pp. xvii, 366. $8.50.

Einzig, P. A textbook on foreign exchange. New York: St. Martin's Press; London: Macmillan, 1966. Pp. xv, 251. $5.

Farmer, R. N. and Richman, B. M. International business: an operational theory. Homewood, Ill.: Irwin, 1966. Pp. 314.

Harrod, R. The International Monetary Fund, yesterday, today and tomorrow. Kiel: Inst für Weltwirtschaft, Univ. Kiel, 1966. Pp. 27, DM 3.50.

Hayter, T. French aid. London: Overseas Develop. Inst., 1966. Pp. 230. 20s.

Holmes, F. W. Freer trade with Australia? Discussion paper no. 10. Wellington: New Zealand Inst. Econ. Research, 1966. Pp 40.

Hufbauer, G. C. Synthetic materials and the theory of international trade. Cambridge: Harvard Univ. Press, 1966. Pp. 165. $6.50.

Kohout, J., Ryska, J. and Wagner, R. Czechoslovakia and the UN Conference on Trade and Development. Praha: Rapid, 1964. Pp. 174.

Krämer, H.-R. Wirtschaftliche und rechtliche Probleme der monetären Integration in der Europäischen Wirtschaftsgemeinschaft. Kieler stud. no. 76. Tübingen: J.C.B. Mohr (Paul Siebeck), 1966. Pp. iv, 179. DM 32.

Müller-Armack, A. Wirtschaftsordnung und Wirtschaftspolitik—Studien und Konzepte zur

Sozialen Marktwirtschaft und zur Europäischen Integration. Freiburg: Rombach, 1966. Pp. 472.

NDEGWA, P. The Common Market and development in East Africa. East African stud. no. 22. Nairobi: East African Pub. House, 1965. Pp. 150. $3.

NEHRT, L. C. Financing capital equipment exports—a comparative study of medium-term export financing. Scranton, Pa.: Internat. Textbook Co., 1966. Pp. 122. Paper, $2.25.

PATTERSON, G. Discrimination in international trade—the policy issues 1945-1965. Princeton: Princeton Univ. Press, 1966. Pp. xiv, 414. $10.

ROOSA, R. V. AND HIRSCH, F. Reserves, reserve currencies, and vehicle currencies: an argument. Essays in internat. fin. no. 54. Princeton: Internat. Fin. Sec., Princeton Univ., 1966. Pp. 41. Single copies free.

RUBIN, S. J. The conscience of the rich nations: the Development Assistance Committee and the common aid effort. New York and London: Harper & Row, 1966. Pp. x, 153. $3.95.

STEINER, G. A. AND CANNON, W. M. Multinational corporate planning. New York: Macmillan; London: Collier-Macmillan, 1966. Pp. xiii, 330. $7.95.

TRIANTIS, S. G. Common market and economic development. Research monogr. ser. no. 14. Athens: Center of Planning and Econ. Research, 1965. Pp. 232.

Importancia de la modernizacion de la administración pública para el programa de integración del Istmo Centroamericano. Ser. no. 701. San José, Costa Rica: Escuela Superior de Admin. Pública, América Central, 1966. Pp. 43.

International dimensions in business—recent readings from *Business Topics*. East Lansing: Inst. for Internat. Bus. Management Stud., Michigan State Univ., 1966. Pp. vii, 302.

The United State balance of payments—an appraisal of U.S. economic strategy. Washington: Internat. Econ. Policy Assn., 1966. Pp. viii, 200. $10.

Business Finance; Investment and Security Markets; Insurance

BERANEK, W. Working capital management. Belmont, Calif.: Wadsworth, 1966. Pp. xi, 145. Paper, $3.95.

BRITTAIN, J. A. Corporate dividend policy. Washington: Brookings Inst., 1966. Pp. xvii, 254. Paper, $2.50.

CHRISTY, G. A. Capital budgeting: current practices and their efficiency. Eugene: Bur. Bus. and Econ. Research, Univ. of Oregon, 1966. Pp. ix, 94. Paper, $4.

HUNT, P., WILLIAMS, C. M. AND DONALDSON, G. AND OTHERS. Basic business finance—text and cases. 3rd ed. Homewood, Ill.: Irwin, 1966. Pp. xxi, 1016. $8.95. First pub. in 1958. Reviewed in 1959 by K. A. Boedecker.

LEA, W. H. The ABC's of the stock market. New York: Vantage Press, 1966. Pp. 122. $2.95.

LIVERMORE, J. L. How to trade in stocks—the Livermore formula for combining time element and price. Palisades Park, N.J.: Investors' Press, 1966. Pp. 112. $6.95.

SHULSKY, S. The new stock buying guide. New York: Arco, 1965. Pp. 112. $3.50.

WESTON, J. F. The scope and methodology of finance. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xii, 143. Paper, $2.50.

Business Organization; Managerial Economics; Marketing; Accounting

BAKKE, F. W. Bonds of organization—an appraisal of corporate human relations. 2nd ed. Hamden, Conn.: Archon Books, Shoe String Press, 1966. Pp. xxxii, 239. $7.50. First pub. in 1950 by Harper and Row.

BATY, W. M. English for business. Belmont, Calif.: Wadsworth, 1966. Pp. xi, 466. Paper, $6.69; text ed. $4.95.

BRIDGES, F. J. AND OLM, K. W. Business policy cases, incidents, and readings. Boston: Allyn and Bacon, 1966. Pp. xiii, 578. $9.50.

BROOM, H. N. AND LONGENECKER, J. G. Small business management. 2nd ed. Cincinnati: Southwestern Pub. Co., 1966. Pp. ix, 832. $8.75.

CHACKO, G. K. Today's information for tomorrow's products—an operation research approach. Washington: Thompson Book Co., 1966. Pp. xii, 225. $11.

CHRUDEN, H. J. AND SHERMAN, A. W., JR. Readings in personnel management. 2nd ed. Cincinnati: Southwestern Pub. Co., 1966. Pp. viii, 568.

DYER, R., HOELTER, J. E. AND NEWTON, J. A. and Assocs. Optical scanning for the business man. New York and Buenos Aires: Hobbs, Dorman & Co., 1966. Pp. xii, 190. $14.50.

ELAM, H. G. Business administration: simulations and problems. Workbook to accompany *Business administration: problems and functions* by R. H. Simonds, R. E. Ball and E. J. Kelley. Boston: Allyn and Bacon, 1966. Pp. xiii, 241. $3.75.

ENTENBERG, R. D. Effective retail and market distribution—a managerial economic approach. Cleveland: World Pub. Co., 1966. Pp. xiv, 591. $12.50.

FARMER, R. N., RICHMAN, B. M. AND RYAN, W. G. Incidents in applying management theory. Belmont, Calif.: Wadsworth, 1966. Pp. xii, 191. Paper, $2.60.

HAMBERG, D. R & D: essays on the economics of research and development. New York: Random House, 1966. Pp. xiii, 170. $4.95.

HILL, S. R. The distributive system. New York and Oxford: Pergamon Press, 1966. Pp. ix, 166. Paper, $2.95.

JOHNSON, A. H., JONES, G. E. AND LUCAS, D. B. The American market of the future. The Charles C. Moskowitz lectures no. 6. New York: New York Univ. Press, 1966. Pp. xi, 138. $3.50.

LINDFORS, G. V., comp. Intercollegiate bibliography. Cases in business administration, vol. 10. Boston: Intercollegiate Case Clearing House, 1966 Pp. x, 107.

NEMMERS, E. E. AND MYERS, J. H. Business research—text and cases. New York and London: McGraw-Hill, 1966. Pp. x, 621. $11.50.

NEUNER, J. J. W. AND KEELING, B. L. Administrative office management. 5th ed. Cincinnati: Southwestern Pub. Co., 1966. Pp. xi, 775. $8.25.

SALIN, E. Die automatische Mittelpufferkupplung—technischer Fortschritt als finanz- und wirtschaftspolitsches Problem mit Kostenschätzungen für 8 europäische Länder, vol. 5. Tübingen: J. C. B. Mohr (Paul Siebeck), 1966 Pp. vi, 77. DM 11.20.

SIELAFF, T. J. AND ABERLE, J. W. Introduction to business—American enterprise in action. 2nd ed. Belmont, Calif.: Wadsworth, 1966. Pp xiii, 623. $11.35; text ed. $8.50 First pub in 1961.

Economic inquiry into food marketing: Part II, The frozen fruit, juice and vegetable industry. Staff report to the Fed Trade Comm., Dec. 1962. Washington: Supt. Docs., 1962 Pp x, 135 Paper, $1.25.

Economic inquiry into food marketing: Part III, The canned fruit, juice and vegetable industry. Staff report to the Fed. Trade Comm., June 1965. Washington: Supt. Docs., 1965. Pp. xiii, 200. Paper, $1.25.

Management information systems and the information specialist. Proceedings of a symposium held at Purdue Univ., July 1965. Lafayette, Ind.: Purdue Univ. Libraries, 1966. Pp. 138. $3.

Industrial Organization; Government and Business; Industry Studies

ALBERTS, W. W. AND SEGALL, J. E., eds. The corporate merger. Chicago and London: Univ. of Chicago Press, 1966 Pp. xviii, 287. $6.

BROWN, R. T. Transport and the economic integration of South America. Washington: Brookings Inst., 1966. Pp. xiii, 288. $6.

CLABAULT, J. M. AND BURTON, J. F., JR. Sherman Act indictments 1955-1965—a legal and economic analysis. New York: Federal Legal Pub., 1966. Pp. xv, 504.

CLARK, J. J. The new economics of national defense. New York: Random House, 1966. . 242. $4.95.

KUMAR, D., NAG, S. P. AND VENKATARAMANAN, L. S. Resource allocation in the cotton tex industry. New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. ix, 191. $9.50.

LAWRENCE, S. A. United States merchant shipping policies and politics. Washington: Bro ings Inst., 1966. Pp. xiii, 405. $8.75.

MANSFIELD, H. Vision—the story of Boeing. New York: Popular Library, 1966. Pp. 3 Paper, 75¢.

OEFTERING, H. M. Probleme der Deutschen Bundesbahn von heute und morgen. Ki Vorträge no. 46. Kiel: Inst. für Weltwirtschaft, Univ. Kiel, 1966. Pp. 17. DM 2.50.

RICHTER, W. Probleme und Möglichkeiten der Spezialisierungskooperation. FIW-Schrift reihe no. 27. Cologne: Carl Heymanns, 1965. Pp. 78. DM 8.50.

STELZER, I. M. Selected antitrust cases—landmark decisions. 3rd ed. Homewood, Ill.: Irv 1966. Pp. x, 316. $3.95. First pub. in 1955. Reviewed in 1956 by E. L. Eckles.

STUART, F., ed. Factors affecting determination of market shares in the American autor bile industry. *Hofstra Univ. Yearbook of Bus.*, Oct. 1965. Hempstead, N.Y.: Hofs Univ., 1965. Pp. 380. $5.

YAMEY, B. S., ed. Resale price maintenance. Chicago: Aldine, 1966. Pp. vii, 303. $7.5(

Committee on transport policy and coordination—final report. New Delhi: Govt. of In Planning Comm., 1966. Pp. vii, 340. $1.35.

Economic report on mergers & vertical integration in the cement industry. Staff report the Fed. Trade Comm. Washington: Supt. Docs., 1966. Pp. xiv, 123. 45¢.

A study on cotton textiles. Geneva: General Agreement on Tariffs and Trade; New Yo Columbia Univ. Press, distrib., 1966. Pp. vi, 246. $3.

Land Economics; Agricultural Economics; Economic Geography; Housi

ADAMS, D. W., PEÑA, A. R. AND GILES, A. El crédito supervisado en la reforma agra colombiana—un estudio evaluativo. Materiales de enseñanza para reforma agraria no Bogotá: IICA-CIRA, 1966. Pp. 102.

BAIN, H. O. California real estate bookshelf. Rev. ed. Berkeley: Univ. of California I Urban and Regional Develop., 1966. Pp. viii, 189. $1.50.

BREESE, G. Urbanization in newly developing countries. Englewood Cliffs, N.J.: Prent Hall, 1966. Pp. vi, 151. $4.95; paper, $2.50.

BRUYN, S. T. Committees in action—pattern and process. New Haven, Conn.: College : Univ. Press, 1963. Pp. 205. $5; paper, $1.95.

CHAUHAN, D. S. Studies in utilization of agricultural land. Agra: Shiva Lal Agarwal: Co., 1966. Pp. 210. Rs. 20.

COOLEY, R. A. Alaska—a challenge in conservation. Madison and London.: Univ. of V consin Press, 1966. Pp. xv, 170. $5.50.

FRANK, C. S. The sugar industry in East Africa—an analysis of some problems and po questions relating to the expansion of the sugar industry in a developing economy. I African stud. no. 20. Nairobi: East African Pub. House, 1965. Pp. 115. $3.

GHOSAL, S. N. Agricultural financing in India—with special reference to land mortg banks. New York: Asia Pub. House, Taplinger, distrib., 1966. Pp. xxiii, 199. $7.

GIMENEZ LANDINEZ, V. Capacitación para la reforma agraria integral—conceptos, meto logías e instrumentos idóneos. Materiales de enseñanza para reforma agraria no. 8. Bogo IICA-CIRA, 1966. Pp. 62.

HAYNER, N. S., in collab. with HAYNER, U. M. New patterns in old Mexico—a study town and metropolis. New Haven, Conn.: College & Univ. Press, 1966. Pp. 316. paper, $2.45.

HUFSCHMIDT, M. M. AND FIERING, M. B. Simulation techniques for design of wa resource systems. Cambridge: Harvard Univ. Press, 1966. Pp. xi, 212. $7.50.

HUNDLEY, N., JR. Dividing the waters—a century of controversy between the United States and Mexico. Berkeley: Univ. of California Press, 1966. Pp. xii, 266. $6.95.

KAHN, M. H. The role of agriculture in economic development—a case study of Pakistan. Wageningen, The Netherlands: Centre for Agric. Pub. and Doc., 1966. Pp. 161.

KIRKENDALL, R. S. Social scientists and farm politics in the age of Roosevelt. Columbia: Univ. of Missouri Press, 1966. Pp. ix, 358. $7.

MOSHER, A. T. Getting agriculture moving—essentials for development and modernization. New York and London: Praeger, 1966. Pp. 190. $6.50.

A training manual and set of selected readings related to the chapter topics are available from the Agricultural Develop. Council, Inc., 630 Fifth Ave., N.Y., N.Y. 10020.

NICHOLLS, W. H. AND PAIVA, R. M. Ninety-nine fazendas: the structure and productivity of Brazilian agriculture, 1963. Ch. II, The Itapecuru Valley of Maranhão: Caxias. Prelim. ed. Nashville: Grad. Center for Latin American Stud., Vanderbilt Univ., 1966. Pp. ix, 151. $3.50.

PANDYA, H. L. India could and can export food—an introduction to the study of Indian food grains. Bombay: Pandya, 1966. Pp. 23. Rs. 2.

PATERSON, R. W. Forecasting techniques for determining the potential demand for highways. Columbia: Bus. and Pub. Admin. Research Center, Univ. of Missouri, 1966. Pp. 128. Paper, $3.

PELL, C. Megalopolis unbound—the supercity and the transportation tomorrow. New York: Praeger, 1966. Pp. vii, 233. $5.95.

PELTIER, L. C. AND PEARCY, G. E. Military geography. New York and London: Van Nostrand, 1966. Pp. 176. Paper, $1.95.

PURCELL, J. C. AND FORD, K. E. Long-run consumption trends and comparative shares by regions for fresh and processed food products. Athens: Univ. of Georgia Agric. Experiment Stations, 1966. Pp. 25.

PURCELL, J. C., ELROD, J. C. AND RAUNIKAR, R. Price-quantity expenditure relations for food, Atlanta consumer panel quarterly estimates. Tech. bull. no. 52. Athens: Univ. of Georgia College of Agric., 1966. Pp. 82.

RAO, V. K. R. V. AND DESAI, P. B. Greater Delhi—a study in urbanisation—1940-1957 New York: Asia Pub. House, Taplinger, distrib., 1965. Pp. xxxiii, 479. $21.

RAUNIKER, R., PURCELL, J. C. AND ELROD, J. C. Consumption and expenditure analysis for dairy products, fats, and oils in Atlanta, Georgia. Tech. bull. no. 51. Athens: Univ. of Georgia College of Agric., 1966. Pp. 68.

———. Consumption and expenditure analysis for fruits and vegetables in Atlanta, Georgia. Tech. bull. no. 53. Athens: Univ. of Georgia College of Agric., 1966. Pp. 64.

RUSSELL, E. J. A history of agricultural science in Great Britain—1620-1954. New York: Humanities Press, 1966. Pp. 493. $10.

SHANAHAN, E. W. Western and Central Europe—a regional geography. New York: St. Martin's Press; London. Macmillan, 1965. Pp. xv, 493. $6. First pub. in 1964.

SMITH, W. F. Aspects of housing demand—absorption, demolition, and differentiation. Research report no. 29. Berkeley: Univ. of California Inst of Urban and Regional Develop., 1966. Pp. vii, 89. $2.50.

SPIEGELMAN, R. G. Analysis of urban agglomeration and its meaning for rural people. Agric. econ. report no. 96. Washington: U.S. Dept. of Agric. Econ. Research Svce., 1966. Pp. 24.

STOCKER, F. D. The role of local government in economic development of rural areas—research needs and opportunities. Agric. econ. report no. 94. Washington: U.S. Dept. of Agric. Econ. Research Svce., 1966. Pp. 20.

WEIMER, A. M. AND HOYT, H. Real estate. 5th ed. New York: Ronald, 1966. Pp. xiv, 758. $8.50. First pub. in 1939.

WEISS, S. F., SMITH, J. E., KAISER, E. J. AND KENNEY, K. B. Residential developer decisions—a focused view of the urban growth process. Urban stud. research monogr. Chapel Hill: Center for Urban and Regional Stud., Univ. of North Carolina, 1966. Pp. v, 94. $2.

Informe de la reunión internacional de ejecutivos de la reforma agraria y de la reunion de evaluación y planeamiento del proyecto 206. Lima-Bogotá 1965. Bogotá: IICA-CIRA, 1966. Pp. 132.

Land tenure conditions and socio-economic development of the agricultural sector—Argentina. CIDA (Inter-American Com. Agric. Develop.). Washington: Pan Am. Union, 1966. Pp. x, 141. $2.50.

Statistical summary of the mineral industry—world production, exports and imports 1959-1964. Prepared by Mineral Resources Div., Overseas Geological Surveys. London: H. M. Stationery Office, 1966. Pp. iv, 417. $5.50.

Tenencia de la tierra y desarrollo socio-económico del sector agrícola—Colombia. CIDA (Inter-American Com. Agric. Develop.). Washington: Pan Am. Union, 1966. Pp. xviii, 563. $4.50.

Tenencia de la tierra y desarrollo socio-económico del sector agrícola—Ecuador. CIDA (Inter-American Com. Agric. Develop.). Washington: Pan Am. Union, 1966. Pp. xxv, 539. $4.50.

Tenencia de la tierra y desarrollo socio-económico del sector agrícola—Guatemala. CIDA (Inter-American Com. Agric. Develop.). Washington: Pan Am. Union, 1966. Pp. xx, 244. $2.50.

Labor Economics

BAKKE, E. W. Mutual survival—the goal of unions and management. 2nd ed. Hamden, Conn.: Archon Books, 1966. Pp. ix, 116. $3. First pub. in 1946.

BERNSTEIN, I. The lean years—a history of the American worker 1920-1933. Baltimore: Penguin Books, 1966. Pp. xi, 577. Paper, $2.25.

BROWN, E. C. Soviet trade unions and labor relations. Cambridge: Harvard Univ. Press, 1966. Pp. ix, 394. $6.95.

DAVIES, J. African trade unions. Baltimore: Penguin Books, 1966. Pp. 256. Paper, $1.25.

DE SCHWEINITZ, D. Labor-management consultation in the factory—the experience of Sweden, England, and the Federal Republic of Germany. Honolulu: Univ. of Hawaii, College of Bus. Admin., 1966. Pp. ix, 128. $2.50.

DODGE, N. T. Women in the Soviet economy—their role in economic, scientific, and technical deveolpment. Baltimore: Johns Hopkins Press, 1966. Pp. xviii, 331. $10.

GILPATRICK, E. G. Structural unemployment and aggregate demand—a study of employment and unemployment in the United States, 1948-1964. Baltimore: Johns Hopkins Press, 1966. Pp. xviii, 235. $8.50.

HABER, W., ed. Labor in a changing America. New York and London: Basic Books, 1966. Pp. ix, 341. $5.95.

KAY, E. R. How to establish current reporting of employment hours. and earnings in developing countries. Bur. Labor Stat., report no. 302. Washington: U.S. Dept. of Labor, 1966. Pp. 150.

KUHN, J. W. Scientific and managerial manpower in nuclear industry. New York and London: Columbia Univ. Press, 1966. Pp. xv, 209. $7.50.

LEDRUT, R. Sociologie du chômage. Paris: Presses Univ. de France, 1966. Pp. 547. F 30.

MOSKOW, M. H. Teachers and unions—the applicability of collective bargaining to public education. Philadelphia: Wharton School, Univ. of Pennsylvania, 1966. Pp. xiii, 288.

PECK, S. M. The rank-and-file leader. New Haven, Conn.: College and Univ. Press, 1963. Pp. 398. $6; paper, $2.95.

ROBERTS, H. S., ed. The challenge of industrial relations in Pacific-Asian countries—proc. of

conference on . . . vol. 1. Honolulu: Univ. of Hawaii College of Bus. Admin., 1966. 174. Single copies free.

———. Who's who in industrial relations . . . vol. 1. A-L. Honolulu: Univ. of Hawaii C lege of Bus. Admin., 1966. Pp. 171. $1.

STIEBER, J., ed. Employment problems of automation and advanced technology—an ternational perspective. Proc. of conference at Geneva, Internat. Inst. Labour Stud., 19 July 1964. New York: St. Martin's Press; London: Macmillan, 1966. Pp. xvii, 479. $16

SURACE, S. J. Ideology, economic change, and the working classes: the case of Italy. Ber ley: Univ. of California Press, 1966. Pp. xiii, 196. $5.

Collective bargaining adjustments to technological change. Dept. of Labor Stat. and search. COMMAT report no. 65-5. Sacramento: Comm. on Manpower, Automation Technology, 1965. Pp. 170.

Michigan manpower study—phase 1. An analysis of the characteristics of Michigan's la force in the next 15 years. Battelle Memorial Inst., Socio-Econ. Research Section. Detr Michigan Employment Security Comm., 1966. Pp. 81. $2.50; includes phase 2 and su material.

National Conference on Labour-Management Relations, 9-10 Nov. 1964. Ottawa, Cana Ottawa: R. Duhamel, 1965. Pp. 353.

Ocupación y desocupación—Gran Santiago, marzo de 1966 Pub no. 84. Santiago: Inst. Econ., Universidad de Chile, 1966. Pp. 30.

Ocupación y desocupación—Gran Santiago, junio de 1966. Inst. Econ. pub. no. 86. Santia Inst. de Econ., Univ. de Chile, 1966. Var. pp.

Unemployment insurance amendments of 1966—report together with minority views accompany H.R. 15119, a bill to extend and improve the federal-state unemploym compensation program. 89th Cong., 2nd sess., Senate, Com. on Fin. report no. 14 Washington: Supt. Docs., 1966. Pp 92

Population; Welfare Programs; Consumer Economics

BACKER, J. E. AND AAGENAES, Ø. Dødelighet blant spedbarn i norge 1901-1963. (Infant m tality in Norway 1901-1963.) Samfunnsøkonomiske stud. no. 17. Oslo: Central B Stat. of Norway, 1966. Pp. 74. NKr. 7.

BENSON, C. S. The school and the economic system Chicago: Sci Research Associates, 19 Pp. 117. Paper, $1.95.

BLAUG, M. Economics of education—a selected annotated bibliography. New York London: Pergamon Press, 1966. Pp. xiii, 190. $6.50.

CHOUDHURI, S. R. Sickness insurance in India and Britain. Calcutta: World Press Priv 1966. Pp. xvi, 318. $5.

GILMAN, C. P. Women and economics—a study of the economic relation between men women as a factor in social evolution. Ed by C. N. Degler. New York: Harper & R 1966. Pp. xxxix, 356. Paper, $2.75. First pub. in 1898 by Small, Maynard & Co.

HABER, W. AND MURRAY, M. G. Unemployment insurance in the American economy—historical review and analysis. Homewood, Ill.: Irwin, 1966. Pp. xx, 538. $8.50.

HASTINGS, J. E. F. AND MOSLEY, W. Organized community health services. Royal Com on Health Svces. stud. Ottawa: Queen's Printer, 1966. Pp. xix, 328. $3.50.

ISLAM, N. Studies in consumer demand, vol. 2. Pakistan: Oxford Univ. Press for Dacca Ur Bur. of Econ. Research, 1966. Pp. 166.

JEROMIN, U. Die Überseechinesen—ihre Bedeutung für die wirtschaftliche Entwickl Südostasiens. Ökonomische stud. no. 12. Stuttgart: Gustav Fischer, 1966. Pp. viii, 1 DM 29.50.

LASSITER, R. L., JR. The association of income and educational achievement. Soc. monogr. no. 30. Gainesville: Univ. of Florida Press, 1966. Pp. 52. $2.

LUBOVE, R, ed. Social welfare in transition—selected English documents, 1834-1909. troductory essays by J. Duffy and S. Mencher. Pittsburgh: Univ. of Pittsburgh Pr 1966. Pp. xiii, 334. $8.

MARCH, G. B., ed. Occupational data requirements for education planning. Proceedings of a conference. Madison: Univ. of Wisconsin Center Vocational and Technical Educ., 1966. Pp. xii, 165.

NASH, P. Culture and the state—Matthew Arnold and continental education. New York: Teachers College Press, Columbia Univ., 1966. Pp. 283. $4.95; paper, $2.95.

ROBINSON, E. A. G. AND VAIZEY, J. E., eds. The economics of education—proc. of conference held by the Internat. Econ. Assoc. New York: St. Martin's Press; London: Macmillan, 1966. Pp. xvii, 781. $16.

STEVENS, R. Medical practice in modern England—the impact of specialization and state medicine. New Haven and London: Yale Univ. Press, 1966. Pp. xii, 401. $10.

STYCOS, J. M. AND ARIAS, J., eds. Population dilemma in Latin America. Washington: Potomac Books, 1966. Pp. xiii, 249. $3.95; paper, $2.45. Contributions by various authors.

SWANSON, J. E., ARDEN, W. AND STILL, H. E., JR. Financial analysis of current operations of colleges and universities. Ann Arbor: Inst. Pub. Admin., Univ. of Michigan, 1966. Pp. xvi, 443. Paper, $3.50.

WEST, E. G. Education and the state—a study in political economy. London: Inst. of Econ. Affairs, 1965. Pp. xiii, 242. 40s.

Children and youth in national development in Latin America. Report of Conference in Santiago, Chile, 28 Nov. to 11 Dec. 1965. New York: UNICEF, 1966. Pp. 132. $2.50; paper, $1.25.

Economic aspects of the social security tax. Research pub. no. 5. New York: Tax Foundation, 1966. Pp. 63. $1.50.

Family incomes and expenditures in greater Santiago—experimental survey. Inst. Econ. pub. no. 85. Santiago: Inst. de Econ., Univ. de Chile, 1966. Pp. 256.

Opening fall enrollment in higher education, 1965. Nat. Center for Educ. Stat. circ. no. 796. Washington: Supt. Docs., 1966. Pp. 90. 50¢.

Population—revue bimestrielle de l'Institut National d'Etudes Démographiques. Nov.-Dec. 1965. Paris: Presses Univ. de France, 1965. Pp. 941-1189. F 6.50.

Contributions by A. Sauvy, J.-N. Biraben, P. Paillat, A. Nizard and R. Pressat.

Public finance and education. *Public Finance*, 1966, *XXI* (1-2). Papers and proc. of the 21st sess. Internat. Inst. Pub. Fin. at Paris, Sept. 20-22, 1965. The Hague: "Public Finance," 1966. Pp. 324.

Contributions by M. Frank, E. Rosselle, M. Orlowski, Z. Pirozynski, J. B. D. Derksen, V. V. Lawrov, M. Peston, M. O'Donoghue, L. Faluvégi, A. T. Peacock, C. D'Hoogh, E. Liefmann-Kiel, S. A. Aluko, E. R. Rolph and J. Wiseman.

Related Disciplines

ADLER, S. The isolationist impulse—its twentieth-century reaction. New York: Free Press; London: Collier-Macmillan, 1957. Pp. 507. Paper, $2.95.

CARR, E. H. German-Soviet relations between the two World Wars, 1919-1939. New York: Harper & Row, 1966. Pp. 146. Paper, $1.25. Orig. pub. in 1951 by Johns Hopkins Press.

FAULKNER, E. J., ed. Man's quest for security—a symposium. Lincoln: Univ. of Nebraska Press, 1966. Pp. xiv, 198. $5.

Contributions by S. Hook, K. Davis, H. P. Rome, H. W. Stoke, H. S. Beers, C. E. Walker and T. S. Power.

GALLIN, B. Hsin Hsing, Taiwan: a Chinese village in change. Berkeley: Univ. of California Press, 1966. Pp. xi, 324. $8.50.

GORDON, G. Role theory and illness—a sociological perspective. New Haven, Conn.: College & Univ. Press, 1966. Pp. 158. $4.50; paper, $1.95.

HAY, D. Europe—the emergence of an idea. New York: Harper & Row, 1966. Pp. ix, 132. $1.25. Orig. pub. in 1957 by Edinburgh Univ. Press.

HILLER, E. T. The nature and basis of social order. New Haven, Conn.: College & Univ. Press, 1966. Pp. 160. $4; paper, $1.95.

ILFELD, F., JR. AND LAUER, R. Social nudism in America. New Haven, Conn.: College and Univ. Press, 1964. Pp. 240. $5; paper, $1.95.

LACY, D. The meaning of the American Revolution. New York and Toronto: New Am. Lib.; London: New English Lib., 1964. Pp. 303. Paper, 95¢.

LAQUEUR, W. AND MOSSE, G. L., eds. International fascism 1920-1945. New York: Harper & Row, 1966. Pp. x, 197. Paper, $1.75.

Contributions by various authors. Originally pub. as Vol. 1, no. 1, 1966, of the *Journal of Contemporary History,* by Weidenfeld & Nicolson, Ltd.

LASKY, V. The ugly Russian. New York: Pocket Books, 1966. Pp. vii, 274. Paper, 75¢. First pub. by Trident Press, 1965.

LEOPOLD, R. W., LINK, A. S. AND COBEN, S., eds. Problems in American history. Vol. I, Through reconstruction; Vol. II, Since reconstruction. 3rd ed. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xxi, 436; x, 437. $4.95 each. First pub. in 1952.

LEVIN, M. B. Kennedy campaigning—the system and the style as practiced by Senator Edward Kennedy. Boston: Beacon Press, 1966. Pp. xix, 313. $5.95.

LOEWENSTEIN, K. Max Weber's political ideas in the perspective of our time. Transl. from the German by R. and C. Winston. Amherst: Univ. of Massachusetts Press, 1966. Pp. xii, 104. Paper, $2.

MANGONE, G. J., ed. UN administration of economic and social programs. Columbia Univ. stud. in internat. organization no. 3. New York and London: Columbia Univ. Press, 1966. Pp. xxi, 291. $6.95.

Contributions by L. M. Goodrich, R. Gregg, G. J. Mangone, T. Meron and W. R. Sharp.

MONTAGUE, J. B., JR. Class and nationality—English and American studies. New Haven, Conn.: College & Univ. Press in assoc. with Washington State Univ., 1963. Pp. 246. $5; paper, $1.95.

NAMIER, L. Facing East—essays on Germany, the Balkans, and Russia in the twentieth century. New York: Harper & Row, 1966. Pp. 143. Paper, $1.25. Orig. pub. in 1947 by Hamish Hamilton, Ltd.

NEHEMKIS, P. R. Latin America: myth and reality. Rev. ed. New York and Toronto: New Am. Lib., 1966. Pp. xi, 317. Paper, 95¢. First pub. in 1964.

NIEHOFF, A. H., ed. A casebook of social change. Chicago: Aldine, 1966. Pp. 312. $6.

PANNELL, J. P. M. The techniques of industrial archaeology. New York: Taplinger, 1966. Pp. 191. $7.50.

ROSE, A. J. Dilemmas down under—Australia and the Southwest Pacific. New York and London: Van Nostrand, 1966. Pp. 112. Paper, $1.45.

SMELSER, N. J. AND LIPSET, S. M., eds. Social structure and mobility in economic development. Chicago: Aldine, 1966. Pp. ix, 399. $10.75.

VOIGT, D. Q. American baseball—from gentleman's sport to the commissioner system. Norman: Univ. of Oklahoma Press, 1966. Pp. xxxi, 336. $5.95.

WEINER, M., ed. Modernization—the dynamics of growth. New York and London: Basic Books, 1966. Pp. xvi, 355. $6.95.

Basic facts about the United Nations. New York: United Nations, 1966. Pp. 62. 35¢

Latin America in Soviet writings—a bibliography. Vol. I: 1917-1958; Vol. II: 1959-1964. Comp. by L. Okinshevich, ed. by R. G. Carlton. Baltimore: Johns Hopkins Press, 1966. Pp. xvii, 257; xii, 311. $15 each; both vols., $25.

Recent publications in the social and behavioral sciences. The ABS Guide 1966 supplement. New York: Am. Behavioral Scientist, div. of Sage Publications, 1966. Pp. 220. $12.50.

South Africa and United States policy. Johannesburg: South Africa Foundation, 1966. Pp. 27. Paper, 50¢

United States government organization manual 1966-67. Washington: Supt. Docs., 1966. Pp. viii, 811. $2.

NOTES

In order to provide a year-round placement service for economists, the American Economic Association in cooperation with the Chicago Professional Office of the Illinois State Employment Service has established a National Registry for Economists. Candidates are asked to fill out and return a standard application form to the Chicago office. This will be kept on file and upon request will be duplicated and submitted to prospective employers. All inquiries concerning this service should be sent directly to the National Registry for Economists, 208 South La Salle Street, Chicago, Illinois 60604.

AER MANUSCRIPTS

The following manuscripts, exclusive of comments and replies and in addition to those listed in previous issues, have been accepted for publication in subsequent issues of the *American Economic Review:*

D. J. Aigner and A. J. Heins, "On the Determinants of Income Equality"

Ahmad Al-Samarrie and H. P. Miller, "State Differentials in Income Concentration"

A. Asimakopulos, "The Biological Interest Rate and the Social Utility Function"

W. J. Baumol, "Macroeconomics of Unbalanced Growth: The Anatomy of Urban Crises"

W. B. Bennett, "Cross-Section Studies of the Consumption of Automobiles in the United States"

Royall Brandis, "The Myth of Absolute Advantage"

J. A. Carlson, "Forecasting Errors and Business Cycles"

R. H. Day, "The Economics of Technological Change and the Demise of the Sharecropper"

Louis De Alessi, "The Short Run Revisited"

P. A. Diamond, "The Role of a Stock Market in a General Equilibrium Model with Technological Uncertainty"

Robert Eisner, "A Permanent Income Theory for Investment: Some Empirical Exploration"

F. T. Dolbear, Jr., "On the Theory of Optimum Externality"

T. J. Finn, "A Graphical Proof of a Property of a Closed Linear Model of Production"

A. M. Freeman, III, "Income Distribution and Planning for Public Investment"

R. E. Hall and D. W. Jorgenson, "Tax Policy and Investment Behavior"

J. A. Hanson and P. A. Neher, "The Neoclassical Theorem Once Again: Closed and Open Economies"

L. R. Klein and R. S. Preston, "Some New Results in the Measurement of Capacity Utilization"

M. K. Lewis, "Friedman-Meiselman and Autonomous Expenditures"

P. A. Meyer, "A Paradox on Profits and Factor Prices"

J. D. Mooney, "Urban Poverty and Labor Force Participation"

M. V. Pauly, "Mixed Public and Private Financing of Education: Efficiency and Feasibility"

C. R. Plott, "A Notion of Equilibrium and Its Possibility under Majority Rule"

F. M. Scherer, "Market Structure and the Employment of Scientists and Engineers"

B. N. Siegel and Robert Campbell, "The Demand for Higher Education in the United States, 1919-1964"

I. H. Silberman, "On Lognormality as a Summary Measure of Concentration"

G. J. Staller, "Patterns of Stability in Foreign Trade: OECD and COMECON, 1950-1963"

Announcements

The Census Bureau will publish early in October a new report, "Long-Term Economic Growth: A Statistical Compendium." This report brings together almost 400 aggregate annual economic time series, almost 800 component series, and many analytical tables and charts that are helpful in studies of economic growth.

Under the authority of the Manpower Act of 1963 as amended, the Department of Labor may enter into contracts or provide grants for the conduct of research by organizations and individuals outside the Department. The Office of Manpower Policy, Evaluation and Research provides support for the following research programs: (1) Contract research for relatively definitive projects on fundamental manpower problems; (2) Research grants of up to $10,000 in direct costs to (a) established scholars for the exploration and development of new research approaches to manpower problems and to (b) universities for the support of promising dissertation research in the manpower field; (3) Manpower research institutional grants of up to $75,000 a year for a period of three years to colleges and universities planning to establish or strengthen their activities in the manpower field. Detailed information on procedures for submission of research proposals is contained in the Guidelines prepared for each of these programs, copies of which may be obtained from the Director, Office of Manpower Policy, Evaluation and Research, Manpower Administration, U.S. Department of Labor, Washington, D.C. 20210.

As part of its annual meeting in San Francisco, November 1966, the Medical Care Section of the American Public Health Association had a session devoted to contributed papers on the Economics of Health and Medical Care. The session was chaired by Selma J. Mushkin. The following papers were read: "The Cost of Treatment of Specific Illnesses: Some Estimating Problems and Preliminary Findings," By Anne A. Scitovsky; "The Shapes of the Costs and Production Functions for Physician's Services by Specialty, Type of Practice and Geographic Area," by Donald E. Yett; "Comparative Costs and Alternative Institutional Arrangements in the Treatment of the Mentally Ill," by Kenneth M. McCaffree; "The High Cost of Premature Death," by Dorothy P. Rice; and "Present States of Cost Benefit Analysis in the Health Field," by Herbert E. Klarman.

The Social Science Institute, Washington University, St. Louis, announces vacancies in the Community Mental Health Research Training Program at both the postdoctoral and predoctoral levels. This program is designed to prepare social scientists for research and teaching positions involving the application of social science theory and method to problems of communty mental health.

Appointments in the postdoctoral program carry a minimum stipend of $6,000 for the nine-month academic year, plus a dependency allowance. Appointments in the predoctoral program carry a minimum stipend of $2,000, plus tuition and dependency allowance. Appointments may be renewed each year with increased stipend. Inquiries and requests for more detailed information may be addressed to George Psathas, Program Director, Community Mental Health Research Training Program, Social Science Institute, Washington University, St. Louis, Missouri 63130. Applications for appointments for the academic year 1967-68 should be directed to the Program Director by March 1, 1967.

Deaths

Arthur Z. Arnold, professor of economics, New York University.

E. G. Smith, professor emeritus, department of marketing, College of Business Administration, University of Texas.

Perry D. Teitelbaum, Resources for the Future, July 3, 1966.

Retirements

J. Fred Bell, department of economics, University of Illinois, August 1966.

Ruth Branigan, associate professor of business management, Old Dominion College, June 1966.

Clyde M. Kahler, professor of insurance, University of Pennsylvania.

Simeon Leland, dean, College of Arts and Sciences, Northwestern University, August 1966.

Charles L. Prather, professor of finance, College of Business Administration, University of Texas.

John H. Prime, distinguished professor of finance, professor emeritus and dean emeritus, School of Commerce, New York University, August 1966.

Donald Scoles, professor of insurance and finance, California State College, Los Angeles, June 1966.

Emil G. Spitzer, International Monetary Fund, November 1965; consultant, department of economic and social affairs, United Nations, February-June 1966.

John A. White, dean and professor of accounting, College of Business Administration, University of Texas.

Visiting Foreign Scholars

Robert L. Crouch, University of Essex: visiting associate professor of economics, Northwestern University.

Michael J. Farrell, Gonville and Caius College, Cambridge: visiting professor of economics, University of California, Berkeley, 1966-67.

Norman J. Gibson, University of Manchester: visiting associate professor, University of Wisconsin.

Laurence Harris, London School of Economics and Political Science: visiting assistant professor of economics, University of California, Berkeley, 1966-67.

Werner Hildenbrand, University of Heidelberg Technische Hochshule at Karlsruhe: visiting assistant professor of economics and mathematics, University of California, Berkeley, 1966-67.

Edmond Malinvaud, Institut National de la Statistique et d'Etudes Economique, Paris: visiting professor of economics, University of California, Berkeley, 1966-67.

E. Victor Morgan, University of Manchester: visiting professor of economics and commerce, Simon Fraser University, fall 1966.

André L. Müller, University College, Durban, Republic of South Africa: visiting assistant professor of economics, Dartmouth College, fall 1966.

A. Qayum, American University in Cairo: visiting professor of economics, University of Iowa, second semester 1966-67.

John M. Samuels, University of Birmingham: visiting associate professor of economics, Purdue University.

Frans Van Winckel, University of Louvain: visiting professor of applied mathematics, Graduate School of Business, University of Chicago, winter and spring 1967.

Promotions

Dennis J. Aigner: associate professor of economics, University of Illinois.

Dean F. Berry: associate professor of industry, University of Pennsylvania.

Richard A. Bilas: associate professor of economics, University of Southern California.

Francis E. Brown: associate professor of marketing and statistics, University of Pennsylvania.

John A. Carlson: associate professor of economics, Purdue University.

Charles R. Carr: professor of economics and management, Purdue University.

Richard C. Clelland: professor of statistics and operations research, University of Pennsylvania.

John R. Cox: professor of finance, California State College, Los Angeles.

Ed F. Crim, Jr.: professor of economics, University of Oklahoma.

Jean B. Crockett: professor, department of finance, University of Pennsylvania.

Michael E. DePrano: associate professor of economics, University of Southern California.

William P. Dommermuth: associate professor of marketing, College of Business Administration, University of Texas.

John E. Elliott: professor of economics, University of Southern California.

Robert P. Fairbanks: associate professor of economics, Northern Illinois University.

Antonio Giles: specialist, Interamerican Institute of Agricultural Science, OAS, Agrarian Reform, Rio de Janeiro.

David K. Hildebrand: assistant professor of statistics, University of Pennsylvania.

Eugene C. Holshouser: professor of economics, University of Georgia.

Edgar M. Hoover: university professor, University of Pittsburgh.

Mamoru Ishikawa: assistant professor of economics, University of Pittsburgh.

Shirley B. Johnson: assistant professor of economics, Washington Square College, New York University.

Jerome B. Kernan: associate professor of marketing, College of Business Administration, University of Texas.

Charles W. King, Jr.: associate professor of industrial management, Purdue University.

Robert L. Knox: associate professor of economics, Arizona State University.

Anne O. Krueger: professor of economics, University of Minnesota.

Jürgen Ladendorf: assistant professor of production, Harvard Business School.

Mildred Levy: assistant professor of economics, College of Business Administration, University of Illinois.

Cliff L. Lloyd: associate professor of economics, Purdue University.

Samuel M. Loescher: professor of economics, Indiana University.

John J. Murphy: associate professor of economics, The Catholic University of America.

David M. O'Neill: assistant professor of economics, University of Pennsylvania.

James A. Papke: professor of economics, Purdue University.

George M. Parks, associate professor of industry, University of Pennsylvania.

W. Nelson Peach: research professor of economics, University of Oklahoma.

Richard H. Puckett: assistant professor of economics, The American University.

J. C. Ray: professor of accounting, California State College, Los Angeles.

Eldon Reiling: assistant professor of economics, Northern Illinois University.

Dean Rickenbach: associate professor of economics, Brigham Young University.

John H. Riew: associate professor of economics, Pennsylvania State University.

Jack L. Robinson: associate professor of economics, University of Oklahoma.

Richard L. Rowan: associate professor of industry, University of Pennsylvania.

John Sagan: treasurer, Ford Motor Company.

Herbert I. Schiller: professor, Bureau of Economic and Business Research, University f Illinois.

Thomas F. Schutte: assistant professor of marketing, University of Pennsylvania.

Graham K. Shaw: assistant professor of economics, Washington Square College, New 'ork University.

Franklin Sherwood: assistant professor of economics, University of Massachusetts.

John J. Sherwood: associate professor of administrative sciences, Purdue University.

Raynard M. Sommerfeld: associate professor of accounting, College of Business Administration, University of Texas.

William H. Starbuck: associate professor of administrative sciences and economics, Purdue University.

Benjamin H. Stevens: professor, department of regional science, University of Pennsylvania.

Gene B. Tipton: professor of economics, California State College, Los Angeles.

Paul Wells: professor of economics, University of Illinois.

Thomas H. Williams: professor of accounting, College of Business Administration, University of Texas.

Julian Wolpert: associate professor, department of regional science, University of Pennsylvania.

F. O. Woodard: professor of economics, Wichita State University.

Administrative Appointments

Nathan M. Becker: dean, School of Business Administration, Pace College.

Philip W. Cartwright: dean, College of Arts and Sciences, University of Washington.

Wayne W. Clark: chairman and professor of economics, Brigham Young University.

E. Kennedy Cobb: associate dean and professor of accounting, School of Business and Economics, California State College, Los Angeles.

Eirik G. Furubotn: chairman, department of economics, State University of New York, Binghamton and Harpur College.

John M. Gersting, John Carroll University: chairman, department of economics and business administration, Birmingham-Southern College.

Thomas P. Gilroy: program director, Center for Labor and Management, University of Iowa, February 1967.

Benton E. Gup: director of research, department of economics, University of Cincinnati.

Donald W. Hill: dean, Rollins College.

Sherwood G. Huneryager: assistant dean, College of Business Administration, University of Illinois.

Finn B. Jensen: chairman, department of economics, Lehigh University.

Leonard Kent: associate dean, College of Business Administration, University of Illinois.

George Kozmetsky: dean, College of Business Administration, University of Texas.

John M. Kuhlman: chairman, department of economics, University of Missouri.

Laurence E. Leamer: chairman, division of social sciences, State University of New York, Binghamton and Harpur College.

E. E. Liebhafsky: chairman, department of economics and finance, University of Houston.

Francis B. McCormick: acting chairman, department of agricultural economics and rural sociology, Ohio State University.

Francis McIntyre: associate dean and chairman, Graduate Faculty, College of Business Administration, University of Hawaii.

Edmund A. Mennis: senior vice president and chairman, Trust Investment Committee, Republic National Bank of Dallas.

Bruce R. Morris: acting head, department of economics, University of Massachusetts.

Thomas P. Murphy, National Aeronautics and Space Administration: director and professor of public administration, University of Missouri, Kansas City.

Gerald P. Nordquist: director, department of economics graduate program, University of Iowa.

Edwin P. Reubens: executive officer of Ph.D. program in economics, City University of New York.

Henry B. Schechter: director, Office of Economic and Market Analysis, Department of Housing and Urban Development, Washington, D.C.

William E. Schlender: chairman, department of management, College of Business Administration, University of Texas.

Larry S. Sgontz: acting director, Bureau of Business and Economic Research, University of Iowa.

Howard J. Sherman: chairman and associate professor, department of economics, University of California, Riverside.

Jack W. Skeels: acting head, department of economics, Northern Illinois University.

Mervin G. Smith: assistant dean of international agriculture, Ohio State University.

Frank M. Tamagna: chairman, department of economics, The American University.

John G. Turnbull: associate dean, College of Liberal Arts, University of Minnesota.

Jack T. Turner, University of Illinois: dean, School of Business Administration, Old Dominion College.

George W. Ulseth: chairman and professor, department of accounting, Wisconsin State University.

Ralph von Gersdorff: regional adviser on social and economic development and planning in Africa, United Nations Economic Commission for Africa, Addis Adaba, Ethiopia.

Ray O. Werner: chairman, department of economics and business administration, Colorado College.

J. Earl Williams: director of the Institute of Human Resources and professor of economics, University of Houston.

D. A. Worcester: acting chairman, department of economics, University of Washington.

Appointments

Rein Abel: lecturer in accounting, Wharton School, University of Pennsylvania.

George A. Akerlof: acting assistant professor of economics, University of California, Berkeley.

Juan B. Aponte: visiting associate professor of insurance, Wharton School, University of Pennsylvania.

Julius F. Aronofsky: visiting professor of statistics, Wharton School, University of Pennsylvania.

W. Michael Bailey: instructor in economics, The American University.

Maurice Baker, Rutgers—The State University: associate professor of agricultural economics, University of Nebraska.

Nancy S. Barrett: instructor in economics, The American University.

Charles I. Barteld: assistant professor of management sciences, School of Business Administration, The American University.

Richard C. Barth: assistant professor of economics, University of Maryland.

Edward Bell: instructor in economics, Miami University.

David A. Belsley, Dartmouth College: assistant professor of economics, Boston College.

Frank Benson, University of Pennsylvania: associate professor of statistics, University of Southampton.

A. V. Berger-Voesendorf: professor of economics, Sacred Heart University, Bridgeport, Connecticut.

John A. Bergeron: associate professor of business administration, Boston University.

Lawrence A. Boland, University of Wisconsin: assistant professor, department of economics and commerce, Simon Fraser University.

Jean Bourgeois-Pichat: visiting professor of economics, Wharton School, University of Pennsylvania.

Robert D. Bowers: assistant professor of economics, Western Reserve University.

David M. Bramhall: professor elect of economics, University of Pittsburgh, fall 1967.

Keith C. Brown: assistant professor of economics, Purdue University.

Antonio Camacho: assistant professor of economics, Purdue University.

Gordon Cameron: visiting associate professor of economics, University of Pittsburgh, spring trimester, 1966-67.

Vincent Cangelosi: associate professor, department of general business, College of Business Administration, University of Texas.

Thomas F. Carroll: professor of economics, George Washington University.

Hollis B. Chenery, Harvard University: visiting professor of economics, University of California, Berkeley, winter 1966.

N. K. Choudhry: visiting associate professor of economics, Wharton School, University of Pennsylvania, spring term.

Neil C. Churchill, Carnegie Institute of Technology: visiting associate professor, Harvard Business School, 1966-67.

Norman Coates: lecturer in industry, Wharton School, University of Pennsylvania.

Stanley H. Cohn, Research Analysis Corporation: associate professor of economics, State University of New York, Binghamton and Harpur College.

John Conlisk, Rice University: assistant professor of economics, University of Wisconsin.

Arthur J. Corazzini: instructor in economics, Dartmouth College.

Arthur J. Cordell, National Commission on Food Marketing: Joel Dean Associates, New York City.

Harley Courtney: assistant professor of accounting, College of Business Administration, University of Texas.

Robert Crockett: lecturer in finance, Wharton School, University of Pennsylvania.

John M. Culbertson, University of Wisconsin: visiting professor in economics, University of California, Berkeley, 1966-67.

Anthony J. Curley: lecturer in finance, Wharton School, University of Pennsylvania.

Edgar R. Czarnecki: assistant professor and program director for labor education, Center for Labor and Management, University of Iowa.

Clarence H. Danof: professor of economics, George Washington University.

Philip Davidowitz: visiting associate professor, Harvard Business School.

Marcia Davidson, U.S. Treasury: assistant professor of economics, Boston College.

Charles M. Dehority: assistant professor of business management, Old Dominion College.

David R. Denzler: assistant professor of industrial management, Purdue University.

Gary Devino: assistant professor of agricultural economics and marketing, Rutgers—The State University.

Walter A. Dinsdale: visiting professor of finance, College of Business Administration, University of Texas.

Bagio Di Venuti, St. Louis University: professor of economics, Catholic University, Dominican Republic.

Ryszard Domanski: visiting lecturer in regional science, Wharton School, University of Pennsylvania.

John A. Domonkos, Federal Reserve Bank, Cleveland: instructor in finance, Western Reserve University.

Richard E. Dutton: visiting associate professor of management, College of Business Administration, University of Texas.

Bruce E. Edwards: associate professor of economics, Bowling Green State University.

Kenneth K. L. Fan: computer research specialist, College of Business and Social Sciences, Utah State University.

Barry C. Field: assistant professor of economics, George Washington University.

Charles Fishbaugh: assistant professor of economics, Florida State University.

Boyd L. Fjeldsted: assistant professor of economics, University of Utah.

F. Marion Fletcher: lecturer in industry, Wharton School, University of Pennsylvania.

Carter L. Franklin: lecturer in industry, Wharton School, University of Pennsylvania.

E. Bruce Fredrikson: University of Pennsylvania, appointment at Syracuse University.

William O. Freithaler: research staff, department of economics, RAND Corporation.

R. J. Freund: associate professor of economics, Texas A & M University.

Bruno Frey: visiting lecturer in economics, Wharton School, University of Pennsylvania.

Ann Friedlaender: assistant professor of economics, Boston College.

Emma L. Fundaburk: assistant professor of economics, Bowling Green State University.

Nelda Garcia: instructor in general business, College of Business Administration, University of Texas.

John L. Gardner: instructor in economics, University of Minnesota.

Charles E. Gearing: assistant professor of industrial management, Purdue University.

William F. Glueck: assistant professor of management, College of Business Administration, University of Texas.

Andrew Gold: assistant professor of economics, Miami University.

H. Scott Gordon: professor of economics, Indiana University.

Bertram Gottlieb, AFL-CIO: visiting professor, Center for Labor and Management, University of Iowa, 1966-67.

David H. Greenberg, Massachusetts Institute of Technology: research staff, logistics department, RAND Corporation.

Stephen A. Greyser, *Harvard Business Review*: assistant professor of marketing, Harvard Business School.

Irwin Gross: lecturer in marketing, Wharton School, University of Pennsylvania.

James A. Gross, Holy Cross College: assistant professor, New York State School of Industrial and Labor Relations, Cornell University.

Herbert Grubel: associate professor of finance, Wharton School, University of Pennsylvania.

Shiv K. Gupta: visiting professor of statistics, Wharton School, University of Pennsylvania.

Charles W. Hackett, Jr: assistant professor of finance, College of Business Administration, University of Texas.

Ernest J. Hall, Jr.: associate professor of management, College of Business Administration, University of Texas.

Michael J. Hamburger, University of Pennsylvania: Federal Reserve Bank of New York.

John S. Hammond, III: instructor in managerial economics, reporting and control, Harvard Business School.

Bent Hansen: professor of economics, University of California, Berkeley.

H. O. Hartley: professor of economics, Texas A & M University.

Gerald R. Hartman: assistant professor of insurance, Wharton School, University of Pennsylvania.

Michio Hatanaka, University of Rochester: professor, Osaka University.

John M. Hatcher: assistant professor of accounting, Purdue University.

Robert H. Hayes, Stanford University: assistant professor of management, Harvard Business School.

Rolf Hayn: senior economist, Pan American Union.

Thomas Havrilesky: assistant professor of economics, University of Maryland.

Upton B. Henderson, University of Missouri: assistant professor of economics, Hiram Scott College.

J. L. Heskett, Ohio State University: associate professor of marketing and business logistics, Harvard Business School.

Sidney Hess: associate professor of statistics, Wharton School, University of Pennsylvania.

R. R. Hocking: associate professor of economics, Texas A & M University.

Thomas E. Holland: associate professor of economics, Texas A & M University.

Ann M. R. Horowitz: assistant professor of economics, Indiana University.

Donald C. Horton: associate professor of economics, Bowling Green State University.

Howard T. Hovde: distinguished visiting professor of marketing, College of Business Administration, Kent State University.

Edward Howle: lecturer in economics, Indiana University.

H. W. Huegy: visiting professor of marketing, College of Business Administration, University of Texas.

Shinichi Ichimura: visiting professor of economics, Wharton School, University of Pennsylvania.

James W. Jenkins: special instructor in economics, Brigham Young University.

Eugene Jentges: instructor in business administration, Hiram Scott College.

Gaylord A. Jentz: associate professor of general business, College of Business Administration, University of Texas

Dudley D. Johnson, University of Virginia: instructor in economics, Dartmouth College.

Howard E. Johnson: assistant professor of management, College of Business Administration, University of Texas

Mildred E. Jones: fuels economist, Tennessee Valley Authority.

Ronald D. Jones, RAND Corporation: associate professor, School of Business and Public Administration, University of Missouri, Kansas City.

Hyman Joseph: assistant professor of economics, University of Iowa.

Ralph Kaminsky: associate professor of public finance, Graduate School of Public Administration, New York University.

Edward J. Kane, Princeton: associate professor of economics, Boston College.

John W. Kendrick: professor of economics, George Washington University.

Edward R. Kittrell, University of Alberta: associate professor of economics, Northern Illinois University.

Benjamin J. Klebaner: economic adviser, Second National Bank Region of New York, New Jersey, Puerto Rico and the Virgin Islands.

Linda J. Kleiger, University of California, Los Angeles: research staff, logistics department, RAND Corporation.

Michael Klein: lecturer in economics, Indiana University.

Lennis M. Knighton: assistant professor of accounting, College of Business Administration, University of Texas.

Shou Eng Koo: associate professor of economics, College of Business Administration, University of Georgia.

Richard Kosobud, Wayne State University: visiting associate professor of economics, College of Business Administration, University of Illinois, Chicago.

John F. Kottas: lecturer in industry, Wharton School, University of Pennsylvania.

Ezra Krendel: visiting professor of statistics, Wharton School, University of Pennsylvania

Harvey A. Krow: visiting associate professor of economics, University of Pittsburgh.

Kermit D. Larson: assistant professor of accounting, College of Business Administration, University of Texas.

Prem S. Laumas, Ohio University: assistant professor of economics, Northern Illinois University.

Alton D. Law, Rutgers—The State University: assistant professor of economics, Western Maryland College.

Chung Lee: assistant professor of economics, Miami University.

James Leontiades: assistant professor of marketing, Wharton School, University of Pennsylvania.

Abba P. Lerner: professor of economics, University of California, Berkeley.

Larry G. Little: special instructor in economics, Brigham Young University.

Philip J. Lund: visiting lecturer in economics, Wharton School, University of Pennsylvania.

Craig C. Lundberg: assistant professor of industry, Wharton School, University of Pennsylvania.

Asatoshi Maeshiro: assistant professor of economics, University of Pittsburgh.

Miles Martin, University of Pennsylvania: appointment at Michigan State University.

Ferdinand J. Mathis, University of Iowa: assistant professor of economics, College of Business Administration, University of Illinois, Chicago.

S. Charles Maurice: temporary instructor in economics, College of Business Administration, University of Georgia.

Melvin L. Meer: assistant professor of economics, University of Maryland.

David I. Meiselman, U.S. Treasury Department: visiting professor of economics, University of Minnesota, spring quarter 1967.

Robert D. Mettlen: assistant professor of finance, College of Business Administration, University of Texas.

Marcel Michel: visiting lecturer in marketing, College of Business Administration, University of Texas, spring 1965-66, fall 1966-67.

Norman C. Miller: assistant professor of economics, Bowling Green State University.

Ralph E. Miller: acting assistant professor of economics, University of California, Berkeley.

J. Sayer Minas, University of Pennsylvania: appointment at University of Waterloo, Canada.

A. L. Nagar: visiting professor of economics, Wharton School, University of Pennsylvania.

Richard E. Neel, University of South Florida: director of institutional planning, Florida Technological University.

John P. Nieuwenhuysen: visiting associate professor, University of Pittsburgh, fall trimester 1966.

J. Randolph Norsworthy, University of Chicago: assistant professor of economics, College of Business Administration, University of Illinois, Chicago.

John H. Norton, University of Pennsylvania: appointment at George Washington University.

Mamoru Ogata: assistant professor of economics, Brigham Young University.

Alper Y. Orhon, Vanderbilt University: instructor in economics, Dartmouth College.

Jerry H. Padgett: assistant professor of economics, College of Business Administration, University of Georgia.

John B. Parr, assistant professor of regional science, Wharton School, University of Pennsylvania.

Charles R. Perry: lecturer in industry, Wharton School, University of Pennsylvania.

Robert F. Pethia: assistant professor of management, College of Business Administration, University of Texas.

Maurice Pfannestiel: assistant professor of economics, Wichita State University.

Edmund S. Phelps: professor of economics, Wharton School, University of Pennsylvania.

Clynn Phillips: assistant professor of economics, University of Wyoming.

George B. Pidot, Jr., Harvard University: assistant professor of economics, Dartmouth College.

Felix Pollaczek: visiting professor of statistics, Wharton School, University of Pennsylvania.

Edward C. Prescott: lecturer in economics, Wharton School, University of Pennsylvania.

Wilfred Prest: visiting professor, University of Pittsburgh, fall trimester 1966.

Charlotte Price: visiting assistant professor of economics, Vassar College.

Arthur M. Rade: financial analyst, research department, Shields & Company.

J. N. K. Rao: associate professor of economics, Texas A & M University.

Robert H. Rasche: assistant professor of economics, Wharton School, University of Pennsylvania.

Albert Rees, University of Chicago: professor of economics and public affairs, Princeton University.

Raymond L. Richman: professor of economics, University of Pittsburgh.

Myles E. Robinson, National Coal Association: professor of transportation and marketing, School of Business Administration, The American University.

C. Stevenson Rowley, Saint Cloud College: assistant professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

David S. Salsburg: assistant professor of statistics, Wharton School, University of Pennsylvania.

Phillip J. Scaletta, Jr.: associate professor of business law, Purdue University.

Stanley S. Schor, University of Pennsylvania: appointment at Temple University.

Harold M. Seeberger, St. Louis University: assigned to Pontificia Universidad Católica del Ecuador.

David Seymour: lecturer in regional science, Wharton School, University of Pennsylvania.

G. L. S. Shackle: visiting professor, University of Pittsburgh, winter trimester 1966-67.

James F. Shepherd: assistant professor of economics, Purdue University.

Donald R. Sherk, U.S. Military Academy: assistant professor of economics, Boston College.

Philip H. Siegel: assistant professor of economics, Auburn College.

Tony E. Smith: lecturer in regional science, Wharton School, University of Pennsylvania.

Daina Smits: instructor in economics, University of Minnesota.

Robert A. Solo: professor of economics and management, Graduate School of Business Administration, Michigan State University.

Frank Stafford: study director, Survey Research Center, University of Michigan.

Henry B. Steele: associate professor of economics, University of Houston.

Frank I. Stern: assistant director, Marketing Information System, New York Central System.

W. W. Stevens: visiting assistant professor of economic education, Purdue University.

Hans S. Stoll: assistant professor of finance, Wharton School, University of Pennsylvania.

Theo Suranyi-Unger, Goettingen University: professor of economics, Northern Illinois University.

Ben B. Sutton: professor of finance, University of Houston.

Akira Takayama: visiting associate professor of economics, Purdue University.

Paul J. Taubman: associate professor of economics, Wharton School, University of Pennsylvania.

Benjamin J. Taylor: assistant professor of economics, Arizona State University.

Azriel A. Teller, Johns Hopkins University: assistant professor of economics, College of Business Administration, University of Illinois, Chicago.

Rollin G. Thomas: visiting professor of economics, Bowling Green State University.

Douglas J. Tigert: assistant professor of industrial management, Purdue University.

Arthur Treadway: instructor in economics, Northwestern University.

David G. Tuerck, University of Virginia: assistant professor of economics, College of Business Administration, University of Illinois, Chicago.

Tze H. Tung: assistant professor of regional science, Wharton School, University of Pennsylvania.

Franklin D. Van Buer, University of Nigeria: assistant professor of economics, Northern Illinois University.

Terence J. Wales: lecturer in economics, Wharton School, University of Pennsylvania.

Franklin V. Walker: professor of political economy, Graduate School of Public Affairs, State University of New York, Albany.

Richard Wallace, Florida State University: assistant professor of economics, University of Missouri.

Brendan Walsh, University of Massachusetts: appointment at Tufts University.

Bernard Warner: visiting professor of statistics, Wharton School, University of Pennsylvania.

Samuel Webb: assistant professor of economics, Wichita State University.

John E. Weinrich, University of Alberta: visiting professor of economic and business enterprise, State University of New York, Binghamton and Harpur College.

Louis T. Wells, Jr.: assistant professor, Harvard Business School.

William F. Whitbeck: assistant professor of marketing, Old Dominion College.

Melvin I. White, Brooklyn College: deputy assistant secretary (for tax policy), U.S Treasury, Washington, D.C.

Jeremy L. Wiesen: assistant professor of business law, Wharton School, University of Pennsylvania.

C. Glyn Williams, Indiana University: assistant professor of economics, Boston College.

John J. Willingham: visiting associate professor of accounting, College of Business Administration, University of Texas.

Sidney G. Winter, Jr., University of California, Berkeley: research staff, economics department, RAND Corporation.

Rufus Wixon: professor of accounting, Wharton School, University of Pennsylvania.

Richard C. Yates: assistant professor of economics, Arizona State University.

Elinor B. Yudin: lecturer in economics, New York University.

Leaves for Special Appointments

F. Gerard Adams, University of Pennsylvania: consultant on questions of forecasting for U.S. State Department, OECD, Paris, 1966-67.

Sabbah Al Haj, Bowling Green State University: visiting lecturer, American University, Beirut, 1966-68.

Jim G. Ashburne, University of Texas: professor of accounting, University of Hawaii, 1966-67.

Vladimir N. Bandera, Boston College: Fulbright professor of economics, Pavia University, Italy, 1966-67.

Raul M. Branco, University of Texas: on assignment with United Nations, 1966-67.

John S. Chipman, University of Minnesota: visiting professor, Harvard University, 1966-67.

Geoffrey P. E. Clarkson, Massachusetts Institute of Technology: visiting professor, London School of Economics and Political Science, 1966-67.

Bernard L. Codner, California State College, Los Angeles: consultant in economics, University of Paraíba, Brazil, and professional staff member on the RITA project, 1966-67.

Herbert S. Denenberg, University of Pennsylvania: associate director of staff, Wisconsin Legislative Council, 1966-67.

H. Peter Gray, Wayne State University: visiting professor of economics, Thammasat University, Bangkok, 1966-67.

Peter Gregory, University of Minnesota: International Labor Organization, Switzerland, 1966-67.

Marshall Howard, University of Massachusetts: visiting professor, University of California, Berkeley, 1966-67.

Pamela Kacser, The American University: visiting lecturer in economics, Columbia University, 1966-67.

Dennis C. Mueller, Simon Fraser University: staff, Brookings Institution, 1966-68.

J. A. Nordin, Kansas State University: visiting professor of economics, University of Wisconsin, second semester 1966-67.

Kenneth W. Olm, University of Texas: visiting professor, University of New Mexico, 1966-67.

Jonas Prager, New York University: assistant professor, Bar-Ilan University and Bank of Israel, 1966-67.

Lawrence L. Schkade, University of Texas: visiting professor, Monterrey Technological Institute, 1966-67.

John Y. D. Tse, Purdue University: Lucas visiting professor, University of Birmingham, England.

Lewis Wagner, University of Iowa: program adviser in economic development, Ford Foundation program in Middle East and Africa.

Richard J. Ward, Department of State AID: chief, planning division, Bureau for Near East, South Asia, AID, fall 1965.

Wilfred H. Watson, University of Texas: visiting professor, University of Hawaii, 1966-67.

Arthur Welsh, University of Iowa: staff economist, Joint Council on Economic Education, New York.

Resignations

James E. Adams, University of Georgia, June 1966.

E. Norman Bailey, University of Texas.

Richard M. Duvall, University of Texas.

Henry A. Einhorn, City University of New York.

Wayne R. Evenson, Purdue University.

John E. Fleming, California State College, Los Angeles, June 1966.

Stanley L. Friedlander, City University of New York.

Alfred E. Hofflander, University of Texas.

Harry L. Johnson, University of Texas.

David Laidler, University of California, Berkeley, summer 1966.

Eugene W. Lambert, University of Texas.

Barbara W. Newell, Purdue University.

Betty Orr, University of Texas.

James P. Quirk, Purdue University.

John M. Scheidell, University of Georgia, June 1966.

Robert E. Seiler, University of Texas.

Robert M. Taylor, University of Texas.

K. J. Walraven, University of Texas.

Robert B. Zevin, University of California, Berkeley, June 1966.

Miscellaneous

Charles A. Bliss, faculty chairman of the Program for Management Development at Harvard Business School is first incumbent of the Royal Little Professorship, established in March 1966 in honor of Royal Little, New England industralist and founder of Textron Inc.

Ernest Bloch, professor of finance, School of Commerce, New York University: designated the Charles William Gerstenberg Professor of Finance for 1966-67.

Howard Raiffa, professor of economics and business at Harvard Business School is first incumbent of the Frank Plumpton Ramsey Professorship, established in August 1966 in honor of [illegible] British scholar.